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AMERICAN POLITICAL SCIENCE REVIEW

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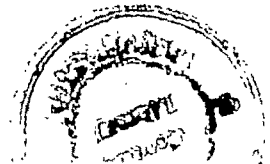
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In Memoriam

Harvey C. Mansfield, Sr.

Managing Editor
American Political Science Review
1956-1965

Died 4 May 1988

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ARTICLES

CONSTITUENCY CANDIDATES VERSUS PARTIES IN JAPANESE VOTING BEHAVIOR

BRADLEY M. RICHARDSON
Ohio State University

Most studies of Japanese elections have seen constituency candidates and their campaigns as the major factor in electoral mobilization. In order to explore the effects of candidate campaigns and voters' images of candidates, as well as party identifications, party images, partisan issue opinions, and past voting, I develop models portraying these effects upon the vote, both separately and together, and test the models using survey data. The results show that partisan attitudes are the dominant force in Japanese parliamentary voting. The most plausible explanation for the importance of partisan factors lies in the electorate's being exposed regularly to information about parties but only intermittently to information about candidates. The plausibility of an information-based explanation of the importance of robust partisan attitudes in Japan suggests in turn that comparative differences in voting may reflect systemic variations in information processes.

Little is known about the relative influence of parties versus local constituency candidates in parliamentary elections. One reason constituency variables have been omitted in research on parliamentary election voting is that elections frequently seem to be dominated more by national parties than by local forces. Centralized party organizations and party list voting would both seem to encourage voter concern for partisan elements of choice. But parties and campaigns are more decentralized in some countries, and electoral laws may encourage a focus on constituency candidates. In these kinds of settings, constituency candidates and their campaigns may actually be very important in parliamentary election voting.

This is a study of partisan versus constituency candidate influences on voting behavior in a Japanese parliamentary election. Japan is a setting where local

constituency campaigns and candidates are often seen as unusually significant. Using survey data on voting in a Japanese general election, I demonstrate that local candidate imagery, exposure to constituency campaigns, and the effects of vote solicitation within local social networks have a significant impact on the decisions of many Japanese voters. However, my findings also show conclusively that party identification, party images, and party-focused habitual voting are the main determinants of the vote. As a corollary to the importance of party variables, people have more developed images of parties than they do of candidates. The more consistent availability of information on parties and consequently greater cognitive buttressing of partisan attitudes appears to be a major factor underwriting the importance of party variables in Japan. The findings of this research qualify earlier ideas that constituency candidates and

their campaigns dominate Japanese parliamentary voting.

Constituency Candidates in Voting Research

Early political psychology was more interested in the partisan dimensions of voting choice than in the effects of constituency campaigns or attitudes toward local candidates. U.S. political psychology from the beginning focused on presidential contests and saw attitudes toward presidential candidates, opinions on partisan issues, and party identification as the main influences on voting decisions (Campbell et al. 1960). Research within the psychological tradition on voting overseas also focused on attitudes toward parties and partisan issues (Baker, Dalton, and Hildebrandt 1981; Valen and Katz 1964). Even where constituency dimensions of the vote could potentially be important, as in Britain's single-member districts, parties and partisan issues have been emphasized in most voting studies (Butler and Stokes 1969; Crewe 1976; Crewe, Sarlvik, and Alt 1977).

In the 1960s U.S. research began to examine the effects on voting of attitudes toward constituency candidates in congressional elections. But knowledge of candidates proved to be low, according to their findings, and party identification still appeared to be the main determinant of choice (Stokes and Miller 1962). More recently, research based on new measurement strategies has disconfirmed the idea that U.S. congressional elections are solely party contests. Instead, evidence shows that local candidate evaluations are a prominent determinant of choice (Mann and Wolfinger 1980). As a result, attitudes toward candidates and other variables that tap exposure to constituency campaigns have come to occupy a more prominent position in congressional voting behavior analysis (Asher 1983). However, up until now, this research has

had very little impact in research abroad. Just how things work out in the presence of different combinations of party-versus-constituency influences on voting in different systems remains to be ascertained.¹

Constituency Forces in Japanese Elections

Institutions and Setting

Nowhere is the constituency candidate dimension of electoral choice so plausible an influence on voting as in Japan. Electoral institutions, typical campaign practices, and political culture all converge to make candidates and their local campaigns potentially very important factors in electoral behavior. Multimember districts predominate in elections for both houses of the national parliament; they are the exclusive pattern in House of Representatives elections and an important mode in House of Councilors local contests. Parliamentary seats are assigned on the basis of individual candidates' district vote totals in most cases. (Only in the House of Councilors' national district, which selects 100 of the upper house's 252 members, is proportional representation the rule.)² More than one candidate from the same party can consequently be elected to the Japanese system if a party's district vote pool is large enough. Responding to these circumstances, both of Japan's leading parties, the Liberal Democrats and the Japan Socialists, run multiple candidates in many constituencies. Multiple candidates are virtually the norm among Liberal Democrats; they are less common in the case of the Socialists. Under this set of circumstances, candidates from the same party compete with each other as well as with candidates from the other parties.

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Candidate-focused Campaigns

Under a combination of multimember districts and intraparty competition, constituency election campaigns are often run mainly by the candidates themselves. The parties play a larger role in election strategy in the parties of the Middle and Left; but even there candidates account for much of the initiative in the constituency portion of the campaign. A number of factors besides the institutional setting and intraparty competition actually encourage candidate-focused campaigns and may have underwritten the historical development of Japan's unique electoral arrangements. These include the small areas, dense populations, redundant and accommodating social structures and supportive cultural norms that define the context of campaigns in many of Japan's parliamentary election districts (Richardson 1974). The presence of local elites who manipulate the vote, community norms and group processes that encourage conformity, cultural values that emphasize personal linkages over abstract ties, and social exchange practices dictating that gifts often be given for the favor of a vote all permit, and even encourage, candidate-focused politics in Japan. The social setting thus combines with the effects of electoral laws to support an unusual degree of emphasis on candidates and their personal connections.

Campaign strategies in Japan reflect the social and institutional parameters just described. Candidates, particularly those within the conservative movement, tend to emphasize their own records and positions more than party affiliations, according to Dore's (1955) analysis of formally registered election statements. Candidates also make strong appeals to local interests and endeavor in both the short and the long run to mobilize the vote in their own favor, for instance, by organizing support groups or gaining the endorsement of community influentials. Anticipating the

"favor" of a vote by providing small services, token gifts, or even candidate-sponsored trips is also not uncommon (Curtis 1971; Richardson 1967). Candidate-oriented campaign techniques and appeals are reputedly most developed in rural districts and in small cities. But even in larger urban constituencies candidate-organized support bases and "traditional" campaigns are found more often than might be expected (Richardson 1979). Obviously candidates are a major feature of Japanese elections, which could make attitudes toward candidates and responses to their campaigns a major component of voting choice.

Constituency and Candidate in Japanese Research

Findings from several sources indicate that constituency considerations influence the vote in national elections in Japan. According to studies dating back to 1958, roughly two out of every five voters in House of Representatives elections normally cite constituency candidate as a more important factor in their vote than party. The focus on candidate has stayed remarkably stable over the past three decades (Table 1). (A concern for party has gained somewhat in this period, accompanied by a decline in *don't know* responses.)

Other evidence supports the theme that constituency candidacies and campaigns are important in Japanese national elections. Contacts with candidates in the form of postal greeting cards, membership in candidate support organizations, attending speeches by the candidate, and requests to vote for a particular candidate are widespread in Japanese parliamentary elections according to the findings of a Komei Senkyo Remmei-Akarui Senkyo Suishin Kyokai series of voting studies extending back over two decades (Richardson 1979). Candidate evaluations, measured by thermometer questions, are also

Table 1. Emphasis On Candidate or Party in Voting Choices in Japanese General Elections (%)

Year	Candidate	Party	Don't Know
1958	45	32	23
1960	43	33	24
1963	51	31	18
1967	47	37	16
1969	33	49	18
1972	38	48	14
1976	40	46	14
1979	46	41	13
1980	38	49	13
1983	42	47	11
1986	39	49	12

Note: Samples were nationwide; all sample numbers were over 2,000.

Source: 1958-67, *Sosenkyo no jittai*, a private election survey report series of the Komei Senkyo Remmei; 1969-76, *Shugiin giti sosenkyo no jittai*, a similar series of reports from the remmei; 1976-86, the Akarui Senkyo Suishin Kyokai, a successor organization to the remmei.

very important to the vote and overshadow the effects of vote requests and contacts with the candidate, according to some research (Watanuki 1986a, 1986b). Each element of constituency candidate-oriented voting—a response to candidates' personal qualities and reactions to efforts to mobilize the vote—constitute dimensions to be considered seriously in Japanese voting research.

Partisan Components of Japanese Electoral Choice

Evidence from various sources suggests that a "constituency campaign" model of voting behavior might go a long way toward explaining electoral choice in Japan. However, other evidence from research in Japan provides support for an alternative paradigm of the vote, wherein parties and partisan issue attitudes are the main determinants of choice.

Political Parties

Japan has had national political parties since the 1890s. Contemporary parties date back to prewar antecedents in the case of the Japan Socialists and the Liberal Democrats. Both parties experienced several mutations before assuming their contemporary form in 1955, and the postwar Socialists are a far more significant party than their prewar predecessors. Still, these two parties have been around for quite awhile in one or another form. The same is true for the Japanese Communist party, which came into being in 1945 and had been a consistent electoral alternative for three decades by 1976. Only the Democratic Socialists, who left the Japan Socialist party in 1959, the Clean Government party, which first appeared as a contestant in national elections in 1967, and the New Liberal club formed in 1976, were comparative newcomers to the political scene at the time of the 1976 study. My earlier research in Japan addressed the effects of discontinuities on partisanship (Richardson 1975), but with the passage of time the parties' continuity is a more prominent factor.

In addition to their relative longevity, Japan's national political parties are highly visible in daily political life. Even if the parties are not as active in many constituencies as the local candidates, all parties still conduct visible national campaigns; including, since 1969, closely regulated television presentations of party platforms. Election manifestos are announced and publicized, and party leaders make campaign speeches throughout the country. Interparty conflict in the Diet and party policy pronouncements between elections also receive heavy media coverage. The durability of interpartisan differences on foreign policy, as well as long-term divisions on some domestic issues imbued with class rhetoric by Japan's permanent opposition parties, gives special emphasis and clarity to party

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differences (Richardson and Flanagan 1984). Because of their permanence and high visibility, the Japanese electorate has ample opportunity to learn about parties and party differences on issues.

Party Variables in Voting Research

Parties and issues are clearly important to Japanese voting behavior according to many studies of electoral choice (e.g., Akuto 1971; Kohei 1972, 1974). Indeed, while party identification has been identified as weaker and less stable in Japan than in other countries (Flanagan and McDonald 1979; Miyake 1970, 1985, 1986a; Miyake, Kinoshita, and Aiba 1967), identifications have still been shown to play a major role in the electoral choices of Tokyo's voters (White 1984) and in the decisions of persons in districts where each party runs only one candidate (Rochon 1981). Other partisan components of a psychological model of voting choice, such as partisan issue attitudes, partisan ideology, and party imagery, are also clearly factors in electoral behavior in Japan (Kabashima 1986; Kohei 1979; Miyake 1986b).

Social Change and the Prominence of Party Variables

Recent rapid social change and advances by the electronic media in Japan appear to have enhanced the importance of a partisan components model of the vote. Four mutually reinforcing processes are involved in these changes: cities are growing in population, industries have invaded the countryside, television is widely diffused, and the past three decades have seen an explosion in the numbers of persons obtaining high school and college educations.

Urban constituencies have always been more the arena where interparty, national issue-focused competition had the greatest electoral resonance (Richardson

1966, 1974). The social networks and local elite influence that underwrite candidate-run campaigns in the countryside and small towns and cities are to some degree attenuated in the more anonymous society of the large cities. Simultaneously, broadly conceived partisan issues have been the main concern of persons in the cities, whose lives and occupations are cosmopolitan in so many ways. Urban districts are also the locale where the written mass communications media were first developed and diffused, a fact that combined with other forces, such as the national political orientation of Japanese labor unions, give cities a particularly cosmopolitan and partisan political life.

As the cities have expanded, so have urban styles of political behavior. Much recent urban growth has taken the direct form of expansion of the cities and their suburbs into former farming areas. This process, which was especially prominent in the late 1960s, led to a drastic change in the social composition and political color of many constituencies (Flanagan 1980; Richardson 1977). Elsewhere urban social and political forces have penetrated the countryside via regional industrial development. In addition, the spread of television has supplemented the already widespread diffusion of the highly centralized written mass media, so that the countryside is itself subject to more partisan information than was the case earlier. Motivating and responding to all of these events was a dramatic restructuring of educational opportunities and accomplishment, with the result that virtually all persons now finish high school, and college attendance has nearly reached North American levels—a big advance over the earlier postwar era (Richardson and Flanagan 1984). Each of these factors—urban residence, industrial occupations, mass media diffusion, and expansion of education—is associated with a preference for party over candidate (Akarui Senkyo Suishin Kyokai 1986).

Social change thus plausibly underwrites the growth over time in the importance of party shown in Table 1.

Party and Candidate in Political Communications Processes

The urbanization of Japanese life and the accompanying widespread diffusion of the written and electronic media are processes that have been underway for a very long time but accelerated beginning in the late 1960s. They take on special importance for this analysis when differences in the information processes concerning parties and candidates and their implications for attitude development are fully appreciated. Parties are the substance of the electronic and written communications media, which are widely diffused in Japan and deal with politics on a regular basis. In contrast, candidates are mainly the subject of informal, verbal communications, which for most people are intermittent. These differences in the regularity with which people are exposed to political information are extremely important.

The mass media are highly centralized in Japan and their news coverage deals mainly with national and cosmopolitan information. Most Japanese read only national newspapers, whose coverage of local affairs is especially scanty relative to their treatment of national and international news. Television coverage of politics and elections is likewise centrally administered and focuses on national and international politicians and events. Some of the public, somewhere around one-fourth of all newspaper readers, do read regional newspapers where the news fare is somewhat more local in orientation (Meideia Risachi Senta 1986). Local pages that carry news for each prefecture (and which are different for each prefecture) are also found in all of the centralized dailies. But unlike the United States, neither prefectural newspapers nor local

pages in the national dailies provide much coverage of individual candidates' campaigns. This reflects either the small size of the relevant areas of candidate activity and therefore their restricted newsworthiness to broader audiences or the fact that local candidacies are for some other reason not seen as interesting or legitimate topics.³ In addition to the virtual absence of media coverage of individual candidates' campaigns, little or no attention is paid by the media to the activities of rank-and-file members of the Diet. What party leaders, cabinet members and the parties' policy spokesmen in the Diet do is reported. But the inner workings of Diet committees and the role of ordinary members of the Diet in processing of legislation or in other legislative functions go unnoticed.

Recent research in social cognition suggests that the differences between the communications processes concerning parties and candidates in Japan can be of profound importance to political attitude development and impact. According to some social psychological thinking, information processing complements people's attitudes by providing the rationales and internal argumentation that support and underwrite an affective orientation (Fiske and Taylor 1984). Both the regularity and amount of information that can be used to justify preferences—or in some instances change them—are important to these cognitive processes. Parties are a regular fare in the mass media and information about them is thematic. Repetition is believed to enhance the salience and vividness of particular information, which makes it easier to process. Information abundance permits more complete internal argumentation. Greater vividness and more thorough internal argumentation enhance the strength of particular attitudes and facilitate their recall from memory (Nisbet and Ross 1980). If individual Japanese find particular parties appealing, for whatever reason, the abundant amount of

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information available to them about parties permits more complete internal argumentation in support of the relevant attitudes. A deeper interpretive base in turn buttresses favorable attitudes and encourages their recall in situations where they are relevant to behavior.

In contrast with the abundance and regularity of messages about parties, information about candidates is only received on an intermittent basis by most people. Even though many candidates maintain permanent organizations that have regular meetings, these efforts involve only one out of every eight or so electors at best. For the remainder of the electorate, candidates are normally visible only at election time. Under these conditions of irregular and intermittent exposure, it is unlikely that many people could develop sufficient information to support robust attitudes even where they normally favor specific candidates at election time.

People thus learn about parties and candidates in different ways in Japan. Related differences in information processing suggest that partisan attitudes will be more robust than candidate orientations. In addition to the effects of the described differences in communications channels, the contexts in which people hear about parties and candidates vary in other ways that likely enhance the depth of attitudes toward parties relative to the richness of candidate impressions. Parties are active in most levels of elections in Japan. Independent candidacies are common only in rural and small city local elections. This means that people hear about parties each time there is an election, a process that makes it possible for partisanship and party imagery to deepen. Specific candidates run in only one kind of election at a time, with the consequence that their exposure is much more limited than in the case for parties, a factor that complements the effects of the overall intermittency of candidate ex-

posure within particular kinds of elections.

Research Design and Data Sources

Strong arguments can be made for both partisan and constituency candidate models of the vote in Japan. The differential effects of attitudes toward partisan political objects and reactions to constituency candidates and campaigns is obviously a central question for Japanese voting behavior research. It is not a question that has been addressed satisfactorily by earlier research in Japan. Most scholarly studies of Japanese political behavior have focused on partisan variables, leaving to the newspapers and other polling efforts investigation of the effects of constituency campaigns; appreciation of the role of constituency factors in voting has been superficial as a result.⁴

The issue of partisan versus constituency components of the vote is addressed here in several stages. I begin by showing the simple distributions of voter contacts with campaigns and attitudes toward constituency and partisan political phenomena. The different clusters of examples of campaign exposure and attitudes toward candidates and parties are in turn grouped into a series of indexes that summarize their candidate or partisan direction. I test constituency campaign and partisan components models of voting, first separately, and then jointly. While undertaking these tests, I add various supplementary paths to the two models in order to portray as credibly as possible, within the scope of this inquiry, the relationships between different kinds of attitudes toward constituency candidates and parties and the effects of particular campaign experiences on attitudes. Finally, the implications of the findings are considered.

The data base for this analysis is the 1976 Japan General Election Study (JABISS survey).⁵ The 1976 study con-

tained extensive question batteries relating to constituency campaigns and their effects. These included information on local campaign contacts, political communications within informal social networks, group memberships and political preferences, and evaluations or images of candidates. Partisan elements of the election contest, party identification, party imagery, and issue opinions were also extensively covered.

Constituency Dimensions of Partisan Choice

Candidate Recognition

Voters in Japan's small, multimember House of Representatives districts usually have an opportunity to learn something about, or to have direct contact with, constituency candidates or be exposed to their efforts to mobilize the vote. This should be more true of constituencies in rural areas, provincial cities, and the older, small business portions of large cities than of the more impersonal middle class and working class residential neighborhoods in Japan's larger cities. But even where social structures are less congenial, candidates work hard to gain at least minimal recognition, using such techniques as widespread mailings of postcards between elections and extensive calling of candidates' names from loudspeakers on buses or automobiles that roam district streets and roads just before elections.

Name recognition itself should be very high in view of the extent to which the campaign is believed to center on the efforts of individual candidates. On the other hand, both the sheer number of candidates in many of Japan's multimember constituencies and the tendency of many candidates to concentrate their efforts in certain areas within specific constituencies dictate that people may not know the

names of all district candidates. Our own strategy in the face of these complexities was to determine simply if people were able to remember the candidate for whom they voted. (The question was open-ended, so as to minimize inflation of responses through cuing.) Most of the voting electorate—actually four out of every five persons who turned out—identified valid candidates in their answers to this question. This finding indicates that candidates are at least minimally salient to most people in Japan.

Candidate Images

Evidence of other kinds indicated that the electorate was well aware of, or had extensive contacts with, the candidates and their constituency campaigns. Respondents to the 1976 survey were asked if a particular candidate from their district held opinions "close to their own views," was a "clean and fresh" candidate, would help their area, would help their occupation, or had "deep ties with the area" (Table 2). Nearly half of those persons who could name the candidate for whom they voted actually indicated at least one concrete image of a particular candidate. Seeing candidates as having "deep local ties" was the strongest theme, thus confirming the parochial focus of many voters. Moderate proportions of the voting public saw candidates as representing their occupation or having similar views. Surprisingly, very few people saw candidates as actually representing the local area, even though such a focus has been emphasized in descriptions of candidates' ties with their election bases (Curtis 1971; Richardson 1967). Since seeing a candidate as having deep local ties was much more common than feeling that a candidate would represent the local area, obligation and local loyalties may be stronger elements of the vote than expectations of performance,

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**Table 2. Candidate and Constituency Variables,
1976 Japanese General Election**

Candidate and Constituency Variables	Percentage of Total ^a
Personal Requests for Voting Support	
Received request(s) from	
Family	5
Relatives	7
Coworkers	13
Neighbors	12
Friends	15
Received at least one request	36
Contact with Candidate	
Received postcard	44
Attended speech	18
Member of Koenkai	18
Had at least one contact	53
Candidate Image	
Felt candidate	
Had done something for local area	6
Had done something for respondent	1
Was "clean" and "fresh"	13
Had similar views to respondent	22
Had deep local ties	28
Would represent respondent's occupation	16
Had at least one favorable image	49

^aPercentages are based on the total number of persons who could name the candidate for whom they had voted. N was 823 in all cases.

Source: 1976 JABISS survey (see n. 5).

Campaign Experiences

Electioneering by individual candidates is a major portion of parliamentary election activity in Japan. Just over 50% of the respondents who voted and knew which candidate they had supported reported having contacts with candidates' campaigns via attendance at speeches, receiving post cards, or joining a support organization (Table 2). Receiving post card greetings, reflecting an activity engaged in heavily by incumbent members of parliament at New Year's and at *chugen* in the midsummer, was the most common link between voter and

candidate, outweighing speech attendance and support association membership by roughly three to one.

Interpersonal Requests

Japanese elections consist of both an open, formal campaign and an informal, less open effort in which candidates and their lieutenants personally contact voters and ask for their support, either directly or via social networks that include local elites and other persons (Flanagan 1971; Richardson 1979). Quite a few persons among those who had voted in the 1976 election had been contacted in this way.

Just over one third of the persons who remembered voting for a particular candidate had received a request to vote for one of the nominees from a family member, friend, relative, neighbor, or co-worker.

All in all, nearly 60% of those who had voted in 1976 and knew the name of the candidate for whom they voted had at least one link with the candidate, such as a favorable image of the nominee or some kind of contact with the candidate, his organization, or the social networks mobilized in his favor. The frequency of activities and attitudes centering on particular candidates and reflecting the importance of constituency campaigns suggests a special prominence for candidates in Japan that fits popular and scholarly pictures of Japanese constituency politics.

Partisan Elements of Choice

Parties and partisan issues are also important to voters in Japan. Nevertheless, partisanship is itself fairly complicated and takes the form of a series of different but still partially overlapping phenomena. Some people in Japan are stable party identifiers, some are "image" partisans and some are habitual voters for the same party. Many voters manifest combinations of the three different partisan orientations.

Party Identification

Party identification is well known as the core concept of U.S. and comparative political psychology and is normally seen as an effective party tie. In the JABISS survey, respondents were asked if they "supported" one of the six significant parties running candidates in the 1976 election. This question was as close a replica of the traditional U.S. party identification item as is possible and was asked in the same form on both panel waves. Four out

of five of the JABISS respondents who had voted indicated a party identification on the first wave.⁶ This figure increased substantially in the second panel wave interviews. However, only 59% of the persons who voted were actually stable identifiers over the two panel waves. The remaining 41% wavered between an identification and an apolitical stance of *no party* or *don't know*—the dominant alternative pattern—actually switched parties between interviews, or were consistently apolitical (see also Flanagan and McDonald 1979; Richardson 1986). This figure for stable self-conscious partisanship seemed very low, given that the period between the two panel waves was only one month. As various studies of party identification in Japan (Flanagan and McDonald 1979; Miyake 1970, 1985, 1986a; Miyake, Kinoshita, and Aiba 1967; Richardson 1975) have noted, lower levels of stability are normal for Japanese party loyalties relative to those in other countries, regardless of whether the intervening time intervals are brief or long. This finding has always led to a discounting of the importance of affective party identifications for the Japanese voter.

Habitual Voting

My earlier research (Richardson 1986) showed that consistent voting for the same party in the past plays an important role in electoral choice in later elections in Japan. For a significant portion of the electorate—at least one voter out of five—habitual voting, measured by reports of voting for the same party in the last three national and local elections prior to the 1976 contest, is the main evidence of a stable party tie. For these people, a voting habit takes the place of a stable party identification, since their answers to the party identification item indicated an apolitical stance, wavering between independence and a partisan commitment, or shifting preferences be-

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tween the first and second wave. For most other persons habitual voting combines with stable party identifications as evidence of continuous partisan attachments reinforced by conforming behavior. When both groups of partisans—stable identifiers in the two waves and persons voting consistently in past elections—are accounted for, 85% of the Japanese vot-

ing public in 1976 indicated some kind of stable party attachment (Table 3).

Party Images

Party images, a third area of the electorate's response to parties, is fairly widespread in the Japanese case (see also Kohei 1979; Miyake 1986b). Party images are

Table 3. Partisan Factors, 1976 Japanese General Election

Partisan Factors	Percentage of Total ^a
Party Identification and Partisan Vote Consistency	
Identified with a party in first wave	82
Identified with same party both waves	59
Voted consistently in recent elections ^b	82
Voted consistently and/or stable identifier	85
Party Image	
Felt party was	
Best in candidates	64
Best in leaders	54
Best in policy	52
Best in occupation representation	61
Best in local area representation	52
Best to end corruption	47
Best to stop inflation	44
Best to govern	59
Had at least one favorable image	85
Closest Party	
Felt some party closest to own position on	
Emperor's role in government	36
U.S.-Japanese security treaty	43
China treaty	40
Korean relations	29
Northern territories	43
Defense forces	43
Business tyranny	46
Social welfare	51
Right to strike	44
Growth vs. inflation	35
Money politics	47
Political contributions	42
Had at least one nomination of a "closest party"	73

^aPercentages are based on the total number of persons who could name the candidate for whom they had voted. *N* = 823.

^bVoting consistency in past three elections includes persons who voted in three out of three elections for same party, persons who voted in two out of three elections for same party while voting three times or those who voted only twice but for same party.

understood to be people's broad, general cognitive pictures of the individual political parties and have been seen by some scholars as major, independent forces in voting behavior in addition to party identifications (Baker, Dalton, and Hildebrandt 1981; Butler and Stokes 1969; Matthews and Prothro 1966). In the JABISS survey images of parties were sought through closed questions regarding which parties were seen as having the best candidates, leaders, or policies; which parties were most capable of representing local or occupational interests; and which parties could best stop inflation, end corruption, or rule Japan. (Each of the eight dimensions of party images covered by the JABISS survey reflected themes in people's party images identified in earlier Japanese studies).

Eighty-five percent of the electorate had at least one favorable cognitive image of a political party, a figure substantially higher than that for stable party identifications (Table 3). The most commonly held image was that of a party "having the best candidates." Sixty-four percent of the voting electorate possessed a favorable image of one of the parties on that dimension. But nominations of parties "best suited to govern" and "best representing my occupation" were close runners-up.⁷ Images were also well represented in the electorate's aggregate responses to parties. Forty percent of the 1976 voting electorate were stable party identifiers, had favorable images of their preferred party, and had voted for that party regularly. An additional 19% combined favorable images with either stable identifications or habitual voting and 5% had only favorable images of a preferred party.

Partisan Issue Opinions

Partisan issue domains are recognized by many voters in Japan, although partisan issues were less salient to the elector-

ate in 1976 than the more general aspects of the parties captured in party image questions. Voters were asked which party they felt was closest to their own opinions on twelve domestic and foreign policy questions, including the emperor's role in government, security and foreign policy, and domestic policy problems such as the economy, social welfare, and political corruption (see Table 3).⁸ Seventy-three percent of the voting public felt that a particular party was closest to their position on at least one of the twelve issues. Half of the voting electorate could nominate a party as closest to their own positions on the single most salient issue, which was the adequacy of social welfare programs. Still, *don't know* answers were systematically higher in the case of replies to the more specifically focused issue proximity questions than on more broadly framed party image items, and fewer voters felt parties were close to their own ideas on multiple issues than had multiple images of the parties. However, once the deficiencies in party identifications and a little flabbiness in feelings of issue proximity are noted, the political parties have obviously cumulatively penetrated the electorate on a substantial variety of dimensions in Japan.

Comparing Party and Candidate Images

One of the significant patterns uncovered by this research was the presence of very large differences in the raw frequencies of multiple images of candidate and party respectively (Table 4). Seventy-eight percent of the Japanese voting electorate in 1976 reported two or more favorable images of some party, and another 7% possessed at least one image. Only 15% of those who voted lacked any favorable image of a party. In contrast, 51% of those who voted had no favorable image of a candidate, and an additional

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25% had just one image. Only 24% had two or more favorable candidate images, in contrast with the 78% level for party images! Cognitive pictures of parties are obviously much more richly developed than those of candidates, reflecting in large part the differences in communications processes discussed above. People know and like a lot more about parties than they do candidates, in part because they see, read, and hear so much more about them on a regular basis. Parties in Japan aren't as remote and disheartening as has been thought in the past, a fact likely relevant to how people vote.

Are There Candidate Voting Traditions?

Given the importance of candidates and their campaigns in Japan, is it possible that there are candidate voting traditions that resemble habitual party voting, or candidate loyalties akin to party identification, even though candidate images are more shallow than mental pictures of parties? Can a more latent form of candidate orientation than images be uncovered? Apparently the answer is *no*. Information from questions about people's self-perceptions of their past voting behavior suggest that a candidate voting habit comparable to partisan voting habits and identifications was simply not present. Respondents were asked before the election if they "normally voted for the same candidate." Surprisingly, in view of the election mobilization literatures' emphasis on candidate campaigning and the high incumbency rates in relevant Diet elections, only 24% of the persons who subsequently voted said they had voted regularly for the same person in the past. In contrast, after the election, when a similar question was asked about partisan voting, three-quarters of the respondents who had voted in 1976 said they had "voted for the same party over time."

Table 4. Multiple Images of
Candidate and Party

Images	Percentage of Total ^a
Candidate	
Two or more	24
One	25
None	51
Party	
Two or more	78
One	7
None	15

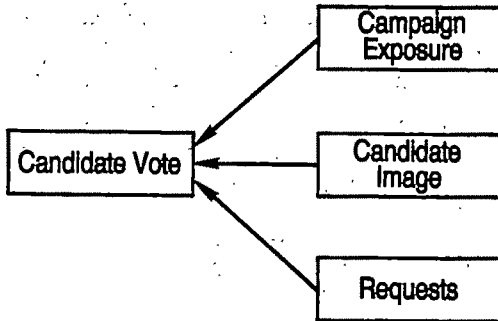
^aFigures for candidate and party images are simple response frequencies for all persons who voted for a specific candidate. $N = 823$.

Although, the two figures are not totally comparable because of differences in question format and timing, the differences between the frequencies of persons who reported having voted consistently for a candidate and those who said they always voted for the same party are so great that far more voters appear to be bound to parties than to candidates.

Models of the Vote

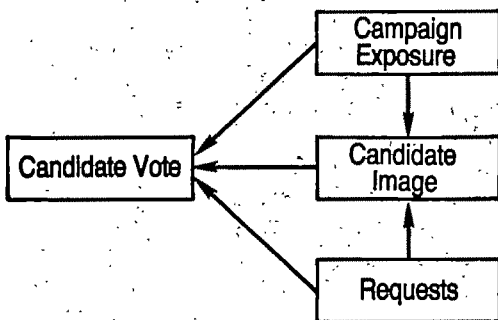
A logical first step toward evaluating the effects of partisan and constituency components of Japanese voting behavior is to see how responses to constituency candidates and their campaigns sort out as determinants of the vote. A model of the effects of the campaign and images of the candidate is proposed in Figure 1. Three aspects of the voting public's exposure to local campaigns have been identified: candidate images, direct contacts with candidates' campaigns, and receipt of requests for a supportive vote. Each of these dimensions of voter response to constituency candidates and campaigns are postulated as directly affecting voting choices. In addition to their direct effects on voting, campaign activities and requests themselves probably help people

Figure 1. A Simple Constituency Model



form candidate images in certain cases. The effects of these experiences are especially important since so little media attention is paid to constituency candidates. The causal diagram shown in Figure 2 contains additional paths so that the indirect effects of campaign experiences and requests on images of candidates can be taken into account. Looking at things this way should help round out our understanding of how people learn about constituency candidates. (Obviously, incumbents and candidates who ran in earlier elections also may benefit from the effects of prior campaigns and efforts to maintain stable followings between elections. Some of these processes are tapped by our questions about post

Figure 2. An Elaborated Constituency Model



cards and membership in support associations, but some important interelection informal communications processes lie beyond the scope of our survey).

A simple causal model of the partisan forces influencing the 1976 parliamentary vote is shown in Figure 3. Stable party identifications, consistent past election choices, favorable party images and feelings of issue proximity—the four ways in which Japanese respond to political parties—are each hypothesized to affect the vote. In addition to their direct effects on the vote, it is likely that the various attitudes represented as independent variables are casually linked to each other. But deciding on the precise causal role of some variables, especially the personal voting habits represented in consistent past voting, was not easy. The behaviors summarized in the voting habit index preditional impact on the latter. But questions about contemporary party identifications presumably tapped *durable* partisan attachments that earlier guided the voting choices. Much of the same logic applies to party images. For these reasons, party images and identifications are seen as causing past voting behavior in the expanded model of partisan components of the vote (Figure 4), even though in certain individual cases causality over time was a more complicated matter.

The models developed thus far have dealt with attitudes toward parties and responses to local candidates and their campaigns as if these were completely separate entities. Nothing is more unlikely. Party loyalties and favorable appraisals of local candidates are undoubtedly intertwined in people's thinking in many ways. They work together to influence how people vote, as is outlined by the third main variant of our causal models (Figure 5). In addition, attitudes toward local candidates undoubtedly affect feelings toward political parties and vice versa. Japanese parties historically began as coalitions of local as well as national

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Figure 3. A Simple Partisan Model

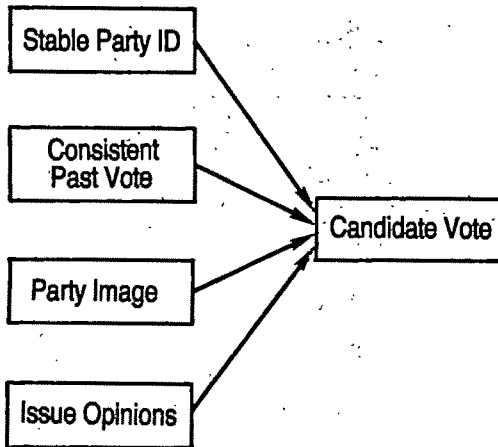
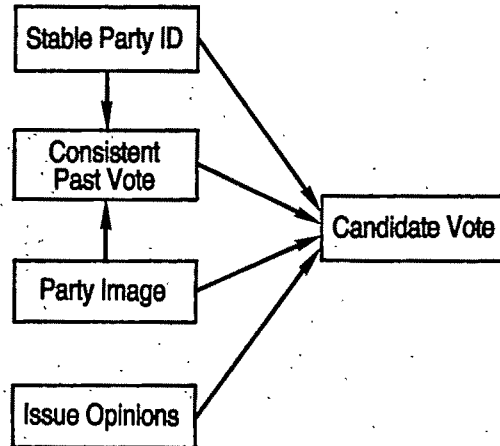


Figure 4. An Elaborated Partisan Model



politicians and interests, and remain intimately linked as informal organizations to their constituency bases today. It is interesting in this context to see the extent to which constituency and candidate considerations enter people's normal frame of reference when thinking about parties. Analysis of answers to different items in our closed question battery on party images showed that feelings that specific

parties "have good candidates"—a constituency candidate referent in House of Representatives elections and House of Councilors' local district contests—was highly correlated with the remaining dimensions of images, which had an exclusively partisan focus. In other words, the two dimensions are tightly integrated. There was no significant segment of the public that thought of the parties exclu-

Figure 5. A Comprehensive Partisan-Constituency Candidate Model

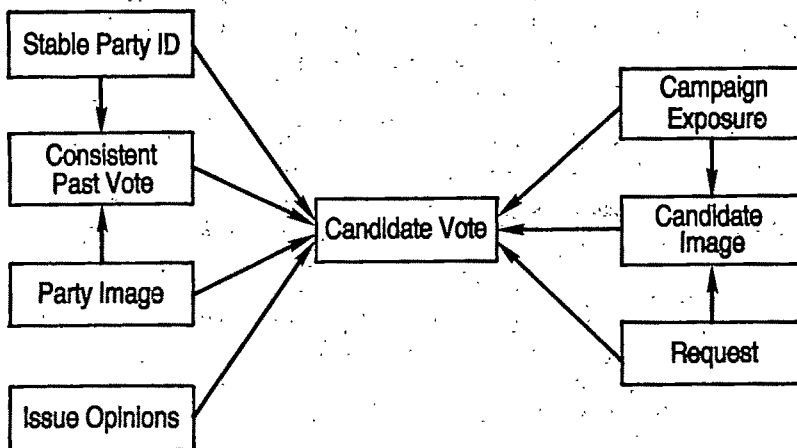
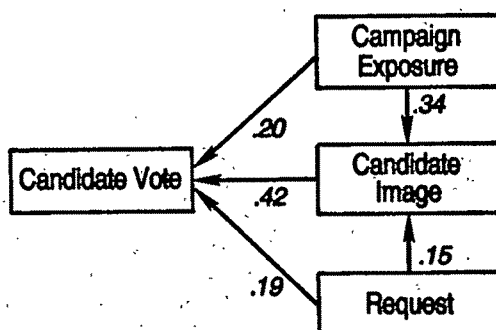


Figure 6. Constituency Components of Voting Choice



Summary Constituency Components	Paths		Total
	Direct	Indirect	
Candidate images	.42	—	.42
Campaign exposure	.20	.14	.34
Interpersonal requests	.19	.06	.25

Note: All numbers are standardized beta coefficients. $N = 822$; $R^2 = .37$.

sively in constituency candidate terms. We are led to believe that cognitive images of the parties in Japan are mainly focused on the parties' general characteristics and that constituency candidates enter into the picture mainly as integrated components of a broadly focused imagery.

Various additional analyses were undertaken to see what direct effects constituency candidate images might have on attitudes toward parties and how partisan attitudes might affect feelings toward candidates. Clearly, persons who favor the candidates of a particular party supported the same party in most instances. But only a cross-time analysis of these relationships could really determine directionality of the effects on partisan attitudes of feelings toward candidates and vice versa. For this reason, there are no arrows for the relationships between candidates and partisan attitudes in the causal model proposed here.⁹

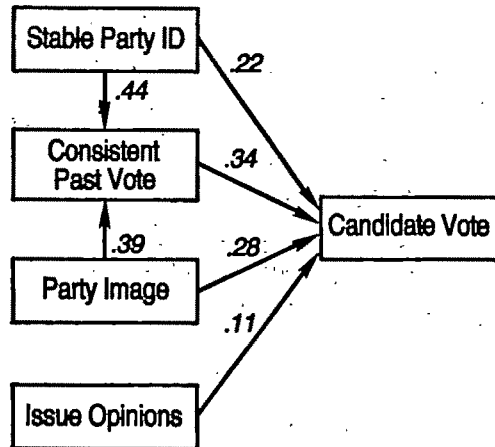
Testing Models of the Vote in Japan

In Figure 6, the results of a multiple regression of the vote for candidate on the three core components of voter reactions to local constituency campaigns are shown. Two things stand out in this simple analysis of constituency and candidate influences on the vote. First, the three aspects of voter experience with constituency candidates and their campaigns do a pretty good job in explaining candidate choice. Altogether, 37% of the variance in the candidate vote is accounted for by the three core variables. Constituency variables are important in Japan.

The second notable feature of the regression findings in Figure 6 concerns the effects of the different aspects of voters' campaign experiences and attitudes. Candidate imagery is more important than either campaign experiences or informal requests for one's vote. Never-

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Figure 7. Partisan Components of the Vote Decision



Summary Partisan Components	Paths		Total
	Direct	Indirect	
Party images	.28	.13	.41
Stable party identification	.22	.15	.37
Consistent past voting	.34	—	.34
Issue opinions	.11	—	.11

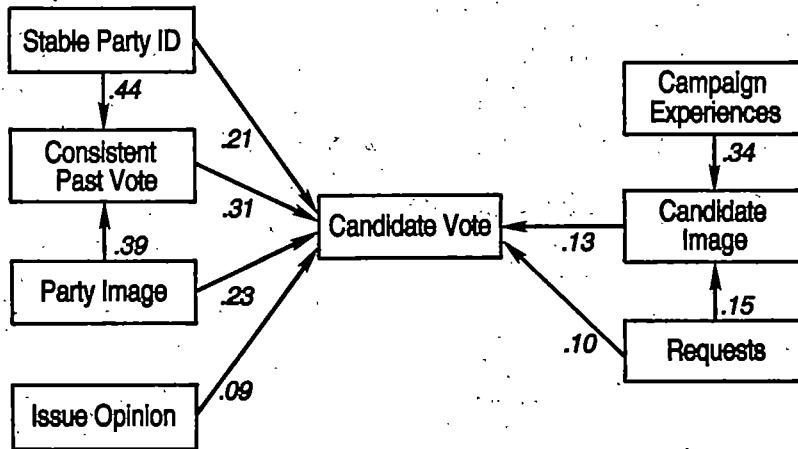
Note: All numbers are standardized beta coefficients. $N = 822$; $R^2 = .67$.

theless, the direct consequences of efforts to mobilize the electorate via requests and campaign contacts were still significant. Moreover, having received a request and being exposed to the campaign—receiving a post card, attending a speech, or joining a support association—had substantial indirect effects on candidate images, as predicted in the elaborated model outlined in Figure 2, and their total effects on voting choices were enhanced as a result.

The analysis in Figure 6 shows that stronger indirect relationships exist between campaign experiences and candidate images than between requests and candidate images. Prior attraction to particular candidates certainly motivates some people to join support organizations or attend speeches, which means that the

attitude linkages have already been made in the past, even though we don't know their direction. The time spans implicit in campaign contacts are also longer than those involved in requests, and the specific forms of exposure are themselves repeatable. Campaign experiences consequently imply greater possibilities for reinforcement than requests, since the requests we asked about were only those occurring during the 1976 campaign. Finally, as I have shown elsewhere (Richardson 1966), requests in urban areas are often simply a recommendation of a particular candidate accompanied by no commentary, while in rural areas requests are usually "rationalized" by favorable comments about the candidate. This means that requests are lacking in sub-

Figure 8. Candidate, Constituency, and Party in Voting Choice



Summary	Paths		Total
	Direct	Indirect	
Party images	.23	.12	.35
Stable party identification	.21	.13	.34
Consistent past voting	.31	—	.31
Candidate images	.13	—	.13
Interpersonal requests	.10	.02	.12
Issue opinions	.09	—	.09
Campaign experiences	—	.04	.04

Note: All numbers are standardized beta coefficients. $N = 823$; $R^2 = .70$.

stance for many people, which could explain why they have such a restricted effect on candidate images.

Estimates of the partisan components model developed above indicated that the strongest direct influence on voting was a consistent voting record in the past (Figure 7). Party images and stable party identifications were also significant to the vote, contributing nearly as much to voting choices directly as did consistent past voting. When the indirect effects of party imagery are added to their direct impact, party images are the strongest single determinant of the vote. Stable party identifications are second in impor-

tance. This analysis demonstrates that other party variables, namely party images and consistent voting in the past (which is best seen as evidence of a voting habit), join party identifications as major factors in the 1976 vote choice in Japan. Clearly, each separate dimension of people's responses to parties has an important contribution, contrary to the expectations of at least traditional political psychology that party identifications would be the dominant influence. Meanwhile, feelings of issue proximity toward the parties, although far from insignificant, had a much weaker impact than the three alternative indications of partisan preferences

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(Figure 7). Importantly, the partisan components model explained 67% of the variance in the 1976 candidate vote, or nearly twice the contribution attributable to candidate images and constituency campaigns.

The final test of the contribution of voters' reactions to consistency candidate and partisan aspects of elections, consisting of an examination of their combined effects on the vote, is summarized in Figure 8. Despite the great emphasis in the extant Japanese election literature on the importance of constituency campaigns, variables representing partisan attitudes and habits—party identification, party images, and voting habits in past elections—were the principal factors influencing the 1976 Japanese vote.

In addition to the overall domination of voting choices by party attitudes and past voting experiences, the contribution of candidate images and campaigns was somewhat lower in the combined models tests than in the case of the constituency model reported above. Most people favored candidates from their own preferred party, or were exposed mainly to the campaigns of candidates from their preferred party (see also Richardson 1979). Candidate choices were thus made under the "umbrella" of a partisan identification or some kind of partisan tie, a finding that severely qualifies the long-standing evidence from simpler analyses (Table 1) that candidate is a major, independent factor in Japanese electoral behavior nearly equal in importance to party. (The path representing the contribution of campaign experiences to voting is dropped in Figure 8, as the relevant relationship failed to meet tests of significance.)

Interpretation and Implications

This analysis has shown that a partisan components model of the vote explains

over twice as much variance in Japanese parliamentary voting as a constituency candidate model. The omnipresent local constituency campaigns in Japan have relatively subdued effects on voting relative to partisan components of choice. Much of the attention to voting in Japan on the part of U.S. scholars (e.g., Flanagan and McDonald 1979; Richardson 1975) has been distracted by the weak party identification phenomenon to the point of ignoring the importance of parties to Japanese voting. The weakness of party identification made sense in light of folklore about the importance of local candidate organizations in Japan (Richardson 1975). But this led us to ignore two extremely important aspects of the Japanese electorate's reactions to parties: party images and consistent voting habits.

Party images—broad cognitive pictures that people develop of political parties—have been suggested as an important form of electorate response to parties in earlier writings on the United States (Matthews and Prothro 1966; Trilling 1976; Wattenberg 1981) and Europe (Baker, Dalton, and Hildebrandt 1981; Butler and Stokes 1969; Wattenberg, 1982). But the status of party images has never been completely clear. To some scholars they are mainly correlates of party identifications; to others they are seen as independently important. In Japan both versions of party images are found. Many identifiers have complementary images of their favored parties. In addition, quite a few people—actually 13% of the voting public—maintain favorable images of parties without being firm identifiers. It is this group within the electorate that gives images a special importance in Japan. The presence of both groups of image partisans enhances the role of cognitive partisanship in a culture where affective partisanship is relatively weak and unstable.

I have already reported the presence of a consistent voting habit in Japan—a sec-

ond form of electoral response to parties different from party identification (Richardson 1986). My analysis here shows more clearly how independently important the effects of this habit actually are relative to those of stable party identifications. This analysis has shown that Japanese voters react to political parties in three partially distinct and partially overlapping ways. Once the variation in the electorate's reactions to parties in Japan is recognized, the importance of parties in Japanese electoral behavior becomes clear.

Partisan components of parliamentary voting have been shown far to outweigh the effects of constituency campaigns and candidates in Japan. Even in a country where constituency factors should be unusually important, attitudes toward parties and party-oriented voting habits were the dominant factors in national election voting. The findings of this research, including evidence of dramatically greater cognitive depth for party than for candidate images, severely qualify much earlier interpretation of Japanese voting. The research also suggests that the communications channels through which information about parties and candidates is acquired merit further study, as do related processes of social change and media diffusion. Parliamentary voting in Japan appears to be substantially different from congressional voting in the United States, the main available comparative referent at present. Constituency candidate evaluations are very important in the U.S. case, whereas parliamentary voting in Japan is dominated more by attitudes toward national parties. Quite a bit of these differences between countries may reflect significant cross-national variations in political communications.

Methodological Appendix

Index Construction. Summary indexes were created for candidate images, re-

quests, constituency campaign exposure, party images, issue opinions, habitual voting, and stable party identification. Each candidate index summarizes the specific candidate direction of a plurality or more of each individual's replies in the relevant content areas. Both the candidate indexes and the candidate vote choice variables were coded arbitrarily as "first LDP candidate," "second LDP candidate," and so on for each election district. (The Communist party, Clean Government party, New Liberals, and Democratic Socialist party did not run plural candidates and were represented by only one candidate code.) Using this procedure and the resulting two-digit code, the responses favoring a particular candidate from among the individual election district alternatives could be analyzed among voters in specific constituencies throughout Japan without reference to district.

Party images, habitual voting, and partisan issue opinions were represented by summary indexes that were coded simply as summary, plurality preferences for one of the six main parties active in the 1976 election—the Liberal Democrats, New Liberals, Democratic Socialists, Clean Government party, Japan Socialists, and Japan Communists. All other responses were treated as missing data so that single-dimensional scales of partisanship suitable for analysis of behavior in multi-party systems could be constructed. Since vote for specific candidates was the dependent variable throughout the analysis in this article, the party codes were arbitrarily recoded to the midpoint of the range of the multiple candidate codes; for example, since there were four codes for Liberal Democratic candidates (11, 12, 13, 14), preferences for the Liberal Democratic on indicators for party variables were coded to 12.5. Party identification was coded simply as the party preferred in both waves, with all other combinations being treated as missing data.

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Missing Data. Missing data constitute a major problem in Japanese political behavior research, sometimes amounting to as much as one-third or one-half of responses. Exclusion of missing data from the analysis in these circumstances results in a distorted view of the Japanese public as a whole, and also presents some statistical problems. Since this analysis addressed aspects of the behavior of all Japanese voters, missing data were coded to the means for each variable and treated as valid codes. This practice tended to depress inter-item correlations and the standardized betas used in the path analyses. For comparison and validation, regressions were run in which missing data were deleted using both pairwise and listwise procedures. Tests employing the candidate and constituency variables using this alternative approach were unacceptable because of extremely small numbers. Those using the party variables produced distorted findings due to heightened multicollinearity (which has not been a problem in earlier runs using the mean for missing data). Importantly, using alternative approaches to missing data had no effect on the relative weights of party versus constituency variables, so the general findings of this analysis were unaffected.

Explained Variance. A problem of multiple regression analysis is a tendency for strong variables to dominate other variables to a degree out of proportion to their actual contribution. In order to be sure that our results were valid, stepwise regressions were run in different ways so as to compensate for the effects of the stronger partisan components variables, and the sequential explanations of variance in the different combinations were compared. In one run, the candidate and campaign variables were introduced first; subsequent addition of the partisan components variables added 33% to explained variance. When the partisan components were entered first, inclusion of candidate

and campaign variables added only a further 3% in explained variance. Clearly, the partisan variables provide the most definitive explanation of parliamentary voting in Japan.

Measurement Validity. The answers to the closed questions employed to identify party and candidate images in the JABISS survey are clearly reasonably close to the decisions involved in the vote itself, as is witnessed by the high proportions of variance in the vote attributable to the two image variables. To some students of voting, the relationship between images and the vote is believed to be tautological or nearly so (Heath, Jowell, and Curtice 1985), and, according to their views, alternative variables should be employed. Several answers can be made to this potential criticism. The party images used in this analysis are linked to party identifications measured at two points in time, to recall of consistent voting in the past, or to both of these presumably stable propensities in over 90% of their occurrences. (I estimated recall of consistent past voting itself to be valid in typically over 90% of its occurrences [Richardson 1986]). Because of these linkages of party images to other long-term variables, people's party images can be assumed generally to have intermediate or long-term status. For this reason, our use of party images is not broadly vulnerable to the criticism that its relationship to the vote is tautological. In addition, as was noted earlier, multiple party images outnumbered multiple candidate images at a ratio of three to one. A difference of this magnitude should itself indicate a substantial degree of validity for both measures. Finally, psychological variables such as party and candidate images are necessary components of Japanese political behavior analysis. They cannot be replaced by social class or other "sociological" variables, which are sometimes claimed to be causally more distant from

the vote, given the relative insignificance in Japanese political life of societal cleavages (Flanagan and Richardson 1977; Watanuki 1988).

Notes

The 1976 Japanese General Election Survey upon which this report is based was supported by grants from the National Science Foundation and the Hoso Bunka Kikō. Special appreciation is due James Ludwig of the Polimetrix Laboratory of Ohio State University for assistance in data management and Aiji Tanaka and Gregory Tilton for help with data analysis.

1. Recent research on candidate visibility and related issues in France in Converse and Pierce (1986) and the Japanese Election Study (JES) series of projects (Watanuki 1986a, 1986b) are exceptions.

2. One hundred councillors are elected in a single nationwide constituency, fifty each in elections at three-year intervals. Proportional representation based on the d'Hondt rule replaced the single non-transferable vote there in 1983. The remaining councillors are elected in local districts in Japan's 47 prefectures.

3. Japanese election constituencies are themselves small. Candidate's campaigns are also often geographically focused within each election district. A given prefecture has typically between three and four constituencies and within each constituency several individual candidate support "territories" may exist.

Although the national dailies and regional papers do carry long series of articles on local campaigns, these rarely go beyond brief references to each candidate and his organizational activities.

4. An excellent and very thorough study of both partisan and constituency components of voting in Japan based on a local sample is Miyake, Kinoshita, and Aiba 1967, which deals with behavior in several different levels of elections in the city of Uji in Kyoto prefecture. Two recent landmark studies also report important findings on the role of partisan or candidate components of the vote in great depth (Watanuki et al. 1986a, 1986b) but do not directly broach the combination and interaction of national and local forces as completely as is done here. Other studies of the contest between candidate and partisan influences have been conducted by two semi-private groups—the Komei Senkyo Remmei and its successor organ, the Akarui Senkyo Suishan Kyokai—and affiliated local groups. While providing valuable trend information, these studies have dealt with constituency campaigns and related phenomena by means of relatively superficial inventories of self-

reported motivations and campaign experiences, such as those reported in Table 1.

5. The acronym is based on the first initials of the principal investigators, Joji Watanuki, myself, Ichiro Miyake, Scott Flanagan, and Shinsaku Kohel. The JABISS survey utilized a nationwide sample that was stratified with regard to urban-rural residence. Selection of individual respondents was made randomly. Interviews were conducted in two panel waves; the analysis reported here was based on the panel portion of the respondents, whose total number was 1,235.

6. The frequency for first-wave party identifications is calculated using the segment of the total sample who reported voting for a specific candidate as the base, as are all other statistics reported in this article. Since people who knew the name of the candidate they had supported were more politically aware than persons who voted but could not identify the candidate of their choice, marginal frequencies for party identification and other variables are slightly higher than those reported elsewhere (e.g., Flanagan and McDonald 1979) for the total voting public or the entire JABISS sample.

7. Since party images were solicited only in the postelection wave, their stability is unknown. However, images were linked closely with one or the other of the two stable party responses; a summary index of respondent dispositions on all eight image dimensions showed correlations of .687 with habitual voting and .657 with stable party identification.

8. The form of the partisan issue proximity questions employed in the JABISS survey could have invited projection of partisan attachments onto issue opinions. But there were actually substantially lower frequencies of feelings that there were parties close to one's position than of willingness to place oneself on position issue scales, which leads us to believe that projection was not as much a problem as might be anticipated. In addition, there are some justifications for using a proximity-type question in the Japanese case. Alternative approaches to measurement of issue opinions and issue-party linkages, such as using issue position questions, are less workable in Japan than in simpler party systems. Issue differences between Japan's Middle and Left parties are very complicated, due to different alignments on different issues and the absence of measurable differences between some parties on some issues; and these complexities favor use of a simple measure of partisan issue linkages, like the proximity question.

9. Estimation of a nonrecursive model of these relationships was not undertaken due to the difficulty of identifying a sufficient number of exogenous variables in the context of Japan's complicated and asymmetrical "social matrix" patterns of partisan support (Watanuki 1988).

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CONTRASTING RATIONAL AND PSYCHOLOGICAL ANALYSES OF POLITICAL CHOICE

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We contrast the rational theory of choice in the form of expected utility theory with descriptive psychological analysis in the form of prospect theory, using problems involving the choice between political candidates and public referendum issues. The results showed that the assumptions underlying the classical theory of risky choice are systematically violated in the manner predicted by prospect theory. In particular, our respondents exhibited risk aversion in the domain of gains, risk seeking in the domain of losses, and a greater sensitivity to losses than to gains. This is consistent with the advantage of the incumbent under normal conditions and the potential advantage of the challenger in bad times. The results further show how a shift in the reference point could lead to reversals of preferences in the evaluation of political and economic options, contrary to the assumption of invariance. Finally, we contrast the normative and descriptive analyses of uncertainty in choice and address the rationality of voting.

The assumption of individual rationality plays a central role in the social sciences, especially in economics and political science. Indeed, it is commonly assumed that most if not all economic and political agents obey the maxims of consistency and coherence leading to the maximization of utility. This notion has been captured by several models that constitute the rational theory of choice including the expected utility model for decision making under risk, the riskless theory of choice among commodity bundles, and the Bayesian theory for the updating of belief. These models employ different assumptions about the nature of the options and the information available to the decision maker, but they all adopt the principles of coherence and invariance that underlie the prevailing notion of rationality.

The rational theory of choice has been used to prescribe action as well as to describe the behavior of consumers, entrepreneurs, voters, and politicians. The use of the rational theory as a descriptive model has been defended on the grounds that people are generally effective in pursuing their goals, that the axioms underlying the theory are intuitively compelling, and that evolution and competition favor rational individuals over less rational ones. The objections to the rationality assumption were primarily psychological. The human animal, it has been argued, is often controlled by emotions and desires that do not fit the model of calculating rationality. More recent objections to the maximization doctrine have been cognitive rather than motivational. Following the seminal work of Herbert Simon (1955, 1978) and the emergence of

cognitive psychology, it has become evident that human rationality is bounded by limitations on memory and computational capabilities. Furthermore, the experimental analysis of inference and choice has revealed that the cognitive machinery underlying human judgment and decision making is often inconsistent with the maxims of rationality. These observations have led to the development of a descriptive analysis of judgment and choice that departs from the rational theory in many significant respects (see, e.g., Abelson and Levi 1985; Dawes 1988; Kahneman, Slovic, and Tversky 1982; Tversky and Kahneman 1986).

We contrast the rational theory of choice with a descriptive psychological analysis, using a series of questions involving political candidates and public referenda. These problems are used to illustrate the differences between rational and descriptive theories of choice and to test their predictions. Some of the questions probed our respondents' views about familiar political issues, such as the Equal Rights Amendment and the prevalence of crime in black neighborhoods compared to white neighborhoods. In other cases involving the test of general hypotheses, such as risk aversion, we introduced hypothetical problems in order to achieve experimental control and eliminate the influence of irrelevant factors. The use of hypothetical problems raises obvious questions regarding the generality and the applicability of the finding. Nevertheless, we believe that the use of carefully worded questions can address key issues regarding people's values and beliefs so long as respondents take the questions seriously and have no particular reason to disguise or misrepresent their true preferences. Under these conditions hypothetical questions can be used to compare alternative theories of political choice that cannot be readily tested using available survey and voting data. Our results, of course, do not pro-

vide definitive conclusions about political decision making, but they may shed light on the formation of political judgment and stimulate new hypotheses that can be tested in national election surveys in the years to come.

We focus on expected utility theory, which is the major normative theory of decision making under risk (von Neumann and Morgenstern 1947; Raiffa 1968; Savage 1954). This model is contrasted with prospect theory, a descriptive analysis developed by Kahneman and Tversky (1979, 1984). The first section deals with the role of the reference point and its impact on the choice between political candidates. In the second section we test the assumption of invariance and contrast it with a psychophysical analysis of numerical scales. The third section deals with the perception and the weighting of chance events, and the role of uncertainty in choice. The fourth section addresses the classical issue of the rationality of voting. It contrasts, again, a rational analysis based on the probability of casting a decisive vote with a less rational analysis that incorporates an element of self-deception. The implications of the present analysis are discussed in the fifth and final section.

Reference Effects, Risk Attitudes, and Loss Aversion

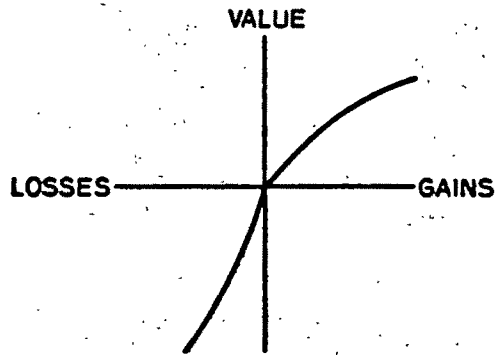
The standard utility function, derived from the expected utility model, has two essential characteristics. First, it is defined on wealth, or final asset position. Thus, a person with wealth W accepts an even chance to win \$1,000 or lose \$500 if the difference between the utility of $W + \$1,000$ and the utility of W (the upside) exceeds the difference between the utility of W and the utility of $W - \$500$ (the downside). Second, the utility function is concave; that is, the subjective value of an additional dollar diminishes with the total amount of money one has. The first

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assumption (asset integration) is necessitated by basic considerations of coherence. The second assumption (concavity) was introduced by Bernoulli (1954) to accommodate the common observations of risk aversion, and it has played an essential role in economics. A person is risk-averse if he or she prefers a sure outcome over a risky prospect that has an equal or greater expected value. For example, most people prefer \$100 for sure over an even chance to win \$200 or nothing. Risk aversion is implied by the concavity of the utility scale because the utility of $2x$ is less than twice the utility of x .

Although risk aversion is quite common, particularly for prospects with positive outcomes, risk seeking is also prevalent, particularly for prospects with negative outcomes. For example, most people find a sure loss of \$100 more aversive than an even chance to lose \$200 or nothing. To explain the combination of risk aversion and risk seeking, prospect theory replaces the traditional concave utility function for wealth by an S-shaped function for changes of wealth. In this theory, therefore, the carriers of values are positive or negative changes (i.e., gains and losses) defined relative to a neutral reference point. Furthermore, the value function is assumed to be concave above the reference point and convex below it, giving rise to risk aversion in the domain of gains and risk seeking in the domain of losses. As in the classical theory, it is assumed that the difference between \$100 and \$200 is subjectively larger than the (numerically equivalent) difference between \$1,100 and \$1,200. Unlike the classical theory, however, it is assumed that the difference between a loss of \$100 and a loss of \$200 is subjectively larger than the numerically equivalent difference between a loss of \$1,100 and a loss of \$1,200. Thus, the value function of prospect theory is steepest at the origin and it gets shallower as one moves away from the reference point in either direction. An

Figure 1. A Hypothetical Value Function



important property of the value function—called loss aversion—is that the downside is considerably steeper than the upside; that is, losses loom larger than the corresponding gains. A typical value function with these characteristics is given in Figure 1.

Attitudes towards Risk

Expected utility theory and prospect theory yield different predictions. The classical theory predicts risk aversion independent of the reference point, whereas prospect theory predicts risk aversion in the domain of gains and risk seeking in the domain of losses (except for small probabilities). Furthermore, prospect theory implies that shifts in the reference point induced by the framing of the problem will have predictable effects on people's risk preferences. These phenomena are illustrated in the following four problems, each involving a choice between alternative political prospects.

The respondents to these and other problems reported in this article were undergraduates at Stanford University or at the University of California at Berkeley. The problems were presented in a questionnaire in a classroom setting. Each problem involved a simple choice between two candidates or positions on a

public referendum. The respondents were asked to imagine actually facing the choice described, and they were assured that the responses were anonymous and that there were no correct or incorrect answers. The number of respondents in this and all subsequent problems is denoted by N , and the percentage who chose each outcome is given in parentheses.

Problem 1 ($N = 89$)

Suppose there is a continent consisting of five nations, Alpha, Beta, Gamma, Delta, and Epsilon. The nations all have very similar systems of government and economics, are members of a continental common market, and are therefore expected to produce very similar standards of living and rates of inflation. Imagine you are a citizen of Alpha, which is about to hold its presidential election. The two presidential candidates, Brown and Green, differ from each other primarily in the policies they are known to favor and are sure to implement. These policies were studied by Alpha's two leading economists, who are of equal expertise and are impartial as to the result of the election. After studying the policies advocated by Brown and Green and the policies currently being pursued by the other four nations, each economist made a forecast. The forecast consisted of three predictions about the expected standard of living index (SLI). The SLI measures the goods and services consumed (directly or indirectly) by the average citizen yearly. It is expressed in dollars per capita so that the higher the SLI the higher the level of economic prosperity. The three projections concerned

1. the average SLI to be expected among the nations Beta, Gamma, Delta, and Epsilon
2. the SLI to be expected by following Brown's policy
3. the SLI to be expected by following Green's policy

The forecasts made by each economist are summarized in the following table:

Projected SLI in Dollars per Capita			
	Other Four Nations	Brown's Policy	Green's Policy
Economist 1	\$43,000	\$65,000	\$51,000
Economist 2	\$45,000	\$43,000	\$53,000

Suppose that as a citizen of Alpha, you were asked to cast your vote for Brown or Green. On the basis of the information provided, whom would you vote for? [Brown, 28%; Green, 72%]

A second group of respondents received the same cover story as in Problem 1, but the economists' forecasts about the other four nations were altered. The forecasts made about the candidates remained the same.

Problem 2 ($N = 96$)

Projected SLI in Dollars per Capita			
	Other Four Nations	Brown's Policy	Green's Policy
Economist 1	\$63,000	\$65,000	\$51,000
Economist 2	\$65,000	\$43,000	\$53,000

Suppose that as a citizen of Alpha, you were asked to cast your vote for Brown or Green. On the basis of the information provided, whom would you vote for? [Brown, 50%; Green, 50%]

Comparing the responses to problems 1 and 2 shows that the choice between Brown and Green was influenced by the projected SLI in other countries. This effect can be explained in terms of the value function of prospect theory. Because the two economists were said to be impartial and of equal expertise, we assume that respondents gave equal weight to their projections. Hence, the actuarial expected value of Brown's policy (\$54,000) is about the same as that of Green's policy (\$52,000). However, Brown is riskier than Green in the sense that the outcomes projected for Brown have greater spread than those projected for Green. Therefore, Brown would profit from risk seeking and Green from risk aversion. According to prospect theory, an individual's attitude towards risk depends on whether the outcomes are perceived as gains or losses, relative to the reference point.

In Problems 1 and 2 it seems reasonable to adopt the average SLI projected for the other nations as a point of reference,

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because all five nations were said to have comparable standards of living. The reference point then will be about \$44,000 in problem 1 and \$64,000 in problem 2. Outcomes projected for Brown and Green would, therefore, be treated as gains in the first problem and as losses in the second. As a consequence, the value function entails more risk aversion in problem 1 than in problem 2. In fact, significantly more respondents opted for the relatively risk-free Green in problem 1 (72%) than in problem 2 (50%) ($p < .005$ by chi-square). Another factor that may have contributed to the finding is a tendency for people to discount the highly discrepant projection for the risky candidate, Brown (i.e., the one made by Economist 1 in problem 1 and by Economist 2 in problem 2). Although this consideration may have played a role in the present case, the same shift in attitudes towards risk have been observed in many other problems in which this account does not apply (Tversky and Kahneman 1986).

To address whether the predictions based on the value function apply to other attributes besides money, we included in the same questionnaire one of two problems in which the rate of inflation was the outcome of the choice.

Problem 3 (N = 76)

Now imagine that several years have passed and that there is another presidential contest between two new candidates, Frank and Carl. The same two economists studied the candidates' preferred policies and made a projection. This time, however, the forecast concerned the projected rate of inflation. The forecasts made by each economist are summarized in the following table:

	Projected Rate of Inflation (%)		
	Other Four Nations	Frank's Policy	Carl's Policy
Economist 1	24	16	4
Economist 2	26	14	26

Suppose that as a citizen of Alpha, you were asked to cast your vote for Frank or Carl. On the

basis of the information provided, whom would you vote for? [Frank, 74%; Carl, 26%]

A second group of respondents received the same cover story as in problem 3, but the economists' forecasts about the other four nations were altered. The forecasts made about the candidates remained the same.

Problem 4 (N = 75)

	Projected Rate of Inflation (%)		
	Other Four Nations	Frank's Policy	Carl's Policy
Economist 1	4	16	4
Economist 2	6	14	26

Suppose that as a citizen of Alpha, you were asked to cast your vote for Frank or Carl. On the basis of the information provided, whom would you vote for? [Frank, 52%; Carl, 48%]

The analysis of problems 3 and 4 closely follows that of problems 1 and 2. The expected rate of inflation was 15% for both candidates. However, this value was below the expected continental rate of 25% in problem 3 and above the expected continental rate of 5% in problem 4. Because high inflation is undesirable, values below reference are likely to be viewed as gains, whereas values above reference are likely to be viewed as losses. Assuming that the continental rate of inflation was taken as a point of reference, the results confirmed the prediction of prospect theory that the more risky candidate (Carl) would obtain more votes in problem 4 (48%) than in problem 3 (26%) ($p < .01$ by chi-square).

Together, the responses to problems 1-4 confirm the prediction of prospect theory that people are risk-averse in the domain of gains and risk-seeking in the domain of losses, where gains and losses were defined relative to the outcomes projected for other countries. These results may shed light on the so-called incumbency-oriented voting hypothesis.

Numerous investigators have shown that the evaluation of an incumbent party is responsive to fluctuations in the national economy. In general, incumbent presidents and congressional candidates of the same party benefit at the polls from improving economic conditions whereas they suffer from deteriorating conditions (Kramer 1971). These results can be understood, in part, as a consequence of the divergent attitudes towards risks for outcomes involving gains and losses. Following Shiepsle (1972), we maintain that incumbents are usually regarded by voters as less risky than the challengers, who are often unknowns and whose policies could drastically alter the current trends, for better or for worse. If people are risk-averse for gains and risk-seeking for losses, the less risky incumbent should fare better when conditions are good than when they are bad. This analysis assumes that the reelection of the incumbent is perceived by voters as a continuation of the current trends, which is attractive when times are good. In contrast, the election of the challenger offers a political gamble that is worth taking when "four more years" of the incumbent is viewed as an unsatisfactory state.

It is important to distinguish this analysis of incumbency-oriented voting from the more common explanation that "when times are bad you throw the rascals out." In the latter account, voters are thought to regard a credible challenger as having to be better than the incumbent, who "got us into this mess to begin with." The present account, in contrast, is based on the notion that the challenger is *riskier* than the incumbent, not necessarily better overall. In problems 2 and 4, the risky candidates profit from hard times even though their expected value was no better than that of the relatively riskless candidates. Obviously, however, a challenger whose expected value is substantially below the incumbent's is unlikely to be

elected even in the presence of substantial risk seeking.

In light of this discussion, it is interesting to share an unsolicited response given by one of our participants, who received problem 4 in the winter of 1981. This respondent penciled in *Carter* over *Frank*, the less risky candidate, and *Reagan* over *Carl*, the riskier candidate. Recall that in this problem the outcomes were less desirable than the reference point. Evidently, our respondent—who voted for *Carl*—believed that the erstwhile incumbent *Carter* would have guaranteed the continuation of unacceptable economic conditions, while the erstwhile challenger *Reagan*, with his risky "new" theories, might have made matters twice as bad as they were or might have been able to restore conditions to a satisfactory level. Because economic and global conditions were widely regarded as unacceptable in 1980, the convexity of the value function for losses may have contributed to the election of a risky presidential prospect, namely *Reagan*.

Loss Aversion

A significant feature of the value function is that losses loom larger than gains. For example, the displeasure associated with losing a sum of money is generally greater than the pleasure associated with winning the same amount. This property, called *loss aversion*, is depicted in Figure 1 by the steeper slope for outcomes below the reference point than for those above.

An important consequence of loss aversion is a preference for the status quo over alternatives with the same expected value. For example, most people are reluctant to accept a bet that offers equal odds of winning and losing x number of dollars. This reluctance is consistent with loss aversion, which implies that the pain associated with the loss would exceed the pleasure associated with the gain, or $v(x) <$

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$-v(-x)$. This observation, however, is also consistent with the concavity of the utility function, which implies that the status quo (i.e., the prospect yielding one's current level of wealth with certainty) is preferred to any risky prospect with the same expected value. These accounts can be discriminated from each other because in utility theory the greater impact of losses than of gains is tied to the presence of risk. In the present analysis, however, loss aversion also applies to riskless choice. Consider the following example: Let $x = (x_i, x_u)$ and $y = (y_i, y_u)$ denote two economic policies that produce inflation rates of x_i and y_i and unemployment rates of x_u and y_u . Suppose $x_i > y_i$ but $x_u < y_u$; that is, y produces a lower rate of inflation than x but at the price of a higher rate of unemployment. If people evaluate such policies as positive or negative changes relative to a neutral multiattribute reference point and if the (multiattribute) value function exhibits loss aversion, people will exhibit a reluctance to trade; that is, if at position x (the status quo) people are indifferent between x and y , then at position y they would not be willing to switch to x (Kahneman and Tversky 1984). We test this prediction in the following pair of problems.

Problem 5 ($N = 91$)

Imagine there were a presidential contest between two candidates, Frank and Carl. Frank wishes to keep the level of inflation and unemployment at its current level. The rate of inflation is currently at 42%, and the rate of unemployment is currently at 15%. Carl proposes a policy that would decrease the rate of inflation by 19% while increasing the rate of unemployment by 7%. Suppose that as a citizen of Alpha, you were asked to cast your vote for either Frank or Carl. Please indicate your vote. [Frank, 65%; Carl, 35%]

Problem 6 ($N = 89$)

Imagine there were a presidential contest between two candidates, Frank and Carl. Carl wishes to keep the rate of inflation and un-

employment at its current level. The rate of inflation is currently at 23%, and the rate of unemployment is currently at 22%. Frank proposes a policy that would increase the rate of inflation by 19% while decreasing the rate of unemployment by 7%. Suppose that as a citizen of Alpha you were asked to cast your vote for either Frank or Carl. Please indicate your vote. [Frank, 39%; Carl, 61%]

It is easy to see that problems 5 and 6 offer the same choice between Frank's policy (42%, 15%) and Carl's policy (23%, 22%). The problems differ only in the location of the status quo, which coincides with Frank's policy in problem 5 and with Carl's policy in problem 6. As implied by the notion of multiattribute loss aversion, the majority choice in both problems favored the status quo ($p < .001$ by chi-square). The reluctance to trade is in this instance incompatible with standard utility theory, in which the preference between two policies should not depend on whether one or the other is designated as the status quo. In terms of a two-dimensional value function, defined on changes in inflation and unemployment, the present results imply that both $v(19, -7)$ and $v(-19, 7)$ are less than $v(0, 0) = 0$.

We have seen that the combination of risk aversion for gains and risk seeking for losses is consistent with incumbency-oriented voting: incumbents profit from good times, and challengers from bad times. We wish to point out that loss aversion is consistent with another widely accepted generalization, namely that the incumbent enjoys a distinct advantage over the challenger. This effect is frequently attributed to such advantages of holding office as that of obtaining free publicity while doing one's job and being perceived by voters as more experienced and effective at raising funds (Kiewiet 1982). To these considerations, the present analysis of choice adds the consequences of the value function. Because it is natural to take the incumbent's policy as the status quo—the reference point to

which the challenger's policy is compared—and because losses loom larger than gains, it follows that the incumbent enjoys a distinct advantage. As we argued earlier, the introduction of risk or uncertainty also tends to favor the incumbent under conditions that enhance risk aversion; that is, when the general conditions are good or even acceptable, voters are likely to play it safe and opt for the relatively riskless incumbent. Only when conditions become unacceptable will the risky challenger capture an edge. Hence, the properties of the value function are consistent with the generally observed incumbency effects, as well as with the exceptions that are found during hard times.

Loss aversion may play an important role in bargaining and negotiation. The process of making compromises and concessions may be hindered by loss aversion, because each party may view its own concessions as losses that loom larger than the gains achieved by the concessions of the adversary. (Bazerman 1983; Tversky and Kahneman 1986). In negotiating over missiles, for example, each superpower may sense a greater loss in security from the dismantling of its own missiles than it senses a gain in security from a comparable reduction made by the other side. This difficulty is further compounded by the fact, noted by several writers (e.g., Lebow and Stein 1987; Ross 1986), that the very willingness of one side to make a particular concession (e.g., eliminate missiles from a particular location) immediately reduces the perceived value of this concession.

An interesting example of the role of the reference point in the formation of public opinion was brought to our attention by the actor Alan Alda. The objective of the Equal Rights Amendment (ERA) can be framed in two essentially equivalent ways. On the one hand, the ERA can be presented as an attempt to eliminate discrimination against women.

In this formulation, attention is drawn to the argument that equal rights for women are not currently guaranteed by the constitution, a negative state that the ERA is designed to undo. On the other hand, the ERA can be framed as legislation designed to improve women's status in society. This frame emphasizes what is to be gained from the amendment, namely, better status and equal rights for women. If losses loom larger than gains, then support for the ERA should be greater among those who are exposed to the frame that emphasizes the elimination of discrimination than the improvement of women's rights. To test Alda's hypothesis, we presented two groups of respondents with the following question. The questions presented to the two groups differed only in the statement appearing on either side of the slash within the brackets.

Problem 7 (N = 149)

As you know, the Equal Rights Amendment to the Constitution is currently being debated across the country. It says, "Equality of rights under law shall not be denied or abridged by the United States or by any state on account of sex." Supporters of the amendment say that it will [help eliminate discrimination against women/improve the rights of women] in job opportunities, salary, and social security benefits. Opponents of the amendment say that it will have a negative effect by denying women protection offered by special laws. Do you favor or oppose the Equal Rights Amendment? (check one)

Not surprisingly, a large majority of our sample of Stanford undergraduates indicated support for the ERA (74%). However, this support was greater when the problem was framed in terms of eliminating discrimination (78%) than in terms of improving women's rights (69%).

Just as the formulation of the issue may affect the attitude of the target audience, so might the prior attitude of the audience have an effect on the preferred formulation of the issue. Another group of respondents first indicated their opinion on

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the ERA, either pro or con. They then responded to the following question.

Problem 8 (N = 421)

The status and rights of women have been addressed in two different ways, which have different social and legal implications. Some people view it primarily as a problem of eliminating inequity and discrimination against women in jobs, salary, etc. Other people view it primarily as a problem of improving or strengthening the rights of women in different areas of modern society. How do you see the problem of women's rights? (check one only)

Of those who indicated support of the ERA, 72% chose to frame the issue in terms of eliminating inequity, whereas only 60% of those who opposed the ERA chose this frame. This finding is consistent with the common observation regarding the political significance of how issues are labeled. A familiar example involves abortion, whose opponents call themselves prolife, not antichoice.

Invariance, Framing, and the Ratio-Difference Principle

Perhaps the most fundamental principle of rational choice is the assumption of invariance. This assumption, which is rarely stated explicitly, requires that the preference order among prospects should not depend on how their outcomes and probabilities are described and thus that two alternative formulations of the same problem should yield the same choice. The responses to problems 7 and 8 above may be construed as a failure of invariance. In the present section, we present sharper tests of invariance in which the two versions of a given choice problem are unquestionably equivalent. Under these conditions, violations of invariance cannot be justified on normative grounds. To illustrate such failures of invariance and motivate the psychological analysis, consider the following pair of problems.

Problem 9 (N = 126)

Political decision making often involves a considerable number of trade-offs. A program that benefits one segment of the population may work to the disadvantage of another segment. Policies designed to lead to higher rates of employment frequently have an adverse effect on inflation. Imagine you were faced with the decision of adopting one of two economic policies.

If program J is adopted, 10% of the work force would be unemployed, while the rate of inflation would be 12%. If program K is adopted, 5% of the work force would be unemployed, while the rate of inflation would be 17%. The following table summarizes the alternative policies and their likely consequences:

Policy	Work Force Unemployed (%)	Rate of Inflation (%)
Program J	10	12
Program K	5	17

Imagine you were faced with the decision of adopting program J or program K. Which would you select? [program J, 36%; program K, 64%]

A second group of respondents received the same cover story about trade-offs with the following description of the alternative policies:

Problem 10 (N = 133)

Policy	Work Force Employed (%)	Rate of Inflation (%)
Program J	90	12
Program K	95	17

Imagine you were faced with the decision of adopting program J or program K. Which would you select? [program J, 54%; program K, 46%]

The modal response was program K in problem 9 and program J in problem 10. These choices constitute a violation of invariance in that each program produces the same outcomes in both problems. After all, to say that 10% or 5% of the work force will be unemployed is to say, respectively, that 90% or 95% of the work force will be employed. Yet respondents showed more sensitivity to the out-

comes when these were described as rates of unemployment than as rates of employment. These results illustrate a "psychophysical" effect that we call the *ratio-difference principle*.

Psychophysics is the study of the functional relation between the physical and the psychological value of attributes such as size, brightness, or loudness. A utility function for money, therefore, can also be viewed as a psychophysical scale relating the objective to the subjective value of money. Recall that a concave value function for gains of the form depicted in Figure 1 implies that a difference between \$100 and \$200 looms larger than the objectively equal difference between \$200 and \$300. More generally, the ratio-difference principle says that the impact of any fixed positive difference between two amounts increases with their ratio. Thus the difference between \$200 and \$100 yields a ratio of 2, whereas the difference between \$300 and \$200 yields a ratio of 1.5. The ratio-difference principle applies to many perceptual attributes. Increasing the illumination of a room by adding one candle has a much larger impact when the initial illumination is poor than when it is good. The same pattern is observed for many sensory attributes, and it appears that the same psychophysical principle is applicable to the perception of numerical differences as well.

Unlike perceptual dimensions, however, numerical scales can be framed in different ways. The labor statistics, for example, can be described in terms of employment or unemployment, yielding the same difference with very different ratios. If the ratio-difference principle applies to such scales, then the change from an unemployment rate of 10% to 5%, yielding a ratio of 2, should have more impact than the objectively equal change from an employment rate of 90% to 95%, yielding a ratio that is very close to unity. As a consequence, program K

would be more popular in problem 9 and program J in problem 10. This reversal in preference was obtained, although the only difference between the two problems was the use of unemployment data in problem 9 and employment data in problem 10.

The ratio-difference principle has numerous applications to political behavior. For example, many political choices involve the allocation of limited funds to various sectors of the population. The following two problems demonstrate how the framing of official statistics can effect the perceived need for public assistance.

Problem 11 (N = 125)

The country of Delta is interested in reducing the crime rate among its immigrant groups. The Department of Justice has been allocated \$100 million (\$100M) for establishing a crime prevention program aimed at immigrant youths. The program would provide the youths with job opportunities and recreational facilities, inasmuch as criminal acts tend to be committed by unemployed youths who have little to do with their time. A decision must be made between two programs currently being considered. The programs differ from each other primarily in how the \$100M would be distributed between Delta's two largest immigrant communities, the Alphans and the Betans. There are roughly the same number of Alphans and Betans in Delta. Statistics have shown that by the age of 25, 3.7% of all Alphans have a criminal record, whereas 1.2% of all Betans have a criminal record.

The following two programs are being considered. Program J would allocate to the Alphan community \$55M. and to the Betan community \$45M. Program K would allocate \$65M to the Alphan community and to the Betan community \$35M. The following table summarizes these alternative programs:

Program	To Alphan Community	To Betan Community
Program J	\$55M	\$45M
Program K	\$65M	\$35M

Imagine you were faced with the decision between program J and program K. In light of the available crime statistics, which would you select? [program J, 41%; program K, 59%]

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A second group of respondents received the same cover story and program description as in problem 11, with the criminal statistics framed as follows:

Problem 12 (N = 126)

Statistics have shown that by the age of 25, 96.3% of all Alphans have no criminal record whereas 98.8% of all Betans have no criminal record. . . . In light of the available crime statistics, which would you select? [program J, 71%; program K, 29%]

It should be apparent that the crime statistics on which respondents were to base their choice were the same across the two problems. Because of the ratio-difference principle, however, the Alphans are perceived as much more criminal than the Betans in problem 11—roughly three times as criminal—but they are seen as only slightly less noncriminal than the Betans in problem 12. As hypothesized, respondents selected that program in which differences in allocations between the groups matched as closely as possible differences in perceived criminality, resulting in a large reversal of preference ($p < .001$ by chi-square).

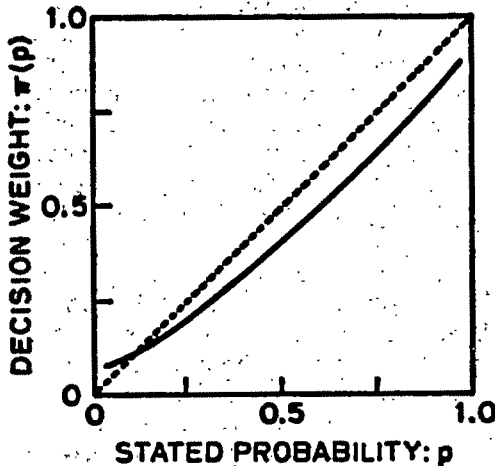
The preceding two problems illustrate an important social problem concerning the perception of crime rates among minority and nonminority segments of the population. It is generally believed that the members of minority groups, such as blacks, have much higher crime rates than do the members of nonminority groups, such as whites (Tursky et al. 1976). Indeed, according to the actual crime statistics compiled by the FBI in 1982, 2.76% of black citizens were arrested for a serious crime compared to .68% of white Americans. The between group difference does appear quite large. Problems 11 and 12 suggest, however, that judgments about the divergent crime rates in the two communities may be altered by how the data are framed. The apparently large difference between crime rates of 2.76% and .68% can be reframed

as a relatively small difference between law-obedience rates of 97.24% and 99.32%.

Quattrone and Warren (1985) showed a sample of Stanford undergraduates the 1982 crime statistics, framed either in terms of the percentages of blacks and whites who were arrested for crime or the percentages who were not. Other respondents were not exposed to these data. As implied by the ratio-difference principle, the respondents who were exposed to the crime commission statistics considered the crime rate to be substantially higher in black communities than in white communities, whereas those exposed to the law-obedience statistics considered the communities to be more at par in crime. Furthermore, the subjects who were not shown the FBI crime data gave responses that were virtually indistinguishable from those given by subjects exposed to the crime commission statistics. This comparison suggests that people may generally formulate beliefs about the proportions of blacks and whites who commit crime, not the proportions who abide by the law.

In another question the subjects who had consulted the FBI statistics were asked to allocate \$100M targeted for the prevention of crime between the two racial communities. It was observed that subjects exposed to the crime commission statistics allocated more money to the black community (mean = \$58.4M) than did the subjects exposed to the law obedience statistics (mean = \$47.2M). Hence, the basic results of this section were replicated for nonhypothetical groups. Moreover, a second study by Quattrone and Warren demonstrated that the same reversals due to framing are obtained when racial differences in crime must be inferred from a set of photographs rather than being explicitly pointed out in a neat statistical table. Taken as a whole, the results suggest that the decision of how to frame the data can have significant political conse-

Figure 2. A Hypothetical Weighting Function



quences for individuals as well as for entire social groups. We suspect that the more successful practitioners of the art of persuasion commonly employ such framing effects to their personal advantage.

The Weighting of Chance Events

A cornerstone of the rational theory of choice is the expectation principle. In the expected utility model, the decision maker selects that option with the highest expected utility that equals the sum of the utilities of the outcomes, each weighted by its probability. The following example of Zeckhauser illustrates a violation of this rule. Consider a game of Russian roulette where you are allowed to purchase the removal of one bullet. Would you be willing to pay the same amount to reduce the number of bullets from four to three as you would to reduce the number from one to zero? Most people say that they would pay more to reduce the probability of death from one-sixth to zero, thereby eliminating the risk altogether, than to reduce the probability of death

from four-sixths to three-sixths. This response, however, is incompatible with the expectation principle, according to which the former reduction from a possibility (one bullet) to a certainty (no bullets) cannot be more valuable than the latter reduction (from four to three bullets). To accommodate this and other violations of the expectation principle, the value of each outcome in prospect theory is multiplied by a decision weight that is a monotonic but nonlinear function of its probability.

Consider a simple prospect that yields outcome x with probability p , outcome y with probability q , and the status quo with probability $1 - p - q$. With the reference point set at the status quo, the outcomes are assigned values $v(x)$ and $v(y)$, and the probabilities are assigned decision weights, $\pi(p)$ and $\pi(q)$. The overall value of the prospect is

$$\pi(p)v(x) + \pi(q)v(y).$$

As shown in Figure 2, π is a monotonic nonlinear function of p with the following properties:

1. Impossible events are discarded, that is, $\pi(0) = 0$, and the scale is normalized so that $\pi(1) = 1$. The function is not well behaved at the endpoints though, for people sometimes treat highly likely events as certain and highly unlikely events as impossible.
2. Low probabilities are overweighted, giving rise to some risk seeking in the domain of gains. For example, many people prefer one chance in a thousand to win \$3,000 over \$3 for sure. This implies

$$\pi(.001)v(\$3,000) > v(\$3),$$

hence

$$\pi(.001) > v(\$3)/v(\$3,000) > .001,$$

by the concavity of v for gains.

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3. Although for low probabilities, $\pi(p) > p$, in general, $\pi(p) + \pi(1 - p) < 1$. Thus low probabilities are overweighted, moderate and high probabilities are underweighted, and the latter effect is more pronounced than the former.
4. For all $0 < p, q, r < 1$, $\pi(pq)/\pi(p) < \pi(pqr)/\pi(pr)$; that is, for any ratio of probabilities q , the ratio of decision weights is closer to unity when the probabilities are small than when they are large; for example, $\pi(.4)/\pi(.8) < \pi(.1)/\pi(.2)$. This property implies the common response to the Russian roulette problem because $\pi(1/6) - \pi(0) > \pi(4/6) - \pi(3/6)$.

Although the description of π has involved stated numerical probabilities, it can be extended to events whose probabilities are subjectively assessed or verbally implied. In these situations, however, the decision weights may also be affected by the vagueness or other details of the choice.

Certainty and Pseudocertainty

Many public policies involve the allocation of funds for projects whose outcomes cannot be known with certainty. The following problems illustrate how preferences among risky projects may be affected by the properties of π , and the results are contrasted with those predicted by the expected utility model.

Problem 13 ($N = 88$)

The state of Epsilon is interested in developing clean and safe alternative sources of energy. Its Department of Natural Resources is considering two programs for establishing solar energy within the state. If program X is adopted, then it is virtually certain that over the next four years the state will save \$20 million (\$20M) in energy expenditures. If program Y is adopted, then there is an 80% chance that the state will save \$30M in energy expenditures over the next four years and a 20% chance that because of cost overruns, the

program will produce no savings in energy expenditures at all. The following table summarizes the alternative policies and their probable consequences.

Policy	Savings in Energy Expenditures
Program X	\$20M savings, with certainty
Program Y	80% chance of saving \$30M, 20% chance of no savings

Imagine you were faced with the decision of adopting program X or program Y. Which would you select? [program X, 74%; program Y, 26%]

The same respondents who received problem 13 also received the following problem. Order of presenting the two problems was counterbalanced across booklets.

Problem 14 ($N = 88$)

The state of Gamma is also interested in developing clean and safe alternative sources of energy. Its Department of Natural Resources is considering two programs for establishing solar energy within the state. If program A is adopted, then there is a 25% chance that over the next four years the state will save \$20 million (\$20M) in energy expenditures and a 75% chance that because of cost overruns, the program will produce no savings in energy expenditures at all. If program B is adopted, there is a 20% chance that the state will save \$30M in energy expenditures and an 80% chance that because of cost overruns, the program will produce no savings in energy expenditures at all. The following table summarizes the alternative policies and their probable consequences:

Policy	Savings in Energy Expenditures
Program A	25% chance of saving \$20M, 75% chance of no savings
Program B	20% chance of saving \$30M, 80% chance of no savings

Imagine you were faced with the decision of adopting program A or program B. Which would you select? [program A, 39%; program B, 61%]

Because the same respondents completed both problems 13 and 14, we can examine the number who selected each of the four possible pairs of programs: X and

A, X and B, Y and A, Y and B. These data are shown in below.

Problem 13	Problem 14	
	Program A	Program B
Program X	27	38
Program Y	7	16

The pair most frequently selected is X and B, which corresponds to the modal choices of each problem considered individually. These modal choices pose a problem for the expected utility model. Setting $u(0) = 0$, the preference for X over Y in problem 13 implies that $u(\$20M) > (4/5)u(\$30M)$, or that $u(\$20M)/u(\$30M) > 4/5$. This inequality is inconsistent with that implied by problem 14, because the preference for A over B implies that $(1/4)u(\$20M) < (1/5)u(\$30M)$, or that $u(\$20M)/u(\$30M) < 4/5$. Note that programs A and B (in problem 14) can be obtained from programs X and Y (in problem 13), respectively, by multiplying the probability of nonnull outcomes by one-fourth. The substitution axiom of expected utility theory says that if X is preferred to Y, then a probability mixture that yields X with probability p and 0 otherwise should be preferred to a mixture that yields Y with probability p and 0 otherwise. If $p = 1/4$, this axiom implies that X is preferred to Y if and only if A is preferred to B. From the above table it is evident that more than half of our respondents (45 or 88) violated this axiom.

The modal choices, X and B, however, are consistent with prospect theory. Applying the equation of prospect theory to the modal choice of problem 13 yields $\pi(1)v(\$20M) > \pi(.8)v(\$30M)$, hence $v(\$20M)/v(\$30M) > \pi(.8)/\pi(1)$. Applied to problem 14, the equation yields $\pi(.2)/\pi(.25) > v(\$20M)/v(\$30M)$. Taken together, these inequalities imply the observed violation of the substitution axiom for those individuals for which $\pi(.8)/\pi(1) < v(\$20M)/v(\$30M) < \pi(.2)/\pi(.25)$. Recall that for any ratio of probabilities q

< 1 , the ratio of decision weights is closer to unity when the probabilities are small than when they are large. In particular, $\pi(.8)/\pi(1) < \pi(.2)/\pi(.25)$. Indeed, 38 of the 45 pairs of choices that deviate from expected utility theory fit the above pattern, $p < .001$ by sign test.

It should be noted that prospect theory does not predict that all respondents will prefer X to Y and B to A. This pattern will be found only among those respondents for whom the value ratio, $v(\$20M)/v(\$30M)$, lies between the ratios of decision weights, $\pi(.8)/\pi(1)$ and $\pi(.2)/\pi(.25)$. The theory requires only that individuals who are indifferent between X and Y will prefer B to A and those who are indifferent between A and B will prefer X to Y. For group data, the theory does predict the observed shift in modal preferences. The only pair of choices not consistent with prospect theory is Y and A, for this pair implies that $\pi(.2)/\pi(.25) < \pi(.8)/\pi(1)$. This pair was in fact selected least often.

The modal preferences exhibited in the preceding two problems illustrate a phenomenon first reported by Allais (1953) that is referred to in prospect theory as the *certainty effect*: reducing the probability of an outcome by a constant factor has a greater impact when the outcome was initially certain than when it was merely possible. The Russian roulette game discussed earlier is a variant of the certainty effect.

Causal versus Diagnostic Contingencies

A classical problem in the analysis of political behavior concerns the rationality of voting and abstaining. According to Downs (1957), it may not be rational for an individual to register and vote in large elections because of the very low probability that the individual would cast a decisive vote coupled with the costs of

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registering and going to the polls. Objections to Downs's view were raised by Riker and Ordeshook (1968), who argued that an individual may derive from voting other benefits besides the possibility of casting a decisive ballot. These additional benefits are collectively referred to as *citizen's duty*, or *D*, and they include affirming one's allegiance to the democratic system, complying with a powerful ethic, participating in a common social ritual, as well as "standing up and being counted." To these rational consequences of voting, we suggest adding a somewhat less rational component.

Elsewhere (Quattrone and Tversky 1984) we have shown that people often fail to distinguish between causal contingencies (acts that produce an outcome) and diagnostic contingencies (acts that are merely correlated with an outcome). For example, there is a widespread belief that attitudes are correlated with actions. Therefore, some people may reason that if they decide to vote, that decision would imply that others with similar political attitudes would also decide to vote. Similarly, they may reason that if they decide to abstain, others who share their political attitudes will also abstain. Because the preferred candidates can defeat the opposition only if politically like-minded citizens vote in greater numbers than do politically unlike-minded citizens, the individual may infer that he or she had better vote; that is, each citizen may regard his or her single vote as diagnostic of *millions* of votes, which would substantially inflate the subjective probability of one's vote making a difference.

To test this hypothesis, which we call the *voter's illusion*, we had a sample of 315 Stanford undergraduates read about an imaginary country named Delta. Participants were to imagine that they supported party A, opposed party B, and that there were roughly four million supporters of each party in Delta as well as four million nonaligned voters. Subjects

imagined that they were deliberating over whether to vote in the upcoming presidential election, having learned that voting in Delta can be costly in time and effort. To facilitate their decision, they were to consult one of two prevailing theories concerning the group of voters who would determine the electoral outcome.

Some subjects considered the *party supporter's theory*. According to this theory, the nonaligned voters would split their vote fairly equally across the two parties. The electoral outcome would be determined by whether the supporters of party A or party B became more involved in the election. The political experts were split as to whether the supporters of A or B would become more involved, but all agreed that the party whose members did become more involved would win by a margin of roughly 200 thousand to 400 thousand votes. Other subjects received the *non-aligned voter's theory*, which held that the supporters of each party would vote in equal numbers. The electoral outcome would in this account be determined by whether the nonaligned voters would swing their support primarily to party A or party B. The experts were split as to which party would capture the majority of the nonaligned voters, but all agreed that the fortunate party would win by a margin of at least 200 thousand votes.

Note that the consequences of voting included in the rational analysis are held constant across the two theories. In both, the "utility difference" between the two parties, the "probability" of casting a decisive vote, the costs of voting, and citizen's duty are the same. But according to the party supporter's theory, there is a correlation between political orientation and participation; that is, either the supporters of party A will vote in greater numbers than will the supporters of party B, or vice versa. In contrast, the non-aligned voter's theory holds that political orientation is independent of participation because party supporters will turn



out in equal numbers. Therefore, only subjects presented with the former theory could infer that their decision to vote or to abstain would be diagnostic of what their politically like-minded peers would decide. If being able to make this inference is conducive to voting, then a larger "turnout" should be found among subjects presented with the party supporter's theory than among those presented with the nonaligned voter's theory. In fact, when asked, "Would you vote if the theory were true and voting in Delta were costly," significantly more subjects responded *no* under the party supporter's theory (16%) than under the nonaligned voter's theory (7%) ($p < .05$ by sign test).

An additional finding corroborated the analysis that this difference in turnout was attributable to the perceived diagnosticity of voting. Respondents were asked to indicate how likely it was that the supporters of party A would vote in greater numbers than the supporters of party B "given that you decided to vote" and "given that you decided to abstain." Responses to these two questions were made on nine-point scales with verbal labels ranging from "extremely likely" to "extremely unlikely." Subjects were informed that their decision to vote or abstain could not be communicated to others. Nonetheless, subjects exposed to the party supporter's theory thought that their individual choice would have a greater "effect" on what others decided to do than did subjects exposed to the nonaligned voter's theory, $F(1,313) = 35.79$ ($p < .001$). Similar effects were observed in responses to a question probing how likely party A was to defeat party B "given that you decided to vote" and "given that you decided to abstain," $F(1,313) = 40.18$ ($p < .001$). This latter difference was obtained despite subject's knowing that they could cast but one vote and that the likely margin of victory was about 200 thousand votes.

The observed differences between re-

spondents exposed to the party supporter's and nonaligned voter's theory cannot be readily justified from a normative perspective (cf. Meehl 1977). The present analysis of causal versus diagnostic contingencies recalls the tragedy of the commons and it applies to other phenomena in which collective action dwarfs the causal significance of a single individual's contribution: The outcomes of most wars would not have changed had one fewer draftee been inducted, and the success or failure of most charity drives do not ordinarily depend on the dollars of an individual donor. These collective actions defy a routine rational analysis for the individual because if each citizen, draftee, or donor "rationally" refrains from making his or her paltry contribution, then the outcomes would be drastically affected. For this reason, exhortations to vote, to fight, and to help those less fortunate than oneself are usually framed, "If you don't vote/fight/contribute, think of what would happen if *everyone* felt the same way." This argument is compelling. Still, just how *does* an individual's private decision materially affect the decisions made by countless other persons?

Concluding Remarks

We contrasted the rational analysis of political decision making with a psychological account based on descriptive considerations. Although there is no universally accepted definition of rationality, most social scientists agree that rational choice should conform to a few elementary requirements. Foremost among these is the criterion of invariance (or extensionality [Arrow 1982]), which holds that the preference order among prospects should not depend on how they are described. Hence, no acceptable rational theory would allow reversals of preference to come about as a consequence of whether the choice is based on rates of

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employment or rates of unemployment, crime commission statistics or law obedience statistics. These alternate formulations of the problems convey the same information, and the problems differ from each other in no other way. We have seen, however, that these alternate frames led to predictable reversals in preference.

Whether our studies paint a humbling of flattering picture of human intellectual performance depends on the background from which they are viewed. The proponent of the rational theory of choice may find that we have focused on human limitations and have overlooked its many accomplishments. The motivational psychologist, accustomed to finding the root of all folly in deep-seated emotional needs, may find our approach much too rational and cognitive. Many readers are no doubt familiar with the versions of these opposing viewpoints found in political science. *The Authoritarian Personality* (Adorno et al. 1950), for example, well illustrates the use of motivational assumptions to explain the appeal of a particular ideology to certain elements of the population.

The descriptive failure of normative principles, such as invariance and coherence, does not mean that people are unintelligent or irrational. The failure merely indicates that judgment and choice—like perception and memory—are prone to distortion and error. The significance of the results stems from the observation that the errors are common and systematic, rather than idiosyncratic or random, hence they cannot be dismissed as noise. Accordingly, there is little hope for a theory of choice that is both normatively acceptable and descriptively adequate. A compelling analysis of the uses and abuses of rationality in theories of political behavior has been presented by Converse (1975) who has detailed the often arbitrary and inconsistent criteria by which rationality has been defined. Our intention was not to reopen the discussion

about the meaning of rationality but rather to enrich the set of concepts and principles that could be used to analyze, explain, and predict the decisions made by individuals in their private lives, in the market place, and in the political arena.

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THE STRUCTURE OF EMOTIONAL RESPONSE: 1984 PRESIDENTIAL CANDIDATES

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Over the past two decades psychological models of affect have changed from valence (one-dimensional) models to multiple-dimensional models. The most recent models, circumplex models, are two-dimensional. Feeling thermometer measures, which derive their theoretical logic from earlier (valence) models of emotional appraisal, are shown to be confounded. Underlying the variation obtained using feeling thermometer measures are two dimensions of emotional response, mastery (positive emotionality) and threat (negative emotionality). Analysis of the 1984 NES survey suggests that positive emotional response is twice as influential as negative emotional response in predicting presidential candidate vote disposition to the presidential candidates. Reliance on emotional response is shown to be uniformly influential across various strata of the electorate.

Policy considerations have little direct influence on vote disposition, though policy considerations are indirectly related to vote disposition through the influence of issues on the degree of feelings of threat evoked by the candidates.

It has been 15 years since it was first recognized that asking people about their feelings toward presidential candidates was equivalent to ascertaining their vote disposition (Brody and Page 1973). Only recently have feelings—emotions—begun to be the subject of serious study by political scientists. The recent studies on emotions and politics have shown that feelings have strong predictive effects on candidate appraisal (Abelson et al. 1982), on issue appraisal (Conover and Feldman 1986), on political participation (Marcus 1985) and on political communication (Roseman, Abelson, and Ewing 1986). The primary concern of the initial studies has been to establish that emotions are worthy of study and

that they add explanatory power to our understanding of a variety of political phenomena.¹

While the studies cited above have established the value of studying emotions and in redressing the imbalance resulting from the dominance of the cognitive approach to human behavior, it must also be noted that these studies have failed to provide a sound theoretical focus. The recent attention to affect has been marked by contradictory and anomalous findings; so that even when the correct structure of emotional response has been identified, its significance and relevance have not been properly understood (e.g., Abelson et al. 1982). Contradictory findings have been reported in different

studies as to the number of dimensions of emotionality. In a study of political communications, Roseman, Abelson, and Ewing (1986) report four dimensions. In a study of economic issues, Conover and Feldman (1986) report three dimensions. In a study of candidate appraisal, Abelson and his colleagues (1982) report two dimensions. And a recent study of the vote decision, Rahn and colleagues n.d., relies on one dimension of affect evaluation.

The anomalous result perplexing investigators has been the consistent finding that when individuals regard a situation or event (such as a candidate, issue, or circumstance) they often simultaneously report good and bad feelings (Diener and Emmons 1984; Warr, Barter, and Brownbridge 1983). Abelson and his colleagues (1982) find, after examining the emotional responses to various candidates for national office, that "perhaps our most psychologically provocative result is the near independence of negative and positive affect" (i.e., the frequent concurrent reports of both good and bad feelings). All of the studies that use multiple dimensions of emotionality report similar "anomalous" findings. Until and unless the structure of emotional response is clarified, how feelings interact with thinking and influence political behavior will remain obscured.

Three classes of models of the structure of affect are generally used: valence models, discrete models and circumplex models. Valence models are the basis of the familiar "thermometer scales" in the National Election Studies surveys. Discrete models are the basis of the feeling measures in the NES. Circumplex models have been a recent development and will be unfamiliar to most political scientists. Circumplex models offer a theoretical approach to variation in feelings that resolves the conflicting and anomalous findings. More importantly, the theory of circumplex models of affect suggests that feelings provide stable strategic inter-

pretations and that variations in these strategic responses have important political implications.

Before turning to circumplex models of affect, it will be useful briefly to review the theoretical rationale of valence and discrete models of affect.

Valence Models of Emotion

In this approach to the study of beliefs and attitudes affect is defined as the *evaluative* dimension of an attitude, hence the term *valence*. Attitudes and beliefs are held about some object (e.g., person or issue). Attitudes are composed of three dimensions: affect, behavioral disposition, and cognition. Affect and evaluation are treated as synonymous (Fishbein and Ajzen 1975). Each attitude subject is evaluated positively or negatively or neutrally. The assumption underlying this class of models is that feelings are unidimensional and affectively bipolar (i.e., positive feelings at one end, negative feelings at the other, and neutral feelings in the middle). That some may evaluate an object positively while others are neutral or negative towards the object is to be understood as a matter of individual differences—an empirical matter to be studied. The tripartite conception of attitudes is gradually being abandoned by psychologists, because emotion and cognition are coming to be recognized as separate mental processes. It retains appeal to some (Breckler 1984).

In political science, feeling thermometers are the most frequent application of valence models of affect. The feeling thermometers have been a popular measure of assessing feelings elicited by candidates and by groups, parties, organizations, and economic classes. The 1984 NES lists 50 feeling thermometers, including the 18 feeling thermometers on political figures. The validity of findings that rely on feeling thermometers is dependent upon the

assumption that affect has a bipolar one-dimensional structure. As this study will demonstrate, that assumption is invalid.

Discrete Models of Emotion

Discrete models of emotions share certain presumptions. First, different emotions are identified as "alternative appraisals of events" (Roseman 1984). The number of fundamental emotions differs from one model to another. Davies (1980) identifies seven principal affects: moral indignation, anger or hatred, envy or resentment, fear or suspicion, powerlessness or disillusion, sympathy or pity, and loyalty or pride. Izard (1971, 1977) proposed a 10-fundamental-emotions model. Roseman has proposed 13 fundamental emotions that characterize the structure of emotions (Roseman 1979). His most recent list of the fundamental emotions includes 14 fundamental emotions: joy, relief, hope, fear, frustration, liking, disliking, anger, guilt, pride, and regret (Roseman 1984). Different combinations of motivational states and situational factors present different circumstances, which then, in turn, elicit different interpretations (cognition) that produce an appropriate emotion (cognitively triggered affect). Perceptions trigger emotional responses depending on the motivational circumstance (for example, whether the event is perceived as rewarding or punishing) and on the situational context (for example, whether the event is seen as personally invested or personally neutral). A situation sensed as punitive will be experienced as a negative emotion: fear or distress if control of the situation is attributed to oneself, anger or hatred if control is attributed to someone else.

Discrete models have made some useful contributions, principally in beginning to pursue parsimony in the structure of emotional appraisal, particularly important since the number of distinguishable emotional terms is measured in the hundreds

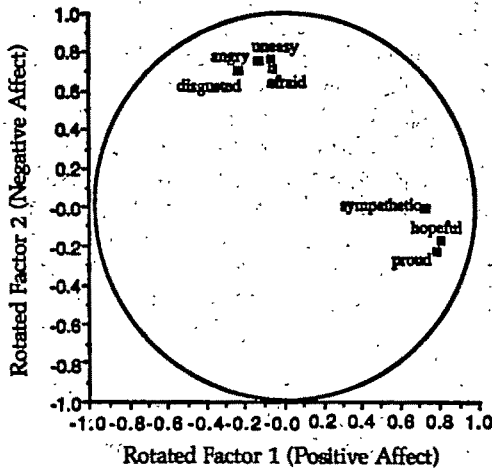
(e.g., Storm and Storm 1987; Wiener, Russell, and Lerman 1978, vol. 2). In particular, discrete models posit that evaluation (emotional appraisal) is to be understood in more strategic terms by positing a link between specific adaptive benefit (Plutchik 1980). Further, by emphasizing the emotional content of the representation of sensory stimuli and its influence on thought and action, discrete models have begun the transition from the orthodox position that cognitive processes can provide a comprehensive account of thinking, awareness, and behavior.²

However, proponents of discrete models disagree at the theoretical level as to the number of discrete emotions (ranging from 7 to 14); and at the empirical level, studies of politics using discrete models report varying dimensions of emotional appraisal (ranging from 2 to 4). And, of greatest importance, empirical studies consistently report that individuals report both positive and negative feelings so frequently that discrete models must conclude either that this is an anomalous finding or that most of the time people are in conflicted motivational states.

Circumplex Models of Emotion

More recent developments have identified the structure of affect as a two-dimensional circumplex (Plutchik 1980; Russell 1980; Zevon and Tellegen 1982). The two dimensions are generally identified as positive emotionality and negative emotionality (affectively unipolar axes at orthogonal locations). Each dimension represents a specific arousal system. One arousal system characterizes certain motor-sensory signals as positive emotionality, ranging from dull and depressed when arousal is low and to elated enthusiastic when arousal is high. The second arousal system characterizes certain motor-sensory signals as negative emotionality, ranging from calm and placid

Figure 1. Emotional Response to Mondale on Seven Mood Terms

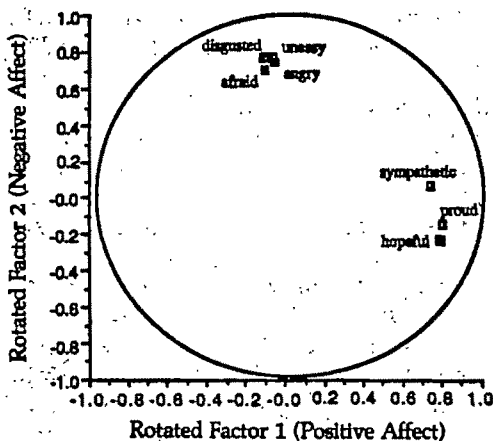


when arousal is low to nervous and anxious when arousal is high. The circumplex model locates all affect terms within the circumplex, the location varying as a result of the differential degrees of arousal on each dimension. Studies of facial expression, tones of voice, and affect words—in English and in other languages—all report the same basic two-dimensional structure (studies reviewed in Wat-

son and Tellegen 1985; see also Masters and Sullivan 1986).

The circumplex model resolves why different studies have reported more than two dimensions. Studies that include affect terms that are heavily clustered in different regions of the circumplex and rely on oblique solutions will indeed report three or more dimensions (e.g., Conover and Feldman 1986; Roseman, Abelson, and Ewing 1986). However, reanalysis of studies with multiple oblique factors reports results that unambiguously support the two-dimensional circumplex model (Watson and Tellegen 1985). The Abelson and colleagues 1982 study of emotional appraisal of candidates, designed to demonstrate the relevance of emotions using one discrete model of emotions (Roseman's structural model; see Roseman 1979, 1984), reports results that validate the circumplex model (though they do not recognize it as such). Thus studies designed from a discrete model perspective, using measures intended to demonstrate the distinct influences of each primary emotion, have found, to the contrary, two dimensions of affect—positive and negative.

Figure 2. Emotional Response to Reagan on Seven Mood Terms

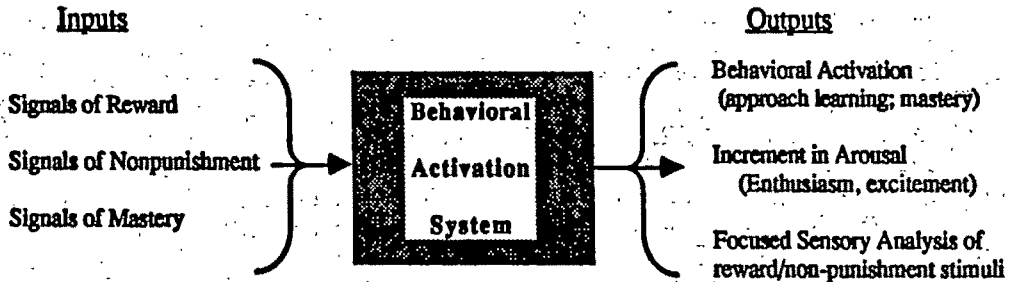


Preliminary Empirical Test

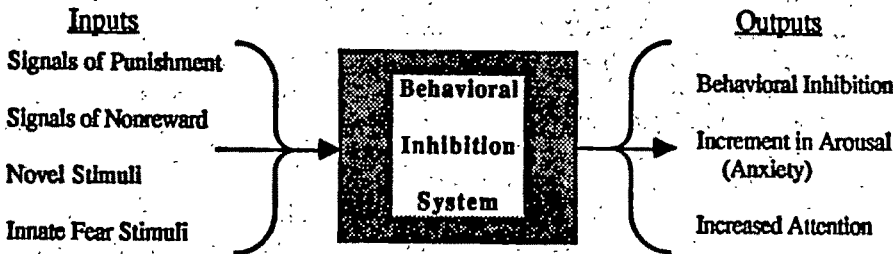
The 1984 NES data afford an opportunity to compare the circumplex model of emotion against valence and discrete models as it applies to emotional response to presidential candidates. Replicating the methodology used in previous NES studies, respondents in the 1984 NES were asked to regard the presidential candidates and asked if "something about that person, or something he has done has made you have certain feelings like anger or pride." Subjects then reported whether they have ever felt that emotion, using a list of seven discrete emotions selected from Roseman's discrete model of the structure of emotions (angry, hopeful,

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Figure 3. Gray-Tellegen Two-Factor Model of Emotional Appraisal



The BAS System—Appraisal and Evaluation of Task Performance
(Scanning for Indications of Success/Failure of Subject Engaged Actions)



The BIS System—Appraisal and Evaluation of Intrusive Information
(Scanning for External Danger or Threat)

afraid of him, proud, disgusted, sympathetic towards him, uneasy). This analysis should provide a robust test since the selection of the affect terms was intended to support the discrete model of emotions (Abelson et al. 1982; Roseman, Abelson, and Ewing 1986). Discrete models of emotions should find a multidimensional solution, probably oblique in structure. Figures 1 and 2 display plots of the factor loadings from factor analyses of the seven emotional measures for Mondale and for Reagan.

Only two dimensions emerge from the factor analysis.³ The plotted factor loadings replicate the findings reported in Abelson *et alii* 1982; emotional response to presidential candidates is two-dimen-

sional. These results contradict the expectations of the valence model that affective evaluation is one-dimensional and validates the circumplex model's expectation that affect is two-dimensional. The only departure from the circumplex model is the somewhat oblique relationship between the two dimensions (which will be addressed more fully in the later covariance analysis). These results are not specific to the 1984 NES data; comparable findings are reported in Abelson and colleagues 1982 and Sullivan and Masters 1988.

The discrete model, as well as a lay grasp of the labels *positive* and *negative*, would suggest that people feel either positive or negative feelings, but not both

(i.e., affect dimensions would be bipolar rather than unipolar). The terms *positive* and *negative* seem mutually exclusive; much like *black* or *white*, *up* or *down*. Yet this is clearly not the case. To resolve this dilemma we need to turn to a more specific circumplex model of affect.

The personality models of Gray (1970, 1973, 1981) and Tellegen (1985) identify emotionality as the manifestation of two primary systems of emotional arousal.⁴ Gray calls the first system the behavioral activation system (BAS). Tellegen calls it positive emotionality. The second system in Gray's model is the behavioral inhibition system (BIS), which Tellegen calls negative emotionality. Figure 3 depicts the Gray-Tellegen model of affect.

The primary contribution of the Gray-Tellegen model of emotion is that each system of emotional response is identified as having a distinct strategic function. One system is oriented towards appraising the implementation of subject-engaged action (positive emotionality). The strategic focus of the positive dimension of emotionality is self-regarding and task management.⁵ This system monitors the ongoing mastery and control achieved by the individual as a result of the recruitment and allocation of mental and physical resources. This system produces variation in the arousal of positive emotion, affects of enthusiasm, pleasure, and excitement. Such affects occur when the task at hand is going well (and conversely, when things are going badly, affects of depression are likely). The second system, negative emotionality, monitors the environment for signs of evident danger. This system is environment-regarding; it is concerned with scanning for threats that may intrude and disrupt ongoing subject-engaged action. Some examples of such stimuli—loud or sudden noises or the appearance of a danger in the form of sudden bad weather during an airplane flight or an unexpectedly violent individual or hostile person appearing on the

scene—reflect the kinds of stimuli that may elicit negative affects.

The two systems efficiently balance the assignment of mental and physical resources between self-absorption (full allocation to the task, or social activity, at hand, which is adaptive and efficient if anxiety is low—i.e., a safe environment) and the claims of attention warranted by external circumstances when confronting a threat (Simon 1967). Further, the two systems of emotionality are well adapted to maintaining social integration through emotional display (Masters and Sullivan 1986; Sullivan and Masters 1988). The development of models of affect has been a steady trend moving from a recognition that affect is engaged in evaluation (valence models) to the further recognition that affect is also linked to motivation (discrete models) and, more recently, to the recognition that affect supports two specific strategic evaluations (circumplex models).

The identification of two principal sources of behavior, self-initiating or self-engaged action on the one hand and action initiated in response to external causes on the other hand, is a familiar one in political science. For example, political efficacy is currently defined as two-dimensional (Craig and Maggionto 1982; Shingles 1986). One dimension concerns attitudes toward self-initiating political action (internal political efficacy) and one dimension concerns attitudes assessing actions by others (external political efficacy). In psychology, a number of personality theories depend on this distinction. For example, recent work on self-monitoring (Snyder and Ickes 1985) and on field dependence also focus on the location of mastery (self versus situation) and the degree of mastery (high or low). Those described as high self-monitors rely on situational information to regulate their behavior. Those described as low self-monitors rely on information from relevant inner states to regulate their

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behavior. The locus of control literature (Phares 1978; Rotter 1966) also identifies two personality types on the basis of the issue of mastery: contrasting between those who believe that outcomes are the result of their own behavior (internals) and those who believe that outcomes are essentially contingent on luck, fate, or powerful others (externals).

The Gray-Tellegen model offers some advantages over these conceptions, though it has much that it shares with them. First, it distinguishes between the two dimensions, threat and mastery, and the degree of arousal (high or low) on each dimension. Second, it differentiates between mood and temperament (again enabling situational and dispositional influences to contend).⁶ Third, it establishes connections between emotional response and motivation (something that information-processing models of cognition fail to do [Hastie 1986]). Finally, while it is almost certain that the Gray-Tellegen model is incomplete in important ways (Derryberry and Rothbart 1984), its identification of two fundamental superordinate dimensions is parsimonious and capable of emendation in light of further work.

With respect to emotional responses to politicians, the Gray-Tellegen model also has specific advantages. The two dimensions of emotional response suggest that political candidates are appraised on two fundamental criteria. First, Is this candidate capable of being in personal command and able to make the environment more tractable, predictable, and bountiful, assuming that such resources are being used to achieve policies and actions supported by the individual (mastery)? The more these features are apparent, the greater the enthusiasm. Second, Is the candidate likely to pursue policies and actions not supported by the individual or give evidence of lack of stature or moral weakness (threat)? The more these features are apparent, especially if they

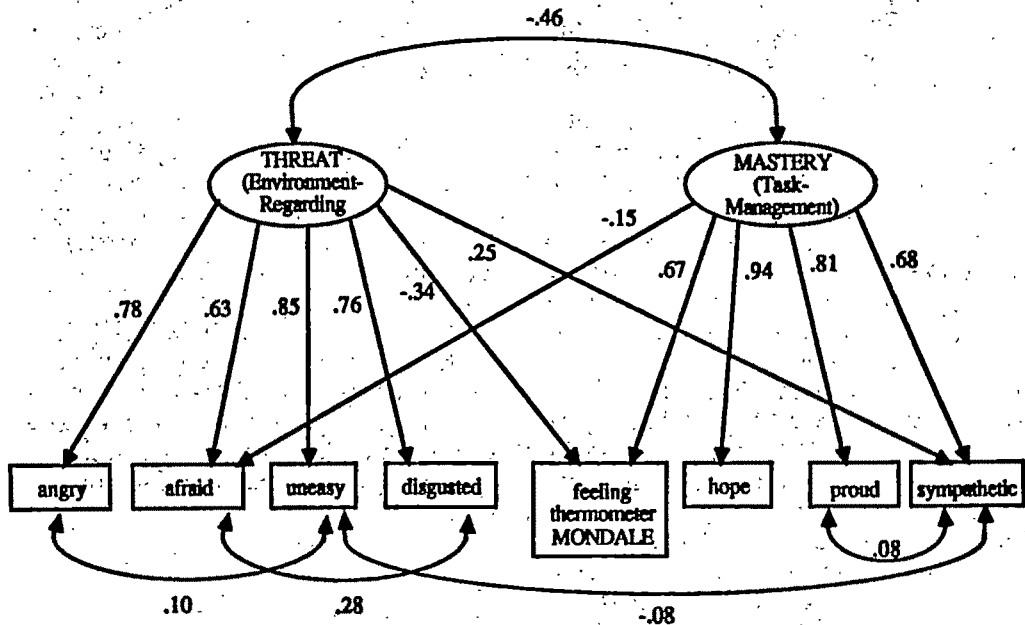
become so suddenly or unexpectedly, the greater the anxiety and distress.

The strategic significance of these appraisals suggests why emotional responses to candidates, and other political subjects and events, are likely to have such empirical predictive validity. Helplessness and depression are inextricably intertwined (Seligman 1975) and concerns for mastery, usually couched as autonomy in political thought, have been central to liberal democratic theory (Bay 1965; Fromm 1941; Lane 1978; Mill 1956). It would be surprising indeed if information about candidates as to the enhancement or diminishing of mastery or the increase or decrease of threat did not play a central role in influencing support for political leaders.

Data Analysis

Fitting a confirmatory factor analysis (LISREL) model (Jöreskog 1969, 1970, 1973) to the 1984 NES data is an effective way of assessing whether the circumplex model can provide a superior alternative to discrete and valence model interpretations of the candidate feeling measures. LISREL requires that a model be specified on theoretical grounds. The LISREL program evaluates the goodness of fit, identifying where the model and data do and do not fit. The data consist of the covariance structure describing the relationships between the various feeling measures and the feeling thermometers. The variables are treated as manifest indicators of latent constructs. The Gray-Tellegen circumplex model identifies the proper structure of emotions as two dimensions, mastery and threat, each affectively unipolar, and the relationship between the two dimensions as orthogonal. Valence models would posit a single affectively bipolar dimension and discrete models would posit a multidimensional solution of 10, 13, or 14 oblique dimensions (limited in this in-

Figure 4. Two-Factor Model of Emotional Response: Mondale



Source: 1984 NES.

stance to 7 by the number of measures available in the NES).

The latent construct *mastery* is defined by the indicators *hopeful*, *proud*, and *sympathetic*. The latent construct *threat* is defined by the indicators *angry*, *afraid*, *disgusted*, and *uneasy*. The feeling thermometers for Reagan and Mondale are identified as confounded measures; that is, the feeling thermometers are indicators that are causally dependent upon both latent constructs.

The model is estimated using polychoric and polyserial correlations, calculated by LISREL, because the feelings measures reported for each candidate are dichotomous. As a result the models are estimated using unweighted least squares (ULS).⁷ Figures 4 and 5 present this model specification and the standardized solutions for Mondale and Reagan.

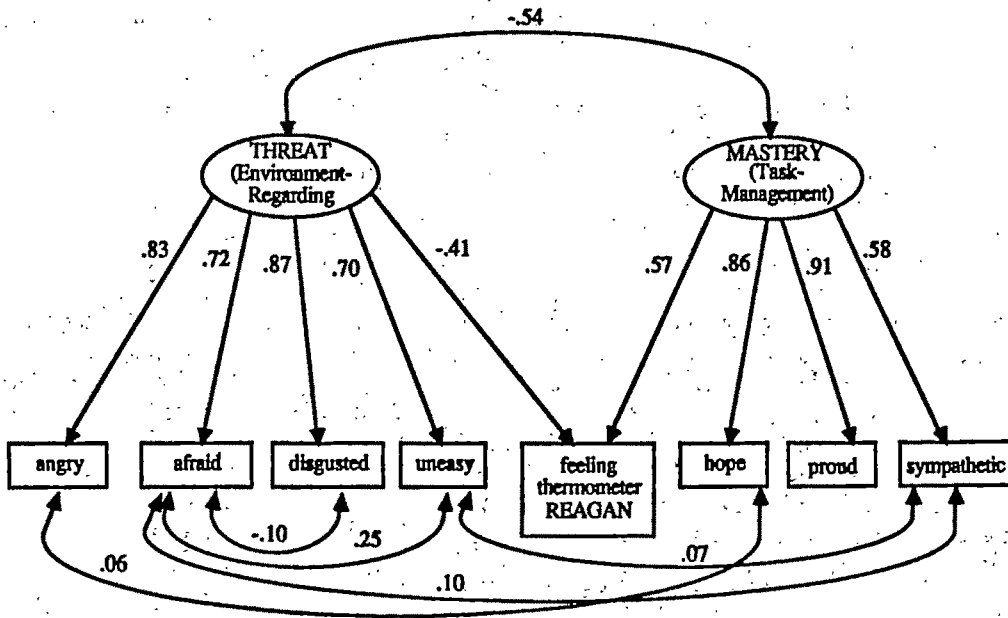
The chi-square value is not available

for ULS estimation; however, goodness-of-fit indexes and, more importantly, the root mean square residual value are provided. For Mondale, the goodness-of-fit index is .999, the adjusted goodness of fit is also .999, and the root mean square residual is .013. For Reagan, the goodness-of-fit index is .999, the adjusted goodness-of-fit is .998, and the root mean square residual is .014. These values demonstrate that each model fits the data very well.

The variance in feeling thermometers is essentially defined by the two latent constructs of emotional response, threat and mastery. The model accounts for much of the variance of the feeling thermometers; $R^2 = .78$ for Mondale's feeling thermometer and $R^2 = .74$ for Reagan's feeling thermometer. These R -squared values are as high as are generally reported for single items in measurement models (the

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Figure 5. Two-Factor Model of Emotional Response: Reagan



Source: 1984 NES.

remaining variance is assumed to be error variance; few single items are completely reliable indicators of a latent construct).

There are some correlated errors in each model, none from one domain to the other domain of great magnitude (the highest being $r = .10$ from *afraid* to *sympathetic* in the Reagan model). The factor analysis, reported in Figures 1 and 2, suggested that *sympathy* is more problematic as a measure of mastery (positive affect). The relatively low epistemic correlations (reliabilities) in each model and the epistemic relationship from threat to *sympathy* in the Mondale model confirm that assessment. Tomkins (1981) has suggested that some affective terms are combinations of biological primary affects and cognitive references. The LISREL and factor analysis of *sympathy* suggests that it may be one of these combinations.⁸ The substantial negative correlations between

the two latent factors, threat and mastery, may be due to either the choice of affect terms (see Appendix) or perhaps to the propensity for respondents to positively respond to political candidates (Lippmann 1922).

These results provide the preliminary evidence that using the feeling thermometer measures, even as a summary, "on-balance" measure of affect evaluation, obscures more than it reveals. Mastery plays a more influential role than threat in defining the feelings towards Mondale and Reagan. The epistemic correlations suggest that mastery may be as much as twice as influential as threat (an interpretation consistent with the reported factor analysis).⁹ This suggests that identification with the candidate (positive affect) is the more potent consideration influencing the disposition to vote for a candidate—though threat is also a powerful influence

(not surprising in the light of the frequent use of "negative campaigning").

But before turning to the question of how feelings may influence voting, can more evidence be brought forward to validate the task management-environment regarding distinction? Theories of democratic leadership identify at least two major ideal types of leadership roles, trustee and delegate (Pitkin 1967). The trustee role, as an ideal type, suggests leadership principally evaluated on bonds of trust and identification (good trustees would display positive qualities of competence and rectitude). The delegate role, as an ideal type, suggests leadership principally evaluated on common policy actions and issue agendas.

While as normative ideals these types are drawn as distinct, we can assume that, empirically, elements of each may have some influence on leadership appraisal.¹⁰ Of course, specific candidates are likely to display features relevant to each ideal type of representative and therefore elicit assessments by the public on standards derived from each type. If we can identify characteristic features of candidates that are apparently linked to leadership, such as trust and identification, and features of candidates that are apparently linked to policy, then it would be possible to test whether the former are likely to elicit feelings of confidence and enthusiasm and whether the latter are likely to elicit feelings of threat. We can hypothesize that personal characteristics of a candidate, features that suggest personal control and probity, are more likely linked to feelings of mastery and that issue positions, describing plans to shape the environment in particular ways, are more likely linked to feelings of threat.

Studies of candidate appraisal have found that a variety of considerations may play a role in assessing candidates (Kinder 1986; Miller, Wattenburg, and Oskana 1986). While these studies have

found that respondents utilize a variety of considerations, the various considerations can be divided into two domains. The first domain contains appraisal of moral leadership (integrity, empathy, honesty, and the like). This domain identifies features of a candidate that are likely to elicit feelings of respect and trust. The second domain identifies appraisal of command: Does the performance of the candidate yield, whether from luck or skill, positive outcomes? This second domain contains appraisals of performance in meeting the crises and demands of the world at large. The failure of a leader to meet the confronting challenges successfully should, according to the task management-environment regarding distinction elicit more feelings on the threat dimension than on the mastery dimension of emotional appraisal. Though, if the failure is perceived to be due to a failure of character, depression rather than anxiety may be a dominant feeling among followers. The 1984 NES contains variables that can be used to construct measures of both moral leadership resources (the first domain) and leadership competence (the second domain).

The 1984 NES contains a number of different assessments of the two presidential candidates: their personal characteristics (15 measures) and Reagan's job approval ratings (4 measures). In addition, questions ascertaining issue positions were used to construct measures of the closeness of the respondent to the two candidates on leading foreign policy issues and domestic policy issues. For Reagan we can assess leadership competence by combining the measures of Reagan's job approval (a simple additive scale combining the four measures of Reagan's job approval, V258, V260, V262, and V264. Cronbach's $\alpha = .88$).¹¹ A comparable set of measures for Mondale is not available.

For both candidates the personal characteristics of the candidate can be assessed

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by using the 16 assessments for Reagan (V319-34) and Mondale (V335-50). Factor analyses of these 16 assessments ("How well does the word _____ describe _____?") shows a single factor accounting for approximately 60% of the common variance for both candidates (the second factor in each case accounts for less than 10% of the common variance). Other studies have found more dimensions (Miller, Wattenburg, and Oskana 1986). A factor analysis of the 1984 NES measures finds that single factors best describe Reagan's characteristics and Mondale's characteristics. Five terms loaded high on both first factors: *hard-working*, *decent*, *compassionate*, *moral*, and *kind*. The common content of this factor can best be described as moral leadership (i.e., matters of rectitude and leadership resources). Simple additive scales for Reagan and Mondale were created from these five items (Cronbach's $\alpha = .88$ for Reagan and Cronbach's $\alpha = .85$ for Mondale).

Issue appraisal measures were created using the simple absolute distances between respondent (V122) and candidates on the liberal-conservative scale (V126 for Reagan and V130 for Mondale) and on the four domestic policy issues on which the respondent's position is ascertained (V375, V382, V401 and V414: government spending, minority aid, economic status of women, and government guaranteed living-guaranteed job). The reliabilities for the respondent-Reagan proximity scale on domestic issues (Cronbach's $\alpha = .81$) and respondent-Mondale proximity scale on domestic issues (Cronbach's $\alpha = .76$) are acceptable. Similar proximity scales were created on three foreign policy issues (V395, V388, and V408: defense spending, involvement in Central America, and cooperation with Russia) and acceptable reliabilities were obtained; Cronbach's $\alpha = .75$ for respondent-Reagan and Cronbach's $\alpha = .67$ for respondent-Mondale.

It would seem plausible to argue that perception of a candidate taking a different policy position would most likely generate feelings of threat (since, if elected, that candidate would attempt to move the society in directions not favored by the respondent). Thus we can hypothesize that the closer the respondent perceives the candidate to his or her position, then the less threatening; *and* that this appraisal should have little impact on feelings of mastery. On the other hand, insofar as the relationship between political leader and supporter is one of identification (as almost all varieties of theories of political leadership attest), then perceptions of the candidate regarding the effective handling of the tasks at hand should be more strongly linked to the elicitation of positive emotionality than to the elicitation of negative emotionality.

In the two regression equations that follow, a number of candidate characteristics, policy appraisals, and one dimension of emotional response are entered as independent variables predicting to the other of the two dimensions of emotionality. The purpose of the regression equations is to identify which candidate features are particularly related to each dimension of emotionality. The regression equations should not be interpreted as claims of causality. The reciprocal influences of emotional and cognitive appraisal remain insufficiently understood to be able to specify with any confidence the causal interplay of the various interpretive processes. There is a lively debate in psychology on this subject and little consensus (Fiske and Pavelchak 1985; Lazarus 1982, 1984; Tassinari et al. 1984; Tomkins 1981; Tsai 1985; Zajonc 1980, 1982). There is little doubt that emotional processes influence memory (Bower 1981) and that emotional and cognitive processes are mutually influential, but precisely *how* is currently not well understood (but see Millar and Tesser 1986). The primary purpose of these

equations, then, is to gain a better understanding of the two distinct dimensions of feelings.

Table 1 presents the relationship of candidate characteristics and policy appraisals to the scale of threat (a simple, summated, equally weighted scale of the four negative affect terms, *angry*, *afraid*, *disgusted*, and *uneasy*; Cronbach's $\alpha = .74$ for Reagan and Cronbach's $\alpha = .75$ for Mondale). Table 2 presents the relationship of candidate characteristics and policy appraisals to the scale of mastery (a simple, summated, equally weighted scale of the three positive affect terms, *hopeful*, *proud*, and *sympathetic*; Cronbach's $\alpha = .68$ for Reagan and Cronbach's $\alpha = .68$ for Mondale). In order to obtain a minimally acceptable scale, in terms of reliability, the term *sympathetic* is retained for use in the scales for Reagan and for Mondale.

The findings generally support the threat-mastery distinction as based on self-initiating and environment-regarding appraisals.¹² For Reagan and for Mondale moral leadership is strongly related to both threat and mastery but more strongly associated with feelings of mastery than it is with feelings of threat. For Reagan, leadership competence—his job approval—is more associated with threat than it is with mastery: the ratio of the β for leadership competence to the β for moral leadership is about two to one.¹³

Of the six possible issue appraisals, closeness of the respondent to Reagan and to Mondale on ideology and domestic and foreign policy issues, three have a substantial association with threat. The farther Reagan is perceived from the respondent on foreign policy issues, the more threatening he is to the respondent (a relationship stronger than the influence of party identification). For Mondale, the crucial appraisal is perceived closeness on domestic issues. The farther Mondale is perceived from the respondent on domestic issues, the more threatening.

Given the tenor and rhetoric of the 1984 campaign, the respective vulnerabilities of the two candidates match closely these findings; Reagan vulnerable on foreign policy and Mondale vulnerable on domestic policy (Pomper et al. 1985). For Mondale only, perception of distance from the voter on ideology has the consequence of evoking threat. This confirms the conventional wisdom that Reagan was generally not perceived in ideological terms. Table 2 shows that only on domestic policy and only for Mondale is closeness between candidate and respondent related to positive emotional response.

Party identification plays a more influential role in determining Mondale's appraisal than it does Reagan's (though this is probably influenced by the lack of a job performance assessment for Mondale; party identification may "pick up" that variance). Interestingly, the small but negative values for party identification and feelings of mastery associated with Reagan suggest that the most partisan Republicans had somewhat more reserved feelings about Reagan than less partisan supporters (perhaps as a result of continued conservative complaint that the Reagan performance in office had not done enough for the conservative agenda). More importantly, these small β 's for party identification show that emotional response to Reagan was broadly shared and not very different whatever the partisan identification of the respondent. By comparison, Mondale evokes different feelings among Democrats than among Republicans. This suggests that Mondale's appeal (or lack thereof) was, much more than Reagan's, limited to the pool of Democratic party identifiers.

The equation for Mondale is less powerful than that for Reagan (the multiple R -squared for Mondale is some 20 points lower). Most of this difference is apparently due to lack of information on Mondale's competence. The importance of job approval in defining the emotional

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Table 1. Regression of Feelings of Threat on Candidate Evaluations

Candidate Evaluation	Reagan			Mondale		
	<i>b</i>	β	<i>t</i>	<i>b</i>	β	<i>t</i>
Candidate characteristics						
Leadership competence (Reagan job approval)	-.47	-.44	-12.7			
Moral leadership	-.45	-.21	-6.8	-.56	-.20	-7.7
Party identification (Republican)	-.06	-.08	-3.1	.13	.21	6.9
Policy appraisals						
Domestic issues (closeness)	-.03	-.02	-.8*	-.18	-.15	-4.9
Foreign issues (closeness)	-.13	-.13	-4.7	-.09	-.05	-1.6*
Ideological closeness	-.06	-.06	-2.2*	-.10	-.10	-3.2
Emotional response						
Mastery	.13	.13	4.9	-.04	-.05	-1.6*
<i>R</i> ²		.52			.29	
Adjusted <i>R</i> ²		.52			.28	
<i>F</i>		208.90			85.71	
<i>df</i>		7, 1369			6, 1273	

Source: 1984 NES.

*Not significant at $p = .01$.

Table 2. Regression of Feelings of Mastery on Candidate Evaluations

Candidate Evaluation	Reagan			Mondale		
	<i>b</i>	β	<i>t</i>	<i>b</i>	β	<i>t</i>
Candidate characteristics						
Leadership competence (Reagan job approval)	.39	.36	10.2			
Moral leadership	.74	.33	11.5	.85	.28	11.5
Party identification (Republican)	-.06	-.06	-3.1	-.22	-.31	-11.3
Policy appraisals						
Domestic issues (closeness)	.05	.04	1.4*	.13	.10	3.4
Foreign issues (closeness)	.05	.05	1.9*	.06	.05	1.8*
Ideological closeness	.06	.05	2.2*	.05	.04	1.5*
Emotional response						
Threat	.13	.13	4.9	-.05	-.04	-1.6*
<i>R</i> ²		.53			.34	
Adjusted <i>R</i> ²		.53			.34	
<i>F</i>		224.8			111.5	
<i>df</i>		7, 1369			6, 1273	

Source: 1984 NES.

*Not significant at $p = .01$.

characterization of President Reagan might, as many have concluded, suggest the benefit of incumbency to candidates for reelection. One handicap facing the challenger is that demonstrating ability to perform in the office must be made without the electorate having the opportunity to observe the challenger in the role of office holder (unless the challenger previously held the office). Of course, some incumbents find their history not so valued an asset (Ford and the pardon of Nixon; Carter and the perceived economic record), and in this instance Mondale has been Carter's vice-president. There is no formal evaluation of Mondale's expected job performance available in the 1984 NES data, so the two analyses are not precisely equivalent. Apart from this difference, the pattern of findings is remarkably similar.

On balance, the pattern of findings suggests that feelings of threat are evoked by perception of job performance, by perceptions of moral leadership, and by policy appraisals. The findings also suggest that feelings of mastery are evoked equally strongly by appraisals of moral leadership rectitude and leadership competence and more weakly by party identification and perhaps some policy appraisals. Finding that each dimension of affect covaries in different ways and to different degrees with policy appraisals and leadership characteristics is consistent with the theoretical assertion that each system of emotional response has a different strategic orientation.

Vote Disposition

Evaluating how much influence cognitive and emotional appraisals have, especially with respect to causality, is problematic, given the current state of knowledge. Brody and Page (1972) argue that distinguishing causality is difficult because issue positions, perceptions, and

evaluations may be subject to persuasion and projection, thus making differentiating causal influences among the proximity judgments difficult. Further, a national cross-sectional survey is not a research design best adapted to the unraveling of the interplay between various sequential and simultaneous relationships. Nonetheless, we can begin to assess the relative influence of the various assessments considered on the propensity to vote for presidential candidate by examining the associations between the different influences and vote intention.

Brody and Page (1973), Kelley and Mirer (1974) and Rahn and her colleagues (n.d.) argue that people compare candidate images, summarizing judgments and feelings so as to form a voting decision based on which candidate fares better in the comparative process. Rahn and colleagues go on to show that using comparative measures consistently produces a more successful predictive model than do models using the respondents' positions on the issues and the respondent's assessments of the candidates' characteristics.

The comparative perceptions approach is replicated here for most of the candidate characteristics. A measure for each respondent was created by subtracting Mondale's leadership scale score from Reagan's leadership scale score. The procedure yields a positive value when the comparison advantages Reagan; zero when the comparison is seen as tied and negative when the comparison advantages Mondale. The scores describe the extent to which a respondent perceives Reagan or Mondale to be the better leader, in the sense of more hardworking, decent, compassionate, moral, and kind. The more positive the scores, the more likely the respondent will decide to vote for Reagan; the more negative the scores, the more likely the respondent will decide to vote for Mondale (insofar as that comparison is influential). Similarly comparative judgments are measured on ideology

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Table 3. Regression of Vote Disposition on Comparative Candidate and Policy Evaluations

Candidate Evaluation	<i>b</i>	β	<i>t</i>
Candidate characteristics			
Leadership competence (Reagan job approval)	.27	.30	10.7
Moral leadership ^a	.04	.03	1.2*
Party identification (Republican)	.09	.15	6.9
Policy appraisals			
Domestic issues ^a	.06	.08	3.3
Foreign issues ^a	.03	.04	1.9*
Ideological ^a	.00	.00	.2*
Emotional response			
Mastery ^a	.14	.27	10.3
Threat ^a	-.08	-.14	-5.7
<i>R</i> ²		.79	
Adjusted <i>R</i> ²		.79	
<i>F</i>		489.3	
<i>df</i>		8, 1016	

Source: 1984 NES.

^aComparative closeness.

*Not significant at $p = .01$.

(the respondent's distance from Mondale subtracted from the respondent's distance from Reagan); domestic policy issues, foreign policy issues, mastery (subtracting the mastery scale for Mondale from the mastery scale for Reagan for each respondent), and threat.

Leadership competence is available only for Reagan, as previously noted, so this is the only measure that is not comparative. I assume, with Rahn and her colleagues, that party identification incorporates a comparative judgment. Unlike Rahn and her colleagues, I treat affect as two-dimensional, with candidates being compared on the degree of feelings of mastery and on the degree of feelings of threat. Table 3, on vote disposition (using the four-point scale for vote disposition),¹⁴ is regressed on candidate characteristics, party identification, policy appraisals, and the two factors of emotional appraisal, threat and mastery. As the vote disposition scale is coded so that intention to vote for Reagan is high, a positive value predicts to a greater inten-

tion to vote for Reagan and a negative value predicts to a greater intention to vote for Mondale.¹⁵

A positive assessment of Reagan's competence and the comparison of feelings of mastery elicited by the two candidates are the two most important factors in predicting a vote disposition favoring Reagan (β 's of .30 and .27 respectively). Next in importance are party identification ($\beta = .15$) and feelings of threat ($\beta = -.14$). Consistent with the LISREL analysis of the feeling measures and the feeling thermometers for Mondale and Reagan, positive feelings are more weighty than negative feelings (a ratio of 2 to 1).¹⁶ As a group the policy appraisal measures do badly in predicting the vote. Only the comparison of perception of closeness on domestic issues reaches statistical significance although its influence ($\beta = .08$) is weak compared to the assessment of Reagan's competence, the two emotional appraisals and party identification. The overall strength of the model is quite high, the adjusted $R^2 = .79$.¹⁷

It is striking that issue appraisals have little apparent direct influence on voting. Feelings about the candidates, rather than thoughtful assessments regarding public policies, appear to be central to the voters' choices. But perhaps it is premature to conclude that issues have little direct influence in determining the outcome of elections. After all, few candidates publicly assert that issues do not matter. A frequent conclusion of voting studies is that the electorate does not engage in careful policy analysis and comparison of the party platforms offered by the major parties of candidates. This conclusion is frequently held to demonstrate that the electorate does not meet the standards required by "classical democratic theory." The obvious references are to Schumpeter, Berelson, and the Michigan school. Perhaps, as is frequently asserted, the public at large does not perform to expectations, but there is a competent political strata that does meet these standards.¹⁸ If, so the argument goes, we divide the electorate into levels of competency, we can discern a more active and influential segment that does have the cognitive skill and experience to utilize the necessary sophisticated skills required of a democratic electorate.¹⁹

In the preliminary analyses that follow three cautionary points should be kept in mind. First, the combination of stratification of the sample and listwise deletion of missing values reduces the size of the subsamples, in some cases substantially. Second, the measurement of cognitive ability and its effects may be better attempted in experimental research designs than in secondary analysis of a cross-sectional national sample. And third, the 1984 NES data are collected quite late in the impression formation process and as a result may be less helpful in illuminating how impressions are formed and evaluating what role cognitive ability and emotional appraisal, respectively, play.

Various measures have been put forth

to define and measure electorate competency, among them different indicators of cognitive complexity, ideological sophistication, and constraint (Neuman 1986). There still remains little consensus on how best to measure cognitive ability in politics (but see Lushkin 1987). We need not select such a measure, as we are here concerned with the search for segments of the electorate that eschew emotional responses to the candidates and rely instead upon the policy positions and ideological perspectives of the candidates. Perhaps the single measure on which most attention has centered is education. While education may not measure cognitive ability in politics directly and may contribute less to its development than expected, most democratic theories expect that education prepares people for citizenship (see, e.g., Thompson 1971). Further, educational attainment has often been used as one measure of sophistication, as it measures the ability and willingness to acquire cognitive skills as well as to learn citizenship (Neuman 1986). If lack of sophistication is the reason for so few using policy considerations to decide which candidate to support, then we can expect to find that stratifying the electorate on education will reveal a difference between those who rely upon feelings about the candidate and those who rely upon policy considerations.

Table 4 presents the same analysis as in Table 3, the regression of vote disposition on candidate characteristics and policy evaluations, but with the data stratified into those with less than a high school diploma, those with at least a high school diploma, and those with some college and postsecondary education. Only unstandardized regression coefficients are provided (*bs*), as the relevant comparisons are between education strata.

The only suggestion that education divides the adult population into strata, each with its own mode of reasoning, is that assessment of Reagan's leadership

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Table 4. Regression of Vote Disposition on Comparative Candidate and Policy Evaluations: Stratified by Education

Candidate Evaluation	Low Education		Medium Education		High Education	
	<i>b</i>	<i>t</i>	<i>b</i>	<i>t</i>	<i>b</i>	<i>t</i>
Candidate characteristics						
Leadership competence (Reagan job approval)	.12	1.8*	.38	7.7	.28	8.5
Moral leadership ^a	.23	2.0*	.03	.4*	.01	.3*
Party identification (Republican)	.22	5.8	.05	2.0*	.08	4.6
Policy appraisals						
Domestic issues ^a	.06	1.2*	.04	1.5*	.07	3.1
Foreign issues ^a	.25	1.2*	.13	1.1*	.16	1.6*
Ideological ^a	.02	.6*	.00	.0*	.03	1.6*
Emotional response						
Mastery ^a	.13	3.0	.14	5.9	.12	7.1
Threat ^a	-.06	-1.3*	-.09	-3.3*	-.08	-5.1
R ²	.83		.80		.80	
Adjusted R ²	.82		.78		.79	
F	60.5		145.2		264.9	
df	8, 100		8, 310		8, 588	

Source: 1984 NES.

^aComparative closeness.

*Not significant at $p = .01$.

competence and moral leadership has little influence on vote disposition among the less educated; in this strata party identification has a substantial association with vote disposition. This might suggest that the less educated, compared to the more educated, rely more on partisan cues and less on candidate characteristics. Studies by Lau (1986) and Miller, Wattenburg, and Oskana (1986) find that the more educated have more political information than the less educated.

Only the domestic issue closeness comparison is significantly related to the vote disposition in the more educated strata (which suggests that issues may have some influence on voters' actions, but most likely this influence is an indirect effect through emotional response). But apart from these differences, the perception that one candidate is ideologically closer to the voter, is closer to the respon-

dent on foreign policy issues, or closer to the respondent on domestic issues does not seem to play a greater role in the vote decision for the more educated than for the less. Moreover, comparative feelings about the candidates seem, if anything, more influential among the more than the less educated (and the ratio of the influence of feelings of mastery to the influence of feelings of threat remains rather constant at two to one, suggesting that across the different educational strata, feelings of threat have about half the impact of feelings of mastery on vote decisions).

Stratifying the sample by interest in politics, another factor commonly held to lead to political sophistication (Neuman 1986), finds even less evidence of greater reliance on cognitive cues among the higher strata. All of the nine policy appraisals, three within each stratum, have weak coefficients and none are statistical-

ly significant (Marcus 1987). The coefficients for the two dimensions of emotional response are weaker in the least interested stratum (.10 and $-.06$ for mastery and threat) and stronger in the moderate and high interest strata and display the same two-to-one ratio found in the analyses above (.17 and .15 for mastery and $-.07$ and $-.09$ for threat). These results suggest the not-surprising relationship that interest in politics and passionate engagement toward or against candidates is linked.

One explanation for why issues play so little influence in candidate choice is offered by Page (1976, 1978). Page's analysis of party platforms and candidate positions concludes that candidates avoid controversial issues and attempt to present ambiguous positions that can appeal to the broadest range of likely voters. Obtaining definitive, or even moderately accurate, information on the positions of candidates on even the most controversial issues of the day may be quite difficult and therefore prevent many voters from issue-voting even if they are so inclined (Page and Brody 1972). Yet for those issues that are important to the voter, issue comparisons may play an important role. To evaluate this possibility, the sample was stratified into thirds by the degree of importance of three domestic issues on which the 1984 NES ascertained how important the issue is to respondents (government spending, economic status of women, and government guaranteed living-guaranteed job; V380, V407, and V420 respectively).²⁰ The results essentially mirror those in Table 4. None of the *bs* for the nine policy appraisals, three in each stratum of importance, has a significant influence on vote disposition. As in the previous tables, the influence of the two dimensions of feeling is stable across the three strata (mastery has *bs* of .15, .13, and .15 and threat has *bs* of $-.05$, $-.08$, and $-.07$ across the three strata).

The results are noteworthy for the con-

sistent reliance on emotional response in the sample as a whole and within various subsamples. While these findings must await confirmation from more appropriate research designs before being treated as more than suggestive, the basic finding of pervasive and uniform reliance on emotional response is consistent with a number of studies in politics (Kinder and Sears 1981; Kuklinski et al. 1987; Sears et al. 1980).

Notwithstanding these findings, it would be incorrect to conclude that issues do not play an influential role in voters choices between presidential candidates. Table 1 presents results that suggest that issues and policy considerations do influence voters by acting upon emotions and particularly on the environment-regarding dimension of emotion, feelings of threat. Emotions are rarely, in the Western tradition, taken as rational or of adaptive benefit, although David Hume is one notable exception (Lloyd 1984). But emotions may provide people with an efficient means of comprehending their circumstances. Reliance on emotional response to characterize candidates on leadership ability, competence, and issues and policy positions provides the electorate with a speedy and efficient means of determining candidate suitability on criteria that are consistent with the requirements of republican forms of government.

Conclusion

The circumplex model provides an economical approach to the study of emotional response. More importantly, by demonstrating that emotionality consists of two concurrent appraisal processes, one strategically dedicated to self-initiating action and one strategically dedicated to evaluating threat, the results reported here resolve the anomalous findings of previous studies that positive and nega-

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tive emotions are often concurrently reported.

The relationship between the structure of emotions and candidate appraisal can also be useful in gaining a better understanding of the likely success of different strategies of political campaigning. The consistent finding that positive emotional response to the candidates is more influential than negative emotional response suggests that elections turn more on moral leadership and leadership competence and less on issues. Issue appraisals do not seem to have much, if any, influence on the emotional enthusiasm created by candidates. Yet issues are shown to have an influence on the threat dimension of emotional response. This suggests that running a campaign based on matching the issue positions of the voters has limited prospects for strengthening a candidate's image of leadership and competence. Indeed, as Gary Hart's decline in the summer of 1987 suggested, running a campaign solely "on the issues" may create a disquieting sense that something is being hidden or that there is something lacking in the character of the candidate. This in turn is likely to provoke press inquiries into the character of the candidate.

Insofar as issue positions of a candidate are seen to be discrepant from the issue positions of the voters, candidates risk arousing feelings of anxiety and threat among voters. Thus candidates may best use issues to forestall others from using issues against them, as in negative campaigning, or from taking positions through miscalculation that place them in opposition to their supporters.

It is important to add that threat feelings are elicited by the prospect of an unpredictable environment as well as by that of a hostile environment (Gray 1981). This would suggest why candidates are so quick to paint their opponents as "flip-flopping on the issues." Successful campaign rhetoric of this genre can make the opposition candidate vulnerable by de-

picting the candidate as uncertain and likely to take unpredictable positions. It would seem that the most successful campaign strategy would try to build support through depicting a candidate as competent to produce good results in the job and of good probity and to use issues only defensively (to sustain calm feelings rather than build enthusiastic support).²¹ Position taking, if misguided, can hurt. But unless necessary to respond to challenges raised by the opposition, position taking is not likely greatly to help a candidate build enthusiasm.

Finally, these results suggest, as has been long noted, that public debate is often accompanied by heat and passion. Madison identified an important relationship between emotions, deliberation, and the attention given to public measures: "It is a measure of misfortune, inseparable from human affairs, that public measures are rarely investigated with that spirit of moderation which is essential to a just estimate of their real tendency to advance or obstruct the public good; and that *this spirit is more apt to be diminished than prompted by those occasions which require an unusual exercise of it*" (*Federalist Papers*, no. 37; italics added). Madison's insight is that passion (or to use our terminology, *emotional response*) is what draws individuals to politics. Madison is properly skeptical of the influence of emotions, but he recognizes that emotions are an unavoidable and dynamic part of politics.²² Emotions do not just form empty feelings, are not merely subjective; rather, they constitute vital strategic understandings (Marcus and Rahn n.d.). While the process by which these emotional responses are arrived at is hidden (Tsal 1985) and is likely to be fallible, emotionality nonetheless does serve instrumental purposes. It is perhaps appropriate to reexamine the received wisdom that emotions constitute "a regrettable imperfection in an otherwise perfect cognitive machine" (Scherer 1982).

Table A-1. Regression of Vote Disposition on Comparative Candidate and Policy Evaluations with Feeling Thermometers To Assess Affect

Candidate Evaluation	<i>b</i>	β	<i>t</i>
Candidate characteristics			
Leadership competence (Reagan job approval)	.27	.30	10.6
Moral leadership ^a	.01	.01	.4*
Party identification (Republican)	.09	.15	6.8
Policy appraisals			
Domestic issues ^a	.05	.07	3.3
Foreign issues ^a	.02	.04	1.8*
Ideological ^a	-.01	-.02	-.9*
Emotional response			
Feeling thermometers ^a	.01	.42	12.0
<i>R</i> ²		.79	
Adjusted <i>R</i> ²		.79	
<i>F</i>		555.3	
<i>df</i>		7, 1026	

Source: 1984 NES.

^aComparative closeness.

*Not significant at $p = .01$.

Appendix: The Measurement of Emotional Response

There is a further test available to assess the claim that the feeling thermometers are confounded measures. The regression presented in Table 3 can be reestimated with the difference between Reagan and Mondale feeling thermometers replacing the two scales of mastery and threat. If relationships are different when the feeling thermometers are used, then it would suggest that the feeling thermometers covary with these independent variables in ways not captured by the two scales of mastery and threat. Table A-1 presents this regression analysis.

The results, except for the measurement of emotional response, are almost identical to those reported in Table 3, with no differences greater than .02 for standardized or unstandardized regression coefficients, *R*-squared or adjusted *R*-squared values. This might suggest that the two-dimensional model adds little to what can

be gained by using the feeling thermometers. However, this conclusion would be a substantial mistake. The feeling thermometers mask two quite different appraisal processes.

The thermometer scale, as a measure of emotional response, is a confounded measurement device. Substantively, responses to feeling thermometers will be an indistinguishable mixture of the intensity of threat and the intensity of mastery evoked by the object of appraisal. Continued reliance on thermometer scales to assess global feelings regarding an object of appraisal would be ill advised.

The feelings measures developed for the National Election Studies are an improvement over the feeling thermometers. They provide multiple measures of the two dimensions and these indicators enable the two dimensions to be discriminated from each other. However, they suffer three liabilities. First, one of the positive mood terms, *sympathetic*, is not a good measure of mastery. It is less reliable than

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is generally desired. And its location in the circumplex, Figures 1 and 2, and the two LISREL models, suggest that sympathy is a compound mood term and should be dropped as a measure of the mastery dimension. Adding one or two additional mastery emotion indicators would seem advisable (see Watson and Tellegen 1985, for numerous alternatives). Second, the feeling measures are dichotomous, thus sacrificing the range of responses offered by the feeling thermometers. Third, current emotional response measures do not identify the nonaffective pole of each dimension. An alternative procedure would be to use semantic differential formats with the appropriate mood term anchoring the low-arousal pole scale and the appropriate mood term anchoring the high-arousal pole of the scale. For example, mastery mood terms and their low-arousal polar opposites such as the following could be used: *indifferent*, *curious*, *enthusiastic*, *unenthusiastic*; *interested*, *withdrawn*; and *depressed*, *happy*. For threat mood terms and their low-arousal polar opposites the following could be used: *tense*, *calm*; *upset*, *comfortable*; *worried*, *contented*; and *nervous*, *relaxed* (Marcus 1985). Using this measurement approach each measure would provide a wider range of scores than the current measures. Further, using this proposed measurement approach would provide for multiple measures and thus ensure ample opportunity for item analysis (reliability and validity assessment) and enable scale building and multiple indicator methods of analysis.

Notes

The data utilized in this analysis were made available by the Inter-University Consortium for Political and Social Research. The data for the 1984 American National Election Study were originally collected by the University of Michigan, Center for Political Studies of the Institute for Social Research for the National Election Studies under the overall direction of Warren E. Miller. Santa Traugott was

director of studies in 1984. The data were collected under a grant from the National Science Foundation. I thank Kimberly Jordan for help in conducting the statistical analyses, and Andy Crider, George Goethals, Saul Kassin, Donald T. Campbell, W. Lance Bennett, Tim Cook, Roger Masters, Lyn Ragsdale, Wendy Rahn, and John L. Sullivan for their useful comments and suggestions.

1. In the data used here, the correlations between feeling thermometers for Mondale and Reagan and vote intention (V425) in the 1984 NES are .50 and .53 for Mondale and Reagan respectively. Further, if we use the strength of voting preference (V426) and voting intention to create a four-point scale, the correlation between Reagan's feeling thermometer score and the vote intention scale increases to .78, and the correlation between Mondale's feeling thermometer and the vote intention scale increases to .71 (the correlation between the vote intention scale and V425 is .94; the increases in correlation between feeling thermometers and vote intention scale result from using a scale with more categories [see Bollen and Barb 1981]).

2. It is necessary to differentiate between conscious and nonconscious cognitive processes, each of which can influence behavior, as well as between the interdependent influences of emotional and cognitive processes (Lewicki 1986; Millar and Tesser 1986; Tassinari et al. 1984).

3. For Mondale, the first factor has an eigenvalue of 2.62 and the second factor has an eigenvalue of 1.54; together the two factors account for 60% of the variance. For Reagan, the first factor has an eigenvalue of 2.74 and the second factor has an eigenvalue of 1.37; together the two factors account for 59% of the variance. In each case the third eigenvalue is well below 1.00 (.77 and .78 respectively).

4. Fonberg (1986) reviews the neurophysiological basis for the two-system paradigm.

5. While the mastery dimension is hypothesized to be self-orientated, the capacity for identification with others (e.g., empathy and group identification) extend appraisals of mastery to collective action and to the actions of others.

6. The full Gray-Tellegen model accounts for mood and temperament (stable emotional differences). Temperament can be defined as reactivity levels. Temperament is the disposition, across situational states, to interpret any environment as, for example, threatening. Because the NES data, used here, does not contain any acceptable measures of temperament, this aspect of the Gray-Tellegen model can not be pursued.

7. The correlation matrices are positive definite. Reestimating with maximum likelihood gives essentially the same solutions. Also, defining the model with three latent concepts, threat, mastery, and feeling thermometer—the latter defined by one indicator—provides the same solution, differing only in that rather than epistemic correlations running from

mastery and threat to the feeling thermometer indicator, the solution has structural parameters running from mastery and threat to the latent concept, feeling thermometer. The values are the same.

8. It may also be the case that there is a third dimension of emotion, *empathy*, which has been shown to have a cognitive and affective dimensions (Davis 1983; Chlopan et al. 1985). The collection of feeling measures in the NES series does not enable this possibility to be pursued. In any case, this and other studies (Masters and Sullivan 1986) suggest that the two dimensions of circumplex model account for the predominant variance in feelings. See n. 3.

9. See n. 4.

10. Sullivan and Masters (1988) have shown using experimental procedures that emotional displays by presidential candidates evoke two dimensions of emotional appraisal in observers, *hedonic* and *agonic*, which are equivalent to the two dimensions conceptualized here as mastery and threat.

11. Scale scores reported for these and all subsequent scales are the average values of the sum of all valid responses when the number of valid responses is equal to, or one less than, the maximum number of items in the scale. For example, a scale with four items will return a scale score if respondents provide valid responses to any three, or all four, scale items. This preserves more cases for subsequent multivariate analysis (which, using listwise deletion, can sharply reduce the number of cases available for analysis).

12. With samples as large as those in the NES series, statistical significance is readily achieved for all but the smallest relationships. Substantive assessments must be made, rather than having interpretation rest on the issue of inferential risk.

13. Since I am interested, in this analysis, in comparing the relative influence of different independent variables and as the scales are "scale-free" (that is, the range of the scale scores is a result of the available number of items making up each scale), the relevant statistic is the β , the standardized regression coefficient.

14. See n. 1.

15. It has been suggested that because competence as a measure of leadership is available only for Reagan, this measure should be removed from the comparative model, in this and in other analyses to follow. Of course, as the incumbent, only Reagan can be evaluated by the voters on presidential leadership performance. For Mondale, voters may extrapolate from Mondale's campaign activity as reported and seen, current policy positions, and from his past record as a political leader. Many voting theories do assume comparison between an incumbent's record of performance and the challenger's positions. Theoretical issues aside, removing Reagan's job approval from the analysis does not change the results in any principal way. All coeffi-

cients increase marginally by about the same small amounts. The *R*-squared drops by an average of .05. More importantly, policy appraisals remain uniformly small and emotional responses become more influential. Alternative tables, with leadership competence removed, are available from the author.

16. The proportion may well change from candidate to candidate and from campaign to campaign, depending on the particular strengths and liabilities of specific candidates. More studies will be needed to ascertain how labile these appraisals are and precisely what kinds of information have most consequence for emotive appraisal.

17. Consistent with the findings reported in Rahn and colleagues n.d., the comparative model does better than models that predict vote choice based on ideological and issues closeness and level of threat and mastery reported for each candidate (i.e., a non-comparative model). For the model predicting vote for Reagan, using distance measures between Reagan and the respondent, the adjusted *R*-squared is .75. For Mondale, the adjusted *R*-squared is .76 when using Mondale's characteristics and closeness to the respondent (including Reagan's job performance in the Mondale model).

18. This tradition has flourished in areas other than voting, for example in the analysis of who comprehends and subscribes to the rules of the game and political tolerance (Mueller n.d.).

19. The studies that find such strata have been countered by studies that dispute the methodology of the studies and the findings themselves (Sullivan, Piereson, and Marcus 1982). But on balance the more accepted position seems to be that put forth by Lippmann, Schumpeter, and others in that tradition (for a review, see Krouse and Marcus 1984).

20. In order to identify the strata, those who identified an issue as *extremely important* were assigned a score of three on that issue; those who identified the issue as *very important* were assigned a score of two on that issue; those who identified the issue as *somewhat important* were assigned a score of one on that issue; and all others were assigned a score of zero. The scores were aggregated to form an index. All those with a total of three or less are the low-importance strata; those with a total of four or five are the medium-importance strata; and those with a total greater than five are the high importance strata.

21. Selective examples show what can transpire when political campaigns fail to sustain the candidate image as strong and competent; for example, Gary Hart, Edmund Muskie, and George Romney quickly come to mind as candidates who failed in precisely this fashion. Personal stability and strength would appear to be minimal requisites for competence.

22. The principal focus of Madison's skeptical attention, in *Federalist Papers*, no. 37, is on the fallibility and partiality of reason; a common con-

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cern in contemporary social science (Greenwald 1980; Marcus and Rahn n.d.; Markus 1986; Nisbett and Ross 1980).

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AGE AND ACTIVE-PASSIVE LEADERSHIP STYLE

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The relationship between age and active-passive leadership style is analyzed with direct observational data describing the behavior of a sample of 24 mayors during issue discussion and debate on their councils over a one-year period of meetings. Activity, measured as a time-based rate of verbal participation, was found to increase with the age of leaders to a high in the midfifties, and to decline for older mayors in their sixties and seventies. Political experience interacted with age to affect activity. Inexperienced mayors displayed a less active style, regardless of age; but experienced mayors in their sixties and seventies had the least active style of all. These findings held up under control for group size, political structure on the councils, and community characteristics.

The Iran-Contra travails of the Reagan presidency raise and illustrate two potentially interrelated problems of leadership. One problem involves the limitations of passive leadership, and the second, the effects of old age upon role performance. Reagan's passive approach to leadership is legend and this style is offered in partial explanation of his failure to monitor and control the activities of his advisors and subordinates more thoroughly. The question was first posed (in the media) with the first Reagan-Mondale debate in 1984, whether a man (at least, this man) in his mid-seventies was physically capable of fully exercising the responsibilities of presidential leadership. Barber (1977) describes the dimensions of "presidential character"—active-passive, positive-negative—as fundamentally fixed early in the political experience of a leader. The interesting question raised by the Reagan case involves the possibility that the active-passive dimension of leadership, regardless of any initial disposition associated with per-

sonality and character, may vary systematically with older age toward less activity in role performance.

Aging and Leadership Style

This argument does not propose that barring serious illness, the physiological effects of aging beyond, say, 65 will turn an active leader into a passive leader; and no doubt there are many active elderly leaders. Rather, the more realistic proposition is that an active leader will become less active, and a passive leader, more passive. A model posing a relationship between age and leadership activity is not alternative, but complementary to Barber's emphasis on personality and character in the prediction of leadership performance.

Active-Passive Leadership

The active-passive dimension of leadership is defined by Barber (1977, 11) in terms of energy investment in job per-

formance. He asks, "How much energy does the man invest in his Presidency?" The concept of activity is not defined abstractly but in concrete physiological terms. Activity involving energy expenditure may be indicated by the frequency of acts of role performance in the task areas that comprise leadership behavior. Although leaders may emphasize different areas of task performance, an underlying question remains of how much effort they put into their work. For any leader, an "activity" dimension of style involves energy investment in job performance.

There is no presumption in this definition of active-passive leadership of any inherent value associated with either active or passive extremes. It is not clear, *a priori*, that passive leaders are somehow poorer or less effective than "active" leaders in their roles. The adaptive value of any particular style should vary systematically with properties of the broader political context. Indeed, it is vital to distinguish between aspects of character and style in role performance and evaluations of outcomes or effectiveness because these are, respectively, independent and dependent variables. It is certainly plausible that a tendency toward less active leadership associated with older age may be compensated by more effective, skillful, and thus efficient leadership for some individuals.

Age

Chronological age is a marker variable indicating in quite relativistic terms the stage of the biological aging process and, less accurately, the social-life stage as well. It is biological aging that may most clearly bear upon leadership style and it should be kept in mind that aging processes occur at differential rates across individuals. Chronological age provides only a crude indicator, not a direct measure, of the process of biological aging; there-

fore a portion of error is built into time-based observations.

Fundamental to the hypothesis is the changing ability of individuals to expend energy in job performance, particularly the bearing of age on arousal, attention span, vocal production and sound processing, sight and visual processing, memory, reaction time, learning rate, physical strength, adaptability to stress, and susceptibility to disease (Birren and Schaie 1985; Botwinick 1978; Fries and Crapo 1981). As Comfort (1979, 16-17) observes, "Vigor declines and disease susceptibility multiplies with increasing age" and "the rate of this increase under the best conditions has a characteristic value in each species and is exponential." He notes that the rate of decline in vigor increases after about 40 in humans, "the end of the plateau of adult vigor" (p. 300). Certainly, biological aging effects should become increasingly pronounced in elite behavior by late middle age (Schubert 1983). One net effect of biological aging in older age must be to constrain physically demanding activity.

Research Design

To explore support for the proposition that aging is associated with less active leadership style, data were utilized from a broader study concerned with age and political decision making in small groups (Schubert, Wiegele, and Hines 1986, 1987). In this study, meetings of 24 municipal councils were observed over a one-year period. Proceedings of meetings were recorded on audio tape and written transcripts were prepared for periods of active discussion and debate. The 24 councils included samples of eight, drawn from three regions of the United States: East, Midwest, and South. These councils governed typically smaller rural and suburban communities, with total populations ranging from 95 to 70,000 in the

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1980 census.¹ Their mode of operation bore little resemblance to the televised melodramatics of big city councils in metropolitan areas and their procedural process and discussion were characteristically informal and relaxed, with substantial spontaneity.

The audio and printed transcript materials provided the basis for coding behavioral events. Specifically, observations were recorded for each speech segment or "speaking turn"—the period an individual holds the floor as speaker—that occurred during council discussions. Approximately 40 thousand turns were coded for the year of meetings across the 24 councils. For this analysis, data were aggregated to the level of the individual over the entire period of observation.

Data on chronological age were collected through personal interviews or, in a few cases where interviews were not granted, through a survey of city clerks. The 24 mayors ranged in age from the midthirties to early seventies about a mean of 54 years. Three of the mayors were in their early seventies, three in their midsixties, and six in their late fifties or early sixties. While these data clearly contain fewer data points for the ages in question than might be desired, they do provide a useful starting point for exploration of the research question because they bracket an age range that extends into "older" age and permit analysis and comparison across the actual age spectrum of political elites.

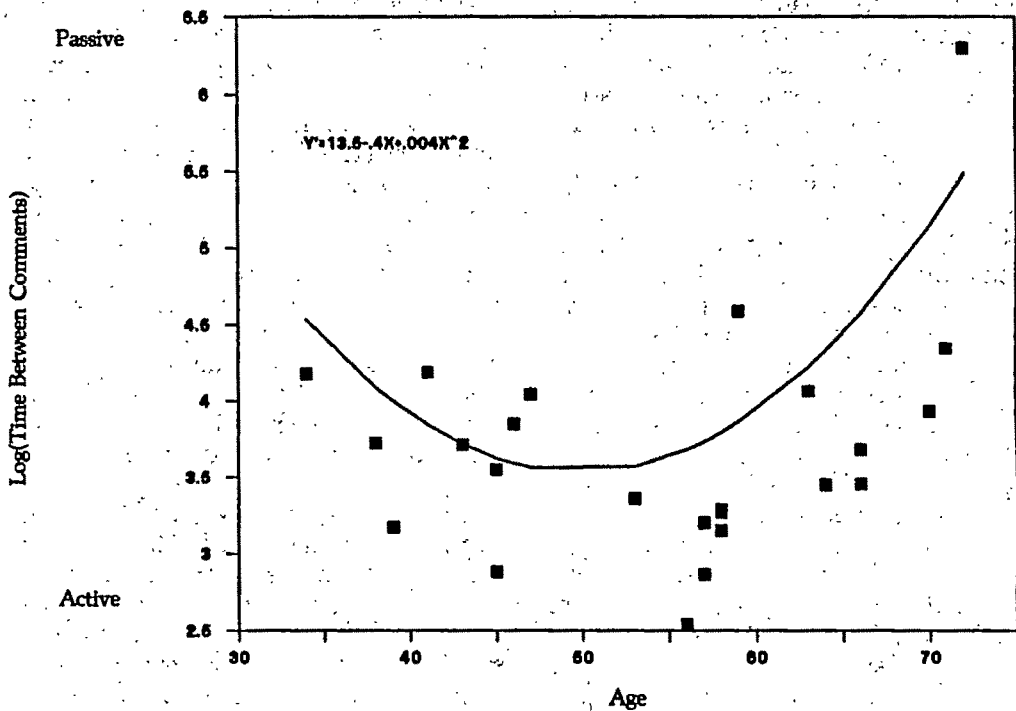
The general concept of leadership activity, defined as energy investment in job performance, was operationalized in terms of verbal activity in the management of group discussion. Because purely procedural comments (e.g., "Is there a second?") were not included in the data, scores describe only discretionary participation. Verbal activity was measured by the mayors' "commitment rate"—that is, total time (in seconds) used by all elected group members speaking during discus-

sion, divided by the total frequency of mayor speaking turns. The resulting scores reveal, on the average, how often mayors spoke during group discussion. The scores range from a low of about once every 20 seconds, indicating high verbal activity, to highs exceeding once a minute, indicating low verbal activity. The comment rate was employed because it provides what is literally a behavioral activity indicator. Measures of relative activity, such as proportion of group comments or total discussion time, would not be appropriate indicators of the dependent variable because they do not describe how much activity occurred in terms that would be comparable across groups.

To guard against the possibility that some mayors might appear to have less activity because they made longer comments, mean length of turn was analyzed as a control variable and found to have no significant effects upon the findings. With respect to reliability, it should be noted that observers recorded the identity of speakers on a pocket computer, when they began speaking during the meetings, and these data were subsequently verified when coders listened to the audio tape in the final coding process. Speaking turns were also timed from the audio tape by computer.

Although managing meetings is but a small part of the leadership tasks of those in leadership roles, it is nonetheless a common behavioral setting across different levels and types of leadership and, in committee decision making contexts, it is an important task. Whether or not verbal activity in leadership during meetings bears upon leadership style in other task areas of leader performance is an empirical question that awaits systematic study. Without belaboring the obvious, actively chairing the discussion process in a small decision making group over the course of a two-to-four-hour meeting entails substantial energy expenditure. Active partic-

Figure 1. Age and Verbal Activity



Note: $R^2 = .4179$; $F = 7.6$, $p < .003$.

ipation requires apt attention to virtually all aspects of proceedings. On small councils in local government, mayors often perform both task and socioemotional leadership roles and energy is expended both in attentive listening and in speaking. It is reasonable to assume that a leader who speaks on the average once every 20 seconds is investing a good deal of energy in job performance.

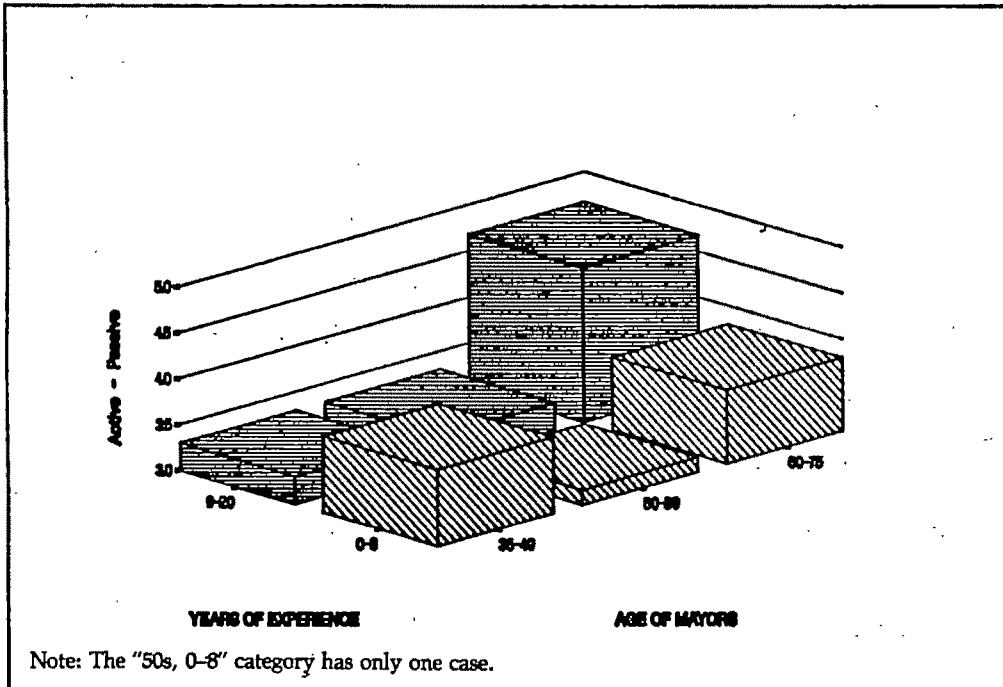
Findings

Because the comment rate scores revealed a pronounced positive skew, including one extremely inactive mayor, these data were transformed to natural logs. Figure 1 presents the scatter diagram for the relationship of age and logged comment rate. The simple bivariate corre-

lations were .34 for the raw rate and .27 for the logged rate, describing a pattern of less verbal activity, the older the mayor. The plot also reveals a pronounced curvilinearity in the relationship. To model this effect a second-order polynomial function was fit to the data, using a nonlinear least squares estimation procedure, and it accounted for a statistically significant 42% of the variance in verbal activity. The pattern in these data is clear. Verbal activity is initially moderate and increases with age for mayors in their thirties and early forties. It is high and stable for mayors in their midforties through midfifties, but declines directly with greater age for those in their late fifties on. The pattern for younger mayors, which is less pronounced, is not predictable from the theoretical model. It is plausible that the

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Figure 2. Age, Experience and Logged Comment Rate



initial increase in activity associated with older age for younger mayors reflects the increasing social status, self-assurance, and political experience associated with maturation into middle age. These factors should not diminish in importance with increasingly older age; yet a positive slope is quite clear from 55 on. Before accepting support for the research hypothesis, it is necessary to consider alternative explanations of the decline in activity associated with older age.

One possibility is that older mayors were less active verbally because they were more experienced and therefore more skillful in managing meetings. It is quite plausible that the loss of vitality in older age is offset or accommodated to some extent by more efficient use of energy in task performance. In a study of state legislators, Fengler (1980) found

that, while attendance and legislative activity declined with age, success (rate of passed to initiated bills) increased with age. Thus older mayors might speak less often but with greater effect upon the group process. As with the effect of biological aging, this "experience" effect would be revealed in declining activity, regardless of any initial, baseline tendency toward "active" or "passive" leadership.

To explore this experience hypothesis, data (acquired during the personal interviews) were developed on each mayor's number of years of prior service on the council, either as mayor or as elected member. Among these 24 mayors, the correlation of age and experience was quite weak at .19 and the scatter diagram revealed no pattern. For 7 mayors over 60, 2 had less than 3 years' experience, 3

Table 1. Predicting Active-Passive Leadership

Equation	Predictor Variable	Regression Coefficient	Standard Error	Beta Coefficient	Two-Tail Probability
$R^2 = .42$; SEE = .598 $F = 7.595, p < .003$	constant	13.480	3.043	.00	.000
	age	-.399	.117	-5.96	.003
	age (squared)	.004	.001	6.24	.002
$R^2 = .629$; SEE = .516 $F = 7.624, p < .001$	constant	14.360	2.933	.00	.000
	age	-.391	.115	-5.55	.003
	age (squared)	.003	.001	5.24	.006
	experience	-.320	.124	-2.55	.019
	age * experience	-.006	.002	2.91	.011
$R^2 = .654$; SEE = .565 $F = 3.31, p < .024$	constant	12.792	3.700	.00	.004
	age	-.342	.136	-4.84	.025
	age (squared)	.003	.001	4.54	.037
	experience	-.334	.141	-2.67	.033
	age * experience	.006	.002	3.00	.021
	population	-.000	.000	-.02	.920
	% rural	.001	.003	.09	.689
	size	.059	.125	.10	.642
	political structure	-.116	.200	-.11	.572
$R^2 = .186$; SEE = .744 $F = 1.447, p < .261$	constant	3.030	.854	.00	.002
	age	.021	.015	.30	.171
	experience	.004	.027	.03	.894
	political structure	-.360	.221	-.34	.120

Note: The dependent variable is the natural logarithm of the time in seconds between comments: lower scores describe a more active style.

had 5 to 10 years, and 2 had 10 to 16. Moreover, experience and the logged comment rate were virtually independent ($r = .03$), and this did not change when experience was added into the least squares equation with the age variables. However, investigation of a possible interaction effect of age and experience on activity yielded intriguing results. Figure 2 illustrates the two-way interaction by plotting the mean comment rate by experience, cut at the median, and age group. Although older mayors were less active overall than others, experienced older mayors were least active of all. In addition, the passivity of younger mayors noted above was associated with inexperience. Experienced younger mayors were as active as middle-aged mayors, and in both of these groups experience was manifest in an energy-intensive, not energy-conserving, leadership style.

Regression analysis with multiplicative terms was employed to estimate the influence of the interaction of age and experience on behavior. The results presented in Table 1 reveal that with the interaction term in the equation, the variance predicted improved from 46% to 63%, and all terms had significant effects. The polynomial effects of chronological age were most powerful, but experience and its interaction with age made notable contributions to the prediction. It should be noted that the partial effects for experience alone indicate more, not less, activity with greater experience. It is the interaction term that predicts to passive style. The standard errors for predictor coefficients are acceptable and the residual plots revealed no abnormalities. There was one apparent outlier (see Figure 1)—an elderly mayor with substantial experience who rarely spoke during his council's

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discussions. With his activity score truncated to 4.5, the regression was rerun with the effect of marginally decreasing the variance explained and the extent to which coefficients passed levels of significance.

The effects of three other intervening variables were considered in the analysis. Partisan *political structure* within groups was considered based on the hypothesis that management of groups with competing party blocs would require greater verbal activity by mayors. Ordinal measurement distinctions were introduced for (1) nonpartisan and one-party councils, (2) councils with 80% or more members from one party, and (3) those with weaker party majorities. *Size of group*, which ranged from five through nine including the mayor, was also considered because group size affects the opportunities for participation, albeit more for members than mayors. Finally, to explore the possibility that leadership style would be influenced by the complexity of issues and the informality of group processes occurring differentially in smaller rural and larger suburban community settings, 1980 census data on *total* and *percentage rural population* were considered.

Results from a multiple regression analysis that included age, experience, and the other variables are reported in Table 1. None of the added variables make even a modest contribution to the prediction of mayoral activity. However, it is interesting to observe from the final equation in Table 1 that without the multiplicative terms, partisan division is moderately associated with more active style and to note that total population is correlated with division in political structure ($r = .44$). The fact that these environmental factors washed out in the more comprehensive model indicates a causal structure in which environmental factors may bear upon the characteristics of office holders, but individual factors explain leadership style.

It may be clear from Figure 2 that the reason why the interaction term is so important is that inexperienced younger (mean = 3.83, $s = .4$, $n = 6$) and older (mean = 3.8, $s = .43$, $n = 4$) mayors were similarly passive and the real decline in activity with older age occurred among those with substantial experience. Thus inexperience is associated with less activity regardless of age. Among older mayors, why were those with experience even less active? Of all the variables, the only major difference between these two categories was years of experience—13.7 to 4 years on the average. Among experienced mayors, why is the passive style of older mayors so different from the active style in the younger categories? Mean years of experience varies from 12 to 14.7 to 13.7 from young to old, and differences did not emerge on the other variables either. What separates these categories is a decade of aging—a mean of 56.9 years in the middle group compared to 67 years in the older group. It is plausible that the answers lie in the nature of experience and the nature of aging in older age. The reason why experience may differentiate behavior is that it allows flexibility in leadership strategies. The reason why age differentiates behavior among experienced leaders is that it affects choices among strategies available to leaders.

Discussion

The frequent citations of Barber's active-passive, positive-negative typology of leaders in both its presidential (1977) and legislative (1965) applications, reveal at least its heuristic value in describing important qualities of leadership. Present concern is limited to the active-passive dimension because there is no apparent theoretical connection between age and the positive-negative dimension. Despite the heuristic value of the active-passive

distinction, questions of its theoretical significance may reasonably be raised.

There is a tendency to confuse active-passive style with effectiveness in role performance, with success in policy making, and with evaluations of "strong" and "weak" leadership. Although activity is conceptually independent of such dependent variables, its broader theoretical significance may ultimately be demonstrated by research into its effects upon such outcome variables. Just prior to the outbreak of information on the administration's Iran-Contra policies, for instance, a major national business magazine ran a cover story posing President Reagan's nominally passive leadership style as a model for the corporate United States. Up to this point, a low-energy leadership style—that was revealed in avoidance of detail, a relaxed approach to informational homework, even sporadic attentiveness and sporadic verbal participation in small group meetings—was widely (or plausibly) understood as positively adaptive in the president's role performance. The Reagan case clearly isolates and distinguishes the activity dimension of leadership from outcome and effectiveness questions and surely begs investigation of the theoretical relationship between style and outcomes.

Although it may be clear that activity in presidential leadership could have important political implications, the question may still be raised whether leaders' activity in managing committee-style decision making groups bears on significant aspects of the broader question. Given the pervasiveness of the face-to-face small group setting across the various levels and structures of government and politics, leader activity in group deliberation processes would appear potentially quite important for a general understanding of political decision making. Of course, it is conjectural whether leadership activity in the management of meetings is related with other tasks associated

with executive role performance. It is reasonable to hypothesize that leaders who are quite active in meetings maintain this style across task areas. Whether passive leaders in meetings tend to be passive across the board appears more problematic and warrants further research.

One important limitation of this study is that the data base is not longitudinal. Effects are hypothesized to occur over a protracted period of time in an individual's life. An optimal data set might include observations on individual elites (e.g., judges, presidents, governors) at points encompassing 10 to 20 years of continuous service in office, beginning by at least age 55. Thus the findings could reflect cohort, rather than aging effects upon behavior. A cohort explanation would imply that age cohorts differed systematically in some general disposition toward energy investment in job performance. Given that cohort effects are manifest through common attitudes and shared values and that the relevant attitudes for "energy investment in job performance" would encompass the so-called work ethic, it might be expected that older leaders in these data, coming of age in the 1930s and early 1940s, would display more, rather than less activity in leadership performance. Of course, cohort effects cannot be ruled out a priori. For a more general model of leader activity, it would be appropriate to consider cohort effects along with social and biological factors of aging.

Conclusion

In a recent review of the literature and state of knowledge on political leadership, Hermann (1986, 185) comments, "Of all the ingredients of political leadership, behavior has received the least attention." She goes on to note the complex problems that empirical research on leadership be-

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havior must confront, and certainly the active-passive dimension is but one small piece of a much larger puzzle. However, the active-passive dimension, narrowly conceived and behaviorally defined, may provide a fundamental baseline from which to pursue more comprehensive empirical inquiry into leadership behavior.

We do tend to rely heavily upon the activity dimension in evaluating role behavior of people both in and out of official capacity, and elderly political elites are particularly vulnerable in this respect. Journalists bring it to our attention, increasing in visual form, when elderly senators tend to fall asleep during committee sessions or a president dozes off during meetings with other heads of state or international elites. Exacting attention to detail, factual recall, the capacity to absorb reams of technical material in late-night work sessions, vocal performance in face-to-face settings, or even lengthy televised debates are all task areas subject to the effects of biological aging. If we knew more about the effects of biological aging upon behavior, we might develop more reasonable expectations and reasoned evaluations of leader performance. Specific accommodations might be structured with respect to the length and timing of physiologically demanding task performance. Indeed, we might develop more realistic expectations of the various qualities of leadership (both costly and beneficial) likely to be associated with elderly elites.

The effects of aging on the active-passive dimension of leadership style were researched through direct observation of behavior for a sample of 24 mayors. The findings reveal that aging may have very important effects upon leadership, directly and through interaction with other political variables. It is quite reasonable to propose that the more encompassing the daily demands involved in a leadership role, the stronger the effects of aging on activity. Empirical effects should be

weakest for part-time mayors of small communities and much stronger for governors and presidents. Of course, there are clear limits to generalization from these findings: the number of cases is small, the data are cross-sectional, and more comprehensive measurement of active-passive style would be desirable. Despite these limitations, the findings make excellent sense in theoretical terms and invite replication through longitudinal research design.

Notes

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1. The only systematic bias in the selection of these councils was to include councils in each regional area with either older or younger age structures (i.e., median ages exceeding 55 or below 44). The outcome was 8 older, 6 younger, and 10 with "mixed" age structures. In both social, economic, and demographic characteristics of communities and sex, age, and occupational characteristics of council members, data were analyzed revealing that the sample councils and communities did not differ significantly from the population of the broader regions from which they were drawn (Schubert, Wiegele, and Hines 1986).

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IDEOLOGICAL CONSISTENCY AS A COLLECTIVE PHENOMENON

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Researchers ordinarily consider ideological consistency to be a characteristic of individuals; groups are considered to be ideological only if they are composed of ideologically oriented individuals. We show how a group as a whole can be characterized as exhibiting an ideological basis for its preferences even though many, or even most, of its members have preferences that are inconsistent with the supposed uni-dimensional ideological continuum. As an illustration, we show that the United States electorate of 1980 had collective preferences among the candidates Kennedy, Carter, Ford, and Reagan as if these preferences reflected an underlying left-right dimension among these candidates, despite the fact that a high proportion of individual voters had preferences among these candidates that did not fit the left-right dimension. In general, we show reasons why collectivities are likely to be more ideologically consistent than are the individuals composing them.

Despite the classic sociological recognition that the social whole is more than the sum of its parts (e.g., by Durkheim), the analysis of ideological consistency in collectivities has focused upon the ideological consistency of the individuals who compose them. For example, the well-known fact that individual voters in the United States by and large do not make choices in terms of a consistent left-right ideology (Axelrod 1967; Niemi and Weisberg 1984, 319-28 and references cited therein) has been interpreted to mean that U.S. society is not ideological in its choices.

We propose that "ideological consistency" may properly be considered to characterize collectivities as well as individuals. By choices being "ideologically consistent," we mean that group decisions

are made as if the options were arrayed on a left-right continuum with voters choosing the alternative closest to their own preferences. In other words, when we speak of collective choices as being "ideological" or "ideologically consistent," we mean (following social choice terminology) that the preference structure is *the same* as that which would occur if voters had "single-peaked" preferences (Black 1958) along a single left-right dimension. We recognize that the term *ideology* has a plethora of meanings (see, e.g., Williams 1983 s.v. "ideology"), but the narrow meaning we give it is one with a long history of use and is directly relevant to the important empirical question of whether or not the U.S. electorate (and various subsets) can properly be characterized as responding to choices in left-right terms.

We demonstrate that even groups composed of individuals most of whose preferences are not ideologically consistent may be found to exhibit collective ideological consistency. Our central theoretical point is that there is a sensible notion of a collective ideology that is different from simply counting the proportion of individuals with ideologically consistent preferences or with some form of ideological self-identification, as is standard in the political science literature (see, e.g., Conover and Feldman 1984; Fleishman 1986; Hamill and Lodge 1986). We shall demonstrate that it is a "fallacy of composition" to believe that collective decision making will be ideological *only* when all or most members of the collectivity, as individuals, are ideological in their preference structure.

Berelson and colleagues (1954, 312) suggested that certain nonreducible properties of groups might be indispensable to the smooth functioning of the political decision-making process:

Individual voters today seem to be unable to satisfy the requirements for a democratic system of government outlined by political theorists. But the system of democracy does meet certain requirements for a going political organization. The individual members may not meet all the standards, but the whole nevertheless survives and grows. This suggests that where the classic [political] theory is defective is in its concentration on the individual citizen. What are undervalued are certain collective properties that reside in the electorate as a whole and in the political and social system in which it functions.

We believe that *collective* ideological consistency is one property that may be important for the stability and coherence of a political system, because a political system that makes consistent and ideologically predictable choices may be more easily accepted as legitimate than one that appears inconsistent and unpredictable. We believe that collective ideological consistency is commonly found; that is, groups often possess the well-ordered preferences that make it possible to treat a

collectivity as if it had coherent ideological preferences.

The coherence of group ideological preferences has important implications; for instance, many political groups (both manifest and latent) can be treated as if they were single entities by actors in the political process. Political candidates can choose positions in response to "group" preferences even though many or most members of the group are not strictly ideological. Similarly, parties can seek out candidates who reflect the "views" of the parties' constituencies. Thus, we can see how ideologically oriented representatives can nonetheless "faithfully represent" the view of an electorate primarily composed of nonideological voters (Feld and Grofman 1986a) and how newspapers and commentaries might sensibly talk about ideological shifts in the electorate, even though many or most voters never had coherent ideological views to start with.

Conceptualizing Individual versus Collective Ideological Consistency

The standard approaches to defining ideology in groups are methodologically individualistic in perspective, that is, they treat a collectivity as being ideological in the manner that, and to the extent that, its individual members are. For example, in the usual Guttman scaling approach (see Weisberg 1972 for a review), a set of choices can be said to conform to an underlying ideological basis of choice if there is an ordering of items such that each voter can be assigned a position on a continuum that completely determines his or her entire set of choices; essentially all individuals must be ideologically consistent for their choices to "Guttman scale."

Similarly, factor analysis, one common approach to ideological consistency, operates on a matrix of bivariate correlations (Harmon 1976; Weisberg 1974) that indicate the extent to which individuals

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have views that are coherent across choices. Ideological consistency is indicated by the amount of the individual variance that can be attributed to a single (or at most a few) factors.

Common sense might seem to suggest that there is no other way to define a group as being unidimensional than by saying that choice in accord with some single dimension is characteristic of members of the group *as individuals*. Yet we will show that it is perfectly possible to talk intelligibly about a group being ideological in its choices even when most of the group's members lack an ideological basis for their preferences. In fact, the converse is also possible; a group may fail to be ideological in its choices even if almost all of the individuals in the group make perfectly ideologically based choices. While there is some tendency for the extent of ideological consistency of individuals to affect the likelihood of collective ideological consistency (and the specific case of total ideological consistency of individuals necessarily results in group consistency), each form of ideological consistency can be high when the other is low (Feld and Grofman 1986b; Niemi 1969). We now turn to a precise statement of what we mean by individual and collective ideological consistency; then we consider some empirical evidence on ideological consistency in the U.S. electorate.

Definitions of Ideological Consistency in Individuals and Collectives

For a particular set of choices (alternatives) arrayed along a unidimensional continuum (e.g., left-right), we shall say that an *individual* has an ideological preference ordering with respect to that continuum if the individual has a most-desired alternative and prefers each of the other alternatives less as they are further from the "ideal" alternative in either direction. Such ideological preference

orderings have been called "single-peaked" preference orderings (Arrow 1963; Black 1958), because they suggest that each individual has a utility function that is peaked at the ideal point and declines in both directions from that peak.¹ Once we specify such a unidimensional continuum and identify a set of alternatives ordered along it, then for every individual in a society we can in principle determine if that individual has preferences that are single-peaked with respect to the given continuum.

For the rest of this discussion, we will assume that a particular unidimensional continuum has been specified. A preference ordering will be said to be ideological if it is single-peaked with respect to the particular continuum that has been specified. Whenever we refer to a collective ordering we mean the preferences of the majority.

For example, consider the three alternatives A, B, and C along a continuum in that order. Each individual can be thought to have a preference ordering over that set of three alternatives; four of the possible orderings (ABC, BAC, BCA, and CAB) are consistent with single-peakedness for this continuum, and the other two possible orderings (ACB and CBA) are ideologically inconsistent with this continuum. To see that ACB is ideologically inconsistent with the continuum ABC note that if the individual had an ideal point at A, he or she must prefer B to C, since B is closer to A than is C along the ABC continuum. The argument that shows CAB to be inconsistent is analogous.

Suppose that 17 individuals all have single-peaked preference orderings: 8 have ABC, 5 have BCA, and 4 have CBA. Of the 17 voters, a majority (13) prefer B over C, a majority (9) prefer B over A, and a majority (9) prefer C over A. It is clear that B is the median of the voter ideal points; B is majority-preferred to both A and C. Furthermore, the group

majority preference, which is B preferred to C preferred to A, is consistent with a hypothetical group utility function with ideal point at B and next highest utility at C. Such a function is single-peaked with respect to the continuum ABC.

Whenever *all* of the individuals have single-peaked preferences with respect to some continuum, it is well known that there is an alternative that is preferred by the majority of the individuals in the group to each and every one of the other alternatives. Furthermore this majority winner corresponds to the ideal alternative of the median voter in the group (Black 1958); and the majority preferences ordering of the group is itself single-peaked with respect to the underlying continuum (Arrow 1963).² Analogous results occur if *all* individuals have single-troughed preference ordering or if all individuals have what are called polarized preferences over every triple of alternatives (see Feld and Grofman 1986b; Plott 1976; Sen 1966), but we shall neglect these essentially technical complications and focus on single-peakedness.

When all individuals have single-peaked preference orderings the process of collective decision making is dramatically simplified. As long as all voters have such ideological preference orderings there cannot be any cyclical majorities (e.g., where A is majority-preferred to B, which is majority-preferred to C, which in turn is majority-preferred to A); also, the majority choice is consistent with all of the requirements of an ideal decision rule as set forth by Arrow (1963) in his classic consideration of social choice. However, while there are many situations where single-peaked preferences are common, there are few situations where everyone has single-peaked preferences.

Researchers have shown that a higher proportion of individuals with single-peaked preferences increases the probability that a group will have single-peaked preferences (Niemi 1969; Niemi and

Wright 1986); however, there is no necessary connection between these two things. If there is even one individual with non-single-peaked preferences, then the group may have non-single-peaked preferences and there may be cyclical majorities. In the previous example, suppose that one of the voters with CBA preferences had changed to CAB preferences instead. There would then be a majority (9) who preferred A to B, a majority (13) that still preferred B to C, and a majority (9) that still preferred C to A. While there would still be 16 of the 17 individuals with single-peaked preference orderings, there would be a majority cycle, preferring A to B to C but C to A. More generally, as long as there are any individuals in the collectivity whose preference ordering is inconsistent with the posited ideological dimension, there is the possibility that the group will have a preference ordering that is inconsistent with the ideological continuum and even that the group will have no well-defined (i.e., transitive) preference ordering among alternatives.

Ideologically Ordered Margins in Collectivities

Collectivities indicate the *strengths* of their preferences by the sizes of their vote margins (see Feld and Grossman 1984). To the extent that collectivities are ideological, this ideology should be reflected by ideologically consistent regularities among the various vote margins between pairs of mutually exclusive alternatives, as well as by a single-peaked majority preference ordering at the aggregate level. Feld and Grofman (1986a, 103) define what it means for vote margins to be ideologically consistent; we expand upon that definition, consider how such ideologically ordered margins may arise, and specify an important new result about the aggregation of subgroups with ideologically ordered margins.

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If all voters have single-peaked preference orderings over the continuum ABC , then everyone who prefers A to B must also prefer A to C . Therefore, if everyone in a group had single-peaked preferences, the total number of individuals voting for A over C would have to be at least as great as the number voting for A over B . Similarly, single-peaked preference orderings over the continuum ABC imply that everyone who prefers A over C also prefers B over C . Hence, if everyone in the group had single-peaked preferences, the total voting for B over C would be at least as great as the number voting for A over C . Thus the voting margins would have an ordering

$$m(A,B) \leq m(A,C) \leq m(B,C)$$

This can be illustrated in a matrix as shown below:

Alternatives	Alternatives		
	A	B	C
A	—	$m(A,B)$	$m(A,C)$
B		—	$m(B,C)$
C			—

The defining characteristic of this matrix of *ideologically ordered margins* is that in the area of the matrix above the diagonal the margins below and to the right of any cell are always at least as great as the margin in that cell. In other words, margins increase (or stay the same) in every row as we read from left to right and increase (or stay the same) as we move down in any column toward the main diagonal. Note that the ordering is the same whether the margins are expressed in terms of the absolute number voting for the row alternative over the column alternative or in terms of the vote margins by looking at the difference in number of voters between those voting for the row alternative over the column alternative and those voting for the column alternative over the row alternative. Also, it makes no difference if we

express the matrix of margins in terms of absolute numbers or in terms of percentages.

For more than three alternatives, the implications of all individuals in the group having single-peaked preferences are the same as for the three alternative cases shown in the matrix above, that is, the alternatives can be represented in a matrix with the alternatives ordered across and down the side. The margins above the diagonal will follow the same pattern as in the matrix above, namely, increasing (or at least not decreasing) as one goes to the right along any row and as one goes down any column.

When margins satisfy the ideological margins condition with respect to a particular continuum, it can be seen that the majority preference ordering of the group as a whole must be single-peaked with respect to that continuum. This follows from the fact that there must be a first alternative, i , that is majority-preferred to the immediately following alternative, $i + 1$ (i.e., i is the first entry in the row whose margin over the alternative $i + 1$ is greater than a majority). Under these conditions the alternative before it, $i - 1$, must get less than a majority when paired against i (since i is the first to be majority-preferred to the next one, $i - 1$ must not be majority preferred to its following alternative); all alternatives before $i - 1$ (all margins above it) must have even lower margins (less than majorities). At the same time, since i receives a majority against the next alternative, $i + 1$, it must receive at least as great a margin (also a majority) against all following alternatives (all margins to the right). Thus, alternative i is majority-preferred to all alternatives before it and after it in the matrix. In like manner, with i deleted, we can find the next most preferred alternative in the majority preference ordering.

Ideological margins may arise even with many non-single-peaked voters. Indeed, in general, even situations where

single-peaked orderings are relatively rare may still give rise to ideologically ordered margins, that is, margins of the sort that would arise if *all* individuals had single-peaked preferences. A simple example can illustrate this point.

Consider 17 voters such that there are 5 ABC, 2 CAB, 4 BAC, and 6 BCA preference orderings; the margins are ideologically ordered with respect to the continuum ABC, as shown in the matrix below, even though only 10 of the 17 individuals, those with the last two preference orderings, have single-peaked preferences with respect to the continuum ABC. Since 4 of the 6 possible preference orderings are single-peaked, this example represents a lower proportion of single-peaked preferences with respect to ABC than would be expected by chance alone (10 out of 17 vs. 4 out of 6); yet as shown in the matrix below, the ordering on margins is exactly the same as could occur if *all* voters were single-peaked with respect to ABC. Eleven voters have single-peaked preference with respect to the continuum BCA, but the group preference ordering BAC is not single-peaked with respect to BCA.

Alternatives	Alternatives		
	A	B	C
A	—	7	9
B		—	10
C			—

Note that if there are only three alternatives, ideological margins require that a majority of voters have single-peaked preferences, but for larger numbers of alternatives there may be ideological margins even if *no* individual has single-peaked preferences. Also note that (if we neglect ties) margins can only be ideological with respect to a single underlying continuum. As illustrated by the above example, the continuum along which the largest number of individuals will be single-peaked need not be the same continuum as that over which the group has

ideological margins. In general, however, we do not expect to find many empirical examples of such a discrepancy between a group-based and an individual ordering-based perspective on what ordering is most characteristic of the group. The context for the individuals in a group is likely to produce tendencies toward single-peakedness on some particular continuum, and that continuum is likely to be the same one which forms the basis for the group's ability to satisfy the ideological margins condition.

The ideological margins condition is important because if it is satisfied for a set of subgroups composing a society, it must also be satisfied for the society as a whole. This is not true for single-peakedness; if we add several subgroups together, each of which has a single-peaked ordering (but without ideological margins), there is no guarantee that the combined group will have a single-peaked ordering.

The example below shows two collectivities that each have single-peaked preferences with respect to some underlying continuum (without ideological margins) that do not combine to produce a collective transitive ordering that is single-peaked with respect to that same underlying continuum. Consider three candidates on the continuum ABC:

Set X below has a hundred people and has majority preferences ordered ABC, which are single-peaked preferences over the continuum ABC, but X does not satisfy the ideological margins condition.

	A	B	C
A	—	60	55
B		—	51
C			—

Set Y below has a hundred people and single-peaked preferences CBA over the same continuum, ABC, but again does not satisfy the ideological margins condition.

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	A	B	C
A	—	45	42
B		—	40
C			—

Sets X and Y combined have two hundred people and majority preference ordering CAB, as shown below.

	A	B	C
A	—	105	97
B		—	91
C			—

The preference ordering of the combined set X and Y is not single-peaked with respect to the continuum ABC. Thus, without ideological margins, the fact that subgroups of a collectivity each have single-peaked preferences does not ensure that the combined group will have single-peaked preferences.

On the other hand, it is easy to see that if we combine two subsets, each of which has ideological margins, the resulting combined group must have ideological margins; clearly, if each of the matrices has the margins ordered from left to right and top to bottom, summing them results in the same ordering. Furthermore, if each of the subsets has mostly ideologically ordered margins and the departures from ideologically ordered margins in each subset are different, combining the subsets may result in margins that are even closer to ideological ordering than was true for the subsets themselves.

We are not claiming that collectivities will inevitably have ideologically ordered margins. We do claim, however, that ideologically ordered margins will be more common than one might think by looking at the bewildering diversity among *individual* preferences. Furthermore, while many or even most of the individuals in a collectivity may have quite nonideological preferences, we expect that many collectivities will at least

approximate the ideological margins condition and will have single-peaked preferences.

We believe that individuals do not make choices consistent with an underlying ideological continuum as often as collectivities do. The aggregate ideological consistency might arise in several ways. One possibility is that there is a relatively ideological elite whose preferences, combined with the self-counteracting "noise" of the nonideological masses, determine the group preferences. Another possibility is that there is a diffuse ideological tendency among a large proportion of the electorate that is sufficient to generate ideology in the aggregate; some individuals might have single-peaked preferences over some subsets of the alternatives while other individuals have single-peaked preferences over other subsets (cf. Feld and Grofman 1986b).

The Extent of Ideology in Margins

In the previous section, we showed that if subgroups have ideological margins, then the entire group necessarily has ideological margins and consequently must have a transitive preference ordering that is single-peaked. In addition, we suggested that if the margins of subgroups closely approximate ideological margins, then it makes it likely that the group as a whole will approximate ideological margins and consequently have single-peaked preferences. Here we will make the notion of "closeness" to ideological margins more precise.

To measure this notion of "strength" of ideological margins we simply measure the average difference between margins. If there are three alternatives, then there are three margins and three margin comparisons ($AC - AB$; $BC - AC$; $BC - AB$).³ For three alternatives, the *average margin difference* is given by $(AC - AB + BC - AC + BC - AB)/6$, which reduces to

$(BC - AB)/3$. If margins satisfy the ideological margins conditions, each of these pairwise margins comparisons should be nonnegative; and even if there is "noise" the average difference in margins should be nonnegative.

If margins rapidly increase in each row or column as we move to the right, away from the main diagonal, or down, toward the main diagonal in some subgroup, then even when this subgroup is combined with other subgroups the pattern of ideological margins is likely to be maintained. The ideologically ordered pattern will tend to stand out over any random variations found in the other subgroups with which it might be combined. (Of course, any subsets drawn randomly from a group with "strong" ideological margins are likely to have ideological margins themselves.)

Recall that ideological margins are ordered so that they are nondecreasing as one moves to the right and down the portion of the margins matrix above the main diagonal. When several subgroups are combined, it should be clear that the margins for the entire group are merely the weighted average of the margins for the subgroups (weighted by the size of the subgroups). Thus if one or another subgroup has one pair of margins slightly out of ideological order (say, 3% in the wrong direction), then when it is combined with other subgroups having this same pair of margins in the right direction, the overall group will have this pair ideologically ordered. So even if some subgroups have slight deviations from ideological margins, there may be ideological margins when subgroups are combined.

Even if the collectivity as a whole does not have strictly ideological margins, it will still have a transitive, single-peaked preference ordering so long as a margin above 50% is not followed (to the right or down) by a margin that is less than 50%. Thus as long as the few particular aggregate deviations from ideological ordering

are not in this crucial region (around 50%) in the group as a whole, they will not interfere with the overall single-peaked collective preference ordering.

In the next section, we provide some illustrative empirical data about the extent to which a population and various subsets satisfy the ideological margins condition. Specifically, we analyze the preferences of U.S. society as a whole and a variety of subgroups within it vis-à-vis the top four candidates for president (Kennedy, Carter, Ford, and Reagan) prior to the 1980 elections. We find that most subsets approximately satisfy the ideological margins conditions with respect to the left-right continuum *KCFR*, which would be a left-right ordering by politically sophisticated observers. Moreover, as hypothesized, we find that the society as a whole, as well as virtually all its subsets, had (majority) preferences that were single-peaked with respect to this continuum, despite the fact that only barely more than half of the *individuals* in the society had single-peaked preferences with respect to this continuum *KCFR*.

An Empirical Example of Ideological Consistency of Groups

In January 1980 individual respondents were asked by National Election Studies (NES) interviewers to indicate their feelings towards each of the presidential candidates in the 1980 election by indicating a number from 0 to 100 (where 100 represents the most positive feelings). By using the feeling thermometer rating as a surrogate for utility (Feld and Grofman 1986b; Niemi and Wright 1986; Weisberg 1974; Weisberg and Grofman 1981), these data allow us to determine each voter's preference ordering over these candidates. The NES survey included a long list of candidates; for the present purposes, we confine our attention to the four candidates

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Table 1. Margins of Preference among 1980 Presidential Candidates

Candidates	Candidates			
	Kennedy	Carter	Ford	Reagan
Kennedy	—	38.0	38.0	41.5
Carter		—	48.3	53.0
Ford			—	53.7
Reagan				—

Note: Entries are the percentage preferring the row alternative to the column alternative.

Source: NES 1980 candidate thermometer data ($N = 1,481$).

on the list who were best known: Carter, Kennedy, Ford and Reagan. Most respondents indicated that they knew of each of these candidates and had preferences among them.

Every one of the 24 possible linear preference orderings over these four alternatives was represented in the NES sample of voters. The eight preference orderings that were single-peaked over the continuum KCFR tended to be somewhat more common than each of the 16 possible non-single-peaked preference orderings; but overall, almost half (47%) of the preference orderings are *not consistent* with single-peakedness on the posited left-right continuum. No ordering is shared by more than 12% of the voters. The most common are RFCK (11.2%), FRCK (10.7%), and RFKC (7.9%), the first two of which are single-peaked; the least frequent orderings were RKCF (.7%), KFRC (1.2%), and RCKF (1.3%), none of which are single-peaked.

Despite the large proportion of non-single-peaked preference orderings, the margins of pairwise preferences conform to the ordering expected to arise from the situation where *all* voters have single-peaked preferences. Table 1 shows the margins of pairwise preference for the entire sample of individuals who indicated feelings about all of the candidates.

Table 1 illustrates how a society with a very large proportion of non-single-peaked preference orderings can, none-

theless, give rise to ideological margins like those that would occur if the entire group had single-peaked preference orderings. Even though a different majority of voters is on the winning side in each pairwise choice among alternatives, the majority of respondents in the NES sample have preference ordering FCRK, which is single-peaked with respect to the KCFR left-right ordering. Conformity of these margins to the requirements imposed by ideological margins would obviously be unlikely to occur by chance. Even though a near majority of those respondents in 1980 had preference orderings inconsistent with the underlying ideological continuum, the group margins still perfectly reflect that continuum.

We believe that this aggregate ideology arises from the fact that there is widespread recognition of the ideological continuum. In the next section, we show that there was general agreement on the placement of these candidates and that subsets with a particular self-identified position had preferences and margins consistent with that ideological position. Niemi and Wright (1986), analyzing the same data, find, as did we, that the society as a whole and various subsets of it exhibit far fewer majority cycles than might be theoretically expected. Niemi and Wright (1986) also found that the absence of cycles could not be attributed to the proportion of individuals having single-peaked preferences and concluded that ideological consis-

tency was not an important factor underlying the transitivity of group preferences. We are suggesting that the proportion of individuals with single-peaked preferences does not measure the importance of an ideological continuum to a group. We agree, however, with Niemi and Wright's suggestion that widespread agreement on the goodness or badness of particular candidates also contributes to the absence of majority cycles. This line of approach is also discussed in Feld and Grofman (1986b) and is one that we have been investigating.

Ideological Self-Identified Subsets

Respondents in the January 1980 NES Election Survey were asked to identify their own position and the position of each candidate on a seven-point scale ranging from extremely liberal (1) to extremely conservative (7). The frequency distribution of respondent ideological self-identification is clustered around the "moderate" responses, with some tendency toward conservatism. We then analyzed the perceived positions of the candidates from the perspectives of the subsets of individuals in each ideological self-placement category. Due to the relatively small number of respondents in the end categories, in our analysis we combine the two extreme categories at each end of the ideological spectrum. Despite substantial variation among individuals, the mean assigned relative positions for the set of candidates differed as would be expected, with the candidates ordered from left to right as Kennedy, Carter, Ford, and Reagan by the members of each subset.

Having established that there is a general tendency among ideologically self-located subsets to perceive a left-right ideological ordering among the four major 1980 U.S. presidential candidates, we next determined whether the aggregate

ordering of each subset was consistent with the self-identified ideological placement of subset members. Subsets with different ideological self-placement would be expected to differ in their rankings of the candidates if ideology was a relevant determinant of candidate preferences. Moreover, even if all individuals with a given ideological self-placement did not prefer candidates in terms of ideological proximity, we expect that each set of ideologically self-identified individuals would have an ordering consistent with that subset's self-placement on the left-right continuum.

If preferences satisfied the ideological margins conditions for the continuum KCFR, the expected directionality of marginal comparisons is as follows: $KC < KF$, $KC < KR$, $KC < CF$, $KC < CR$, $KC < FR$, $KF < KR$, $KF < CF$, $KF < CR$, $KF < FR$, $KR < CR$, $KR < FR$, $CF < CR$, $CF < FR$, $CR < FR$.

Table 2 shows information about the preferences of the extremely liberal, the somewhat liberal, the moderate, the somewhat conservative, the extremely conservative, and the self-identified non-ideological. Table 2 also shows the margins of preference for each ideologically self-identified subset. The next to last column in lower part of Table 2 shows the number of ideologically consistent pairs of margins for each subset (out of the possible 14). As expected, each ideologically self-placed subset also has a majority preference ordering consistent with its self-placement; for instance, liberals have the majority preference ordering CKFR, moderates have the ordering FCRK, the somewhat conservative have the ordering FRCK.

Table 2 also shows our measure of strength of ideological consistency, the "average difference in pairs of margins" measure described in the previous section. Using that measure, we find that most ideologically self-identified subsets had pairs of margins which were, on average,

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highly consistent with our predictions, with the average differences shown in Table 2 ranging from 18% (for the somewhat liberal and the nonideological) to 33% for extreme liberals. The value of this measure could range (for four candidates) from -57% to 57%. In each subset the *average* difference in pairs of margins

is comparable to, or considerably higher than, the *maximum* difference between pairs of margins in an ideologically inconsistent direction (shown in column 1 in Table 2).

The average strength of ideological consistency across all subsets is considerably higher than the maximum ideological

Table 2. Margins in Pairwise Choices among Individuals with Different Self-Identified Ideological Positions

Ideologically Self-identified Position	Maximum Difference between Pairs of Margins			Matrix of Pairwise Margins of Preferences			
	Ideologically Inconsistent Direction (%)	Ideologically Consistent Direction (%)		Kennedy	Carter	Ford	Reagan
Extremely liberal or liberal	31	76	Kennedy	—	-5	23	46
			Carter	—	—	41	71
			Ford	—	—	—	40
			Reagan	—	—	—	—
Somewhat liberal	5	40	Kennedy	—	-11	1	25
			Carter	—	—	11	29
			Ford	—	—	—	24
			Reagan	—	—	—	—
Moderate	7	36	Kennedy	—	-22	-29	-23
			Carter	—	—	-4	8
			Ford	—	—	—	7
			Reagan	—	—	—	—
Somewhat conservative	19	62	Kennedy	—	-35	-54	-52
			Carter	—	—	-26	-19
			Ford	—	—	—	8
			Reagan	—	—	—	—
Conservative or extremely conservative	24	78	Kennedy	—	-35	-51	-59
			Carter	—	—	-37	-44
			Ford	—	—	—	19
			Reagan	—	—	—	—
Nonideological or no response	12	32	Kennedy	—	-24	-13	-6
			Carter	—	—	8	20
			Ford	—	—	—	8
			Reagan	—	—	—	—
Ideologically Self-identified Position	Ordering		Number of Ideologically Consistent Margin Comparisons		Average Difference in Pairs of Margins (%)		
Extremely liberal or liberal	CKFR		11/14		33		
Somewhat liberal	CKFR		12/14		18		
Moderate	FCRK		12/14		18		
Somewhat conservative	FRCK		12/14		23		
Conservative or extremely conservative	RFCK		8/14		21		
Nonideological or no response	CFRK		12/14		18		

inconsistency between pairs of margins averaged across all subsets. Thus in general, when subsets are combined, we would expect even the most extreme "errors" in directionality between pairs of margins in each subset to be outweighed by the high average level of ideological consistency across all subsets. In other words, the strength of ideological preference calculations shown in Table 2 leads us to expect that almost any combination of subsets would tend to result in a collectivity with strong ideological margins and thus would almost certainly result in a transitive majority preference ordering single-peaked with respect to the posited left-right continuum.⁴

Despite the high aggregate level of ideological consistency in each of the ideologically self-placed subsets shown in Table 2, even within each ideologically self-identified subsets there is tremendous variation in *individual* preferences. Although each subset has a single-peaked majority preference ordering consistent with the subset's ideological self-placement, *individuals* within each subset do not show high ideological consistency. Among the very liberal, there are 16 distinct individual preference orderings and the modal preference (KCFR) is shared by only 37% of the individuals in the set. Only 65% have orderings that are single-peaked. Among the somewhat liberal, there are 21 different individual preference orderings and the modal preference (CKFR) is shared by only 15% of the individuals in the set. Only 52% have orderings that are single-peaked. Among the self-identified moderates, there are 22 different preference orderings, with the two modal categories having only 9% each. We find that 54% of this set of individuals have single-peaked preference orderings. Among the somewhat conservative, there are 19 different preference orderings, with the modal category having 9%. We find that 62% of this set have single-peaked preference orderings.

Among the extremely conservative, the group margins are less consistent with ideology than among the extremely liberal; there are 21 different preference orderings, with the modal category having only 13% each. In this subset only 53% of the individuals have single-peaked preference orderings. Nonetheless, as before, the group as a whole has single-peaked preference ordering RFCK.

When those who claim to have no ideological position are examined, they show more variation among individuals but a similar aggregate result. The set of non-ideologically self-identified individuals exhibits *all* 24 difference preference orderings, with no more than 9% holding any one ordering. Less than half (46%) held preference orderings that were ideologically consistent with the posited left-right continuum. Nevertheless, as we anticipated, even the set claiming no ideological position as individuals *collectively* had a transitive majority preference ordering, CFRK, which was single-peaked with respect to the posited left-right continuum KCFR. That preference ordering indicates an aggregate position just slightly to the left of the self-identified moderates.

What we find striking about the data on the ideological consistency of individuals and subsets as a function of their self-reported position on a liberalism-conservatism scale are that (1) *no* subset had more than 65% of its members with single-peaked orderings and some subsets had only barely above 50% of their members with single-peaked ordering; yet (2) *each* subgroup had a single-peaked transitive majority preference ordering consistent with its self-reported ideology.

Other Types of Subsets

Liberals and conservatives will be found in different proportions among various population subsets. We looked at subsets defined by age, education, social

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class identification, and gender. For each subset (10 in all, because some variables were polychotomies, not dichotomies) we determined majority preferences.

Common sense might lead us to expect that some subgroups might more likely be collectively ideologically consistent than others. On the other hand, we might expect that the perception of a left-right ideological continuum as perceived by the population as a whole will be shared by the various subsets in the population. If this were true, we would expect difference not in the continuum shared by the groups but in the preference ordering held by various subsets for alternatives along that (shared) ideological continuum, of the sort we found for ideologically self-identified subsets. Also we might expect the fit of the ideological margin assumption to be less perfect for most subsets than for the society as a whole because of (1) idiosyncratic differences among subsets, that is, some subgroups, for instance, "Right to Life," might have a consistent ideology that is only partially correlated with a left-right ordering, and (2) the additional sampling error imposed by the smaller size of subgroup samples.

There are four different preference orderings manifested among these 10 subsets (for example, the working class and females have the ordering CFRK; the old and those who did not graduate high school the ordering CRFK; males, the college-educated and the middle class the ordering FRCK; younger and middle-aged voters and those with just a high school degree the ordering FCRK)³ and two different majority winners. However, only the two subsets with ordering KCFR were not single-peaked. Thus 80% of the 10 subsets have single-peaked preference orderings, despite their dramatic differences in actual preferences, and all of the subsets had a transitive majority preference ordering.

The fact that virtually all of these subsets have, in the aggregate, single-peaked

preferences with respect to the posited left-right continuum supports the hypothesis that the left-right perceptions of the nature of the presidential choices is diffused throughout the society, rather than found exclusively in one subgroup or another. Thus it seems clear that while ideology is not perfectly uniform across subgroups, some degree of common ideological orientation is present throughout the society. At the same time, the fact that the two subsets whose transitive ordering is not single-peaked with respect to the left-right continuum are the least well educated and the aged suggests these social groupings may reflect the societal ideological perspective less than others.

Conclusions and Implications

We have shown that only a bare majority of individuals in our 1980 sample have single-peaked preferences for presidential candidates, while 80% of all our subsets and the society as a whole have single-peaked preference orderings. Thus, as we had hypothesized, these collectivities are more ideological than the individuals who compose them. A major implication of our work is that the usual individual-based analysis of ideology will often miss a clear-cut ideology that may be present in the group as a whole. In particular, our approach helps to account for Inglehart's (1985) finding that there is stability of belief at the birth cohort (aggregate) level in contrast to seemingly random variations of beliefs over time at the individual level. In addition, our work helps to explain the common tendency for even the most knowledgeable observers to talk about groups as if groups had preferences and why it is reasonable for political observers to characterize political decisions as indications of the ideological tendencies of the electorate even though a high proportion of the electorate is not making decisions based upon ideology.

As we have shown, it is perfectly possible to talk intelligibly about a collectivity being ideological in its choices even when most of the collectivity's members lack an ideological basis for their preferences. Such a notion can be theoretically well defined without recourse to any "quasi-mystical" notions of emergent group properties. For the mass electorate to have single-peaked preferences (and something close to ideological margins), it is not necessary (or even critical) for most of its members as individuals to see the world primarily in ideological terms.

It is important to distinguish between the question whether members of a society agree with each other about preferences and the question whether they agree with each other about the continuum along which alternatives are to be evaluated. In other words, collectivities can agree, in the aggregate, on what we might call the "terms of the debate," while having great disagreement as individuals about what is the best choice. Moreover, agreement on that shared continuum at an aggregate level does not require a high degree of agreement at the individual level.

If there is a shared continuum, then the size of the margins conveys useful information about the nature of societal preferences along the left-right dimension. For example, in the situation previously described, in the society as a whole in January 1980 Carter did better against Ford than did Kennedy, even though both lost. Nevertheless, this is interpretable as indicating that the "median" in the population was—and a winning candidate would have to be—to the right of Carter.

The fact that the ideological nature of groups is not a simple function of the proportion of individuals who have single-peaked preferences suggests that the sources of ideological orientation must largely be attributed to tendencies toward ideological perceptions which may be only dimly realized in any single indi-

vidual but which may cumulate across individuals (almost none of whom are perfectly ideological) so as to consistently provide an ideological cast to the decisions of the society as a whole and to virtually all of its subgroups. For example, there almost certainly is a tendency of the mass media to use ideological terms as shorthand labels, creating some diffusion of ideological characterizations even though many individuals are incapable of identifying choices in left-right terms and even though some who are capable of doing so do not make their choices entirely in those terms.

Furthermore, it has been shown (Feld and Grofman 1986b) that if a group has ideological margins it can be perfectly represented by a set of representatives all of whom have single-peaked preferences. Thus group ideology, as we define it, helps us account for an important puzzle about why legislative representatives appear to possess a well-defined ideology in left-right terms even though the voters they supposedly represent (and whose votes they compete for in order to win elections) by and large lack such an ideology.

Our empirical findings are only illustrative and we have not explored the factors underlying the emergence of a collective ideology. What we have accomplished, we believe, is to provide a *collective* perspective on the notion of ideology. If so, then the long-standing question of whether there is ideological consistency in *individuals* (e.g., Converse 1964, 1970) needs to be supplemented with a search for the roots of ideological consistency in *collectivities*.

Notes

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1. Single-peaked preferences are identical to Coombsian I-scales (Coombs 1960). A sufficient but not necessary condition for all voters to have single-peaked preferences is to have voters choosing among alternatives in terms of which is closest to their ideal point along some line.

2. Note that the focus of our work is on ideological consistency, not merely on whether there is majority; of course, ideological consistency, as we have used the term, implies transitivity, thus it implies the existence of a majority winner. Preferences are *transitive* if, whenever alternative *a* is preferred to alternative *b* (denoted $a P b$) and also $b P c$, then $a P c$.

3. In general, for m alternatives, there are c_m pairwise comparisons of margins where

$$c_m = c_{m-1} + \left(\sum_{i=1}^m x^i \right) - (m) = 2c_{m-1}c_{m-2}$$

$$+ m^2 - 1.$$

4. It should also be noted that the departures in Table 2 from an ideological ordering of margins are by and large ones that are least likely to interfere with single-peaked preferences for the collectivity. Respondents appear to distinguish more clearly among alternatives near to them than among alternatives far from them (a variant of the well-known "assimilation-contrast" effect). Thus, self-identified liberals did not prefer Ford over Reagan as much as we might have expected from comparisons of their margins; similarly self-identified conservatives did not prefer Carter over Kennedy as much as we might have expected. Reductions in strength of ideological margins caused by an assimilation-contrast effect of this sort do not generally affect the collective preference ordering. Since these inconsistent margins will differ across subsets, lessened ideological consistency of this sort in any given subset will tend to be cancelled out when we combine subsets composed of individuals with different ideological perspectives.

5. It should, however, be noted that these are not independent comparisons; each individual is included in five different subgroups (one for each variable). However, since many individuals do not have single-peaked orderings, it is possible for some subgroups to have ideologically consistent margins while others do not.

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A CULTURALIST THEORY OF POLITICAL CHANGE

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The most telling criticism of political culture theory is that it has coped very inadequately with political change. There is a good reason for this: the assumptions of the political culture approach in fact lead to the expectation of continuity. But continuity can be reconciled with changes, though only changes of particular kinds. The nature of political changes consistent with culturalist assumptions and with the culturalist expectation of continuity are here specified by hypotheses about (1) the effects of changes in social context, whether "normal" or involving abrupt discontinuity, and (2) the effects of attempted revolutionary transformation.

The political culture approach to building positive political theories and to political explanation has been with us since about 1960, and has been much described abstractly and much applied to concrete cases. The seminal works are Almond and Coleman's (1960) and Almond and Verba's (1963). Applications of the approach are covered comprehensively in a retrospective on the influence of their work by Almond and Verba (1979). Explications of it as a contender for paradigmatic status in political science, so to speak, occur in numerous works (e.g., Bill and Hardgrave 1973; Dawson and Prewitt 1969; Merkl 1970; Putnam 1973; and Pye and Verba 1965). My own use of the concept of culture, which I consider more precise than that of others, is discussed in the Appendix.

Political culture theory may plausibly be considered one of two still viable general approaches to political theory and explanation proposed since the early fifties to replace the long-dominant formal-

legalism of the field—the other being political rational choice theory. Indeed, determining which of the two modes of theorizing and explaining—the "culturalist" or the "rationalist"—is likely to give the better results may be the single most important item now on the agenda of political science (Eckstein 1979a).

Whether or not it is advisable to take the culturalist road to theory depends above all on the ability to produce a cogent culturalist theory of political change: a theory consistent with the assumptions (postulates) of the approach and confirmed by experience. Criticisms of culturalist political theories certainly have emphasized the occurrence of certain changes in political structures, attitudes, and behavior and culturalist accounts of their occurrence in order to impugn the approach. Rogowski (1974), for example, has argued that political culturalists have been very offhand in dealing with change—that they have tended to improvise far too much in order to accommodate political changes into their frame-

work. They have done so, he writes, to the point that they no longer have a convincing way to treat political change at all. His argument is directed at culturalist theory in general, but he singles out Almond's work with Powell (1966) as especially indicative of the sins that culturalists commit.

This argument—and others to similar effect—strikes me as cogent criticism of how culturalists have in fact dealt with political changes. Furthermore, difficulties accounting for change in general and for certain kinds of change especially seem to me inherent in the assumptions on which the political culture approach is based.

Difficult, however, does not mean impossible, nor implausible. It is quite possible to deduce from these assumptions a logically cogent account of how political change, and every kind of such change, occurs. My purpose here is to provide such an account, as remedy for the "ad hocery" Rogowski rightly criticizes.

The Postulates of Culturalist Theories and The Expectation of Continuity

The basic reason why a culturalist account of change is intrinsically difficult to construct (hence, why culturalists have in fact tended to waffle in explaining political change) is simple: the postulates of the approach all lead to the expectation of political continuity; they make political continuity the "normal" state.

The Postulates of Culturalism

To see why this is so we must first make explicit the fundamental assumptions from which culturalist theory proceeds—its "axiomatic" basis, so to speak. These assumptions unfortunately have been left implicit in culturalist writings. It is neces-

sary to make them explicit if one is compellingly to specify what experiences are "normal" in a culturalist world and what conditions culturalist theory can and cannot accommodate.

The touchstone of culturalist theory is the *postulate of oriented action*: actors do not respond directly to "situations" but respond to them through mediating "orientations." All else either elaborates or follows from that postulate. What exactly, then, does the postulate assert?

"Orientations to action" are general dispositions of actors to act in certain ways in sets of situations. Such general dispositions pattern actions. If actors do not have them, or if orientations are ill formed or inconsistent, actions will be erratic: patternless, anomic. The idea of "orientations to action" follows a particular psychological stimulus-response model: not the simple "single-stage" behaviorist model in which nothing "subjective" intervenes between the experience of situations and responses to it (actions) but "mediational" models in which responses to stimuli (actions in situations) are considered results both of the experience of objective situations and actors' subjective processing of experience. "Orientations" do the processing. We may call them, as did Bentley, soul-stuff, or mind-stuff. The critical methodological task of studies based on such models is, of course, to penetrate reliably and with validity into the subjective.

Orientations are not "attitudes": the latter are specific; the former *general*, dispositions. Attitudes themselves derive from and express orientations; though attitudes may, through their patterning, help us to find orientations. If orientations frequently occur in collectivities they may be called "culture themes," as by Mead and Metraux (1954). Pye (Pye and Verba 1965) has distinguished four sets of such "themes" that he considers useful for making cultural comparisons on the societal level: trust-distrust, hier-

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archy-equality, liberty-coercion, parochial-national identifications. Putnam (1973) considers the theme of conflict or its counterpart, harmony, critical for cross-cultural analysis. These "themes" exemplify how "orientations" are general dispositions that pattern sets of actions and sets of specific attitudes. It is conventional to regard orientations as having three components: cognitive elements that, so to speak, decode experience (give it meaning); affective elements that invest cognition with feelings that "move" actors to act; and evaluative elements that provide goals toward which actors are moved to act (Pye and Verba 1965).

The assumption of oriented actions would be vacuous without the addition of a second postulate, which we might call *the postulate of orientational variability*: orientations vary and are not mere subjective reflections of objective conditions. The significance of this postulate lies particularly in this: if the processing of experiences into actions were uniform—if it were fixed at the biological level or if it always involved "rationalist" cost-benefit calculation—then mediating mind-stuff could simply be left out of theory. In Hempel's terms, we would only need to know "initial conditions" (situations, structures) to explain actions, since we already know the universal covering law needed to complete an *explanandum*. No doubt ingenuity is required in relating conditions to actions via uniform orientations: the rational choice theories we have provide more than enough cases in point. But this does not alter the logic of the argument that without orientational variability we remain in a strictly behaviorist world. Similarly, if actions are merely "superstructural," we manifestly need only to know situations to explain actions. In that case, only the explanation of deviant cases (like false class consciousness) would require the use of mediating variables.

If orientations are not inherent in actors

but variable, then something that is variable must form them. And if orientations are not simply subjective reflections of varying objective situations, then the variable conditions through which they are formed must themselves be cultural. Orientations are not acquired in some automatic way; they must be learned. Thus, a *postulate of cultural socialization* must hold if the first and second assumptions hold: orientations are learned through the agency of external "socializers." The repertoire of cognitions, feelings, and schemes of evaluation that process experience into action must be imparted by the socialized carriers of culture. The process can be direct, by "teachers" who are culturally variable actors; or it can occur indirectly simply through the experience of variable cultures.

"Rationalist" theorists do not, of course, reject the notion of political socialization. That would be silly. What divides culturalist and rationalist theorists here involves the issue of late-in-life learning, or resocialization.

In regard to that matter, culturalists proceed from a *postulate of "cumulative" socialization*. This means two things. First, although learning is regarded as continuous throughout life (which is not likely to be questioned) early learning—all prior learning—is regarded as a sort of filter for later learning: early learning conditions later learning and is harder to undo. Second, a tendency is assumed toward making the bits and pieces of cognitive, affective, and evaluative learning form a coherent (consistent, consonant) whole.

The postulate of cumulative learning provides the culturalist account of how two fundamental needs of actors in societies are satisfied: the need for economy of action and the need for predictability in interaction. Life would hardly be bearable, even possible, if one had to think out every action, taking into ac-

count all pertinent information and lack of information. Orientational schemata thus save virtually all decision costs. Social life, similarly, would hardly be possible without reliable preknowledge of others' actions and of the effect of one's own actions on those of others. Without such preknowledge social life would tend to be entropic. As Crozier (1964) has cogently argued, "uncertainty" of action also begets power—arbitrary power.

Both economy of action and predictability in interaction are diminished to the extent that individual orientations are inconsistent and that early learning may readily be undone. These conditions have effects similar to a lack of orientations to actions and of socially shared orientations altogether. They lead to erratic, incoherent behavior by individuals and in social aggregates: anomie in the former; the absence of anything like a stable *conscience collectif* in the latter.

It should be pointed out that the culturalist solution of the problems of economy of action and social predictability is not a unique solution, however plausible it may seem. Thus, in the rationalist perspective, economy of action is provided by "ideologies" or by the sensible delegation of decision-making powers (Downs 1957). The fixity required for predictability in social life follows from the very fact that rational choice is considered a fixed disposition. If this is so, one can anticipate the actions of others and adjust one's own behavior to the anticipation. Social predictability may also be achieved through rationally formulated and enforced contractual arrangements or general legal rules. (It should be apparent that the two accounts of economy of action and social predictability provide a good basis for evaluating the relative power of culturalist and rationalist perspectives.)

To summarize, "cultural" people process experience into action through general cognitive, affective, and evaluative pre-

dispositions; the patterns of such predispositions vary from society to society, from social segment to social segment; they do not vary because objective social situations or structures vary but because of culturally determined learning; early learning conditions later learning and learning involves a process of seeking coherence in dispositions. And this is so in order to "economize" in decisions to act and to achieve predictability in social interactions.

The Expectation of Continuity

When the postulates of the political culture approach are made explicit, it should be evident why political culture theorists *should* have difficulties in accounting for political change. The assumptions of culturalist theory manifestly lead to an expectation of continuity, even in cases of changes in the objective contexts of political actions.

The expectation of continuity in aggregate (and individual) orientations follows most plainly from the assumption that orientations are not superstructural reflections of objective structures, but themselves invest structures and behavior with cognitive and normative meaning.

Cultural continuity also manifestly follows from the assumption that orientations are formed through processes of socialization. To the extent that socialization is direct (by precept), generational continuity must occur, the socializers being formed, "cultural men." To the extent that socialization is indirect (by experience), generational continuity still follows; experience with authority occurs first in the family, then in schools, where unformed children encounter formed adults. In either case, what is true of one generation should continue substantially to be true in the next. This applies as much to cultural divisions in a society as to more general culture types and themes—if any exist in the first place.

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This, incidentally, makes the political culture perspective quite compatible with the finding that political regimes typically are short-lived (Gurr 1974).

The expectation of continuity in political cultures follows, most obviously, from the assumption of orientational cumulativeness, namely, that earlier learning conditions later learning and that actors tend to seek orientational consonance. The first allows some room for adult socialization and resocialization—but not much. The second makes unlikely the internalization of piecemeal orientational change that might increase dissonance.

But if change in culture patterns and themes were categorically excluded, political culture theory must immediately be thrown out as obvious nonsense: changes happen, including cultural changes. The saving grace of culturalist theory here is that continuity is, so to speak, an ideal-typical expectation—one that holds in an *abstract*, parsimonious cultural world. It is an expectation akin to that of inertia in the Galilean conception of motion. Physical inertia does not rule out changes of direction or rest, acceleration, and deceleration. It does make such phenomena depend on contingent factors that may or may not impinge on objects in motion. Continuity is the inherent (lawful) expectation and so, therefore, is resistance to change of motion: exceptionally great forces are needed to induce great changes in direction or velocity. The notion of continuity as inertia in motivations (the psychological counterpart of physical motion) thus opens the door to culturalist accounts of change.

Through that door, however, the tendency toward improvised, post hoc accounts of political change may enter—may be bound to enter. If one's preferred theoretical approach implies a strong bias toward the continuity of culture or resistance to cultural change, then it is always tempting to extemporize theory-saving

"special" conditions, or adjustments in concepts or theory to handle occurrences of change—especially major change. If, say, theoretical difficulties arise from emphasizing early socialization, then why not just relax that emphasis and assign more scope for late socialization or adult resocialization? If the assumption of a tendency toward orientational consonance makes it awkward to explain certain observations, then why not simply posit more toleration for dissonance? Or why not redefine consonance? In that way, however, one is likely to end with the term continuity meaning nothing more than "not completely (or instantaneously) changeable"—which drains the term of all reasonable meaning. This is exactly the point of Rogowski's criticism of how culturalists have in fact accounted for political change.

The remedy is to develop an explicit general culturalist theory of change, consistent with culturalist assumptions, in order to prevent ad hoc tinkering with culturalist postulates and their implications. Such a theory should state, prior to explanations of specific changes, the characteristics of change that the political culture approach can logically accommodate and those that do not fit its constraints.

To formulate such a theory, I will consider two broad types of cultural changes: those arising "naturally" from changes in situations and structural conditions and those that result from "artifice"—deliberate attempts to transform political structures and behavior.

Situational Change

Pattern-Maintaining Change

Actors must often face novel situations with which their dispositional equipment is ill suited to deal. The world changes or presents us with experiences that are un-

familiar for other reasons (say, the penetration of peasant societies by market forces). The unfamiliar is encountered routinely in maturation, as one proceeds from family to school, from lower schools to higher ones, and from schools to participation in adult institutions. At the level of society and polity, novel situations arise from internal "development," however development may be conceived. Novel situations also arise from socially internal discontinuities (economic crises or political disruptions, like those caused by governmental instability or collapse, or from changes brought about by protest movements), or from externally imposed changes. Immigration brings actors into unfamiliar situations. So does internal migration and social mobility. The encounter of novel situations will, no doubt, occur much more frequently among individuals than on the macro level, but it also occurs in groups and societies.

Novel situations may be short-lived results of ephemeral upheavals. In that case no cultural adjustments are needed, nor are they likely to occur. What, however, should one expect if such situations persist?

If cultures exhibit inertia then it should be expected that changes in culture patterns and themes will occur so as to maintain optimally such patterns and themes; that is to say, changes in culture are perfectly consistent with culturalist postulates if they occur as adaptations to altered structures and situations and if the function of change is to keep culture patterns in existence and consonant. "Pattern maintenance" (Parsons' concept) can take that form just as well as strict cultural continuity.

The French have a half-facetious adage for this sort of pattern maintenance: The more things change, the more they remain the same. The saying no doubt fits (used to fit?) France. The pragmatic masters at pattern maintaining change, however, have been the British. Tory concessions to

British working class voters and interests are the usual case in point. Their function—sometimes "latent" but in the case of Disraeli's Tory democracy quite explicit—was to maintain Tory hegemony in the face of considerable sociopolitical change through the maintenance of as much as possible of what the Young England Circle considered the feudalistic virtues: the disposition to defer to one's betters and action by the betters on behalf of the lower orders. The point applies to reforms of the suffrage and also to the less well known role of the Tories in the evolution of the British welfare state, which Tory governments not only have kept virtually intact but much of which they pioneered.

An alternative to pattern-maintaining change is to subject unfamiliar experience to procrustean interpretation in order to obviate cognitive or normative change. "Perceptual distortion" has turned up frequently in experiments on how individual cognitive dissonance is handled (see Brehm and Cohen 1962). We know at least a little about the same way of dealing with the unfamiliar on the political macro level. To give just one example: party political elections in Northern Nigeria were initially regarded as a version of long-familiar elections to chieftaincy, in which the "candidates" were a small number of ascriptively defined eligibles (Whitaker 1970). The extent to which perceptual distortion can be adaptive to unfamiliar experience no doubt is highly limited. However, where institutions like elections to chieftaincy exist in traditional cultures, the adaptation of dispositions to other kinds of elections should be easier than in other cases.

Change Toward Flexibility

Highly modern societies have traits that make it especially likely that actors and aggregates of actors will frequently con-

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front novel situations. Social mobility, vertical and horizontal, is the most obvious cause. Because any changes in dispositions are costly (dysfunctional) in the culturalist perspective, one should expect as a correlate to the expectation of pattern-maintaining cultural change, that the more modern societies are, the more the elements of their cultures will be general, thus flexible. No doubt there are considerable limits upon how general and flexible orientations can be and still perform their functions of making experience meaningful, actions economical, and interactions predictable. In more modern societies one should not expect culture to change as readily as situations and structures. Situational and structural change tend to occur with great frequency and rapidity in modern societies, and the assumption of orientational inertia postulates resistance to frequent, swift reorientation. Rather one should expect that the rigidity of cultural prescription will relax, so that culture can accommodate much social fluidity.

The tendency toward cultural flexibility can be regarded itself as a way to maintain cultural patterns and themes. As societies become more changeable, the elements of culture increasingly become "forms" that can subsume a variety of "contents." It is probably no coincidence that some sociologists early in the twentieth century (especially Simmel [1950]) adapted the Kantian distinction between form and content to social analysis. Durkheim argued much the same point directly. In early societies, he wrote, "the collective environment is essentially concrete . . . [and] the states of conscience then have the same character." ("Culture" is not a bad translation of his notion of a *conscience collectif*.) As societies develop, the "common conscience" is obliged to rise above diversity and "consequently to become more abstract. . . . General ideas necessarily appear and become dominant" (Durkheim 1960, 287-91).

I want to make three other points pertinent to the expectation that cultural abstractness and flexibility will grow with social development. First, the disposition to act "rationally" introduces just the kind of general and flexible culture trait that inherent social fluidity requires. (Durkheim [1960] already associated rational attitudes and behavior with the abstractness of thought necessary in highly developed societies.) The rationalization of modern life—which Weber considered to be its governing trait—thus may be an accommodation to structural conditions rather than, contra Weber, their underlying cause.

Second, the obviously difficult problem of finding a proper trade-off between two warring imperatives in modern societies, that of cultural flexibility and that of cultural fixity, is bound to be a practical difficulty, not just a theoretical one. Reconciling fixity with flexibility, abstractness, and formality may be a crucial element in what has widely been perceived as growing malaise in highly modern societies. Anomie will follow not only from lack of internal guides to action but from guidelines too general and loose to serve in the relentless particularity of experience. Highly modern society thus may be intrinsically acultural and, for that reason, transitory or susceptible to *surrogates* for culture—including cults and dogmas.

The expectation of cultural flexibility, finally, should apply to *all* highly modern societies. It thus pertains to polities initially based on rigid dogma (like communist societies) that have successfully pursued modernization. In such societies, the first expectation, that of cultural inertia, should hold. Old culture should resist new dogma. The expectation of pattern-maintaining change (or perceptual distortion) should hold as well. So one should expect also that as culture changes in such societies, it will change toward greater flexibility—and therefore to reinterpret-

tions of dogma that make it increasingly pliable.

Cultural Discontinuity

Contextual changes can be so considerable or rapid or both that neither pattern-maintaining changes nor changes that gradually relax cultural rigidity to deal with social fluidity are possible. Rapid industrialization is the case in point usually cited. Changes resulting from war or from the formation of new polities also generally involve upheavals in social contexts. Such upheavals may result as well from economic traumas like the great inflation of 1923 in Germany (which led to far greater social disruption than the Great Depression—or possibly even the Black Death). And traumatic change sometimes strikes special segments of society rather than the whole.

We must deal, therefore, with social discontinuity, as well as "normal" change. Culturalists have tended either to avoid the matter or, worse, to treat cases of social trauma simply as "deviant cases" in which the theoretical constraints of their perspective are off—not least, the expectation of cultural inertia.

Obviously, traumatic social discontinuity will have cultural consequences different from contextual stability or less rapid, less pervasive change. Even in such cases, however, we may not simply improvise. If the assumptions of culturalists are correct, then traumatic social discontinuity should have logically expectable consequences, no less than other change.

The one consequence of social trauma absolutely precluded by culturalist assumptions is rapid reorientation. Social upheaval may overcome cultural inertia, but if so, actors should be plunged into a collective infancy in which cognitions that make experience intelligible and normative dispositions (affect, evaluative schemes) must be learned again, and learned cumulatively. No culturalist may

expect, for instance, a democratic political culture to form, in a few short years, in a society like Germany after World War II, or "national" orientations to form rapidly in postcolonial tribal societies. Instead, changes in political cultures that occur in response to social discontinuity should initially exhibit considerable formlessness. For *formlessness* one may substitute other terms, like Durkheim's *anomie* or Merton's *deinstitutionalization*. The essence of the matter is that culture loses coherent structure. It becomes highly entropic.

The idea that rapid, large-scale contextual changes are personally disorienting and culturally disruptive is hardly new. Lipset (1960) argued a generation ago that rapid economic development is associated with political "extremism" ("anomic protest movements" like anarchism and syndicalism), despite the fact that high levels of such development are related to political stability. Huntington (1968) later made much the same point, and Olson (1963) has probably developed it most gently.

To say that formlessness under conditions of socioeconomic discontinuity should be "considerable" is not mere hedging. Cultural entropy can never be complete. If it were, no patterned action or interactions would be possible at all. In any case, social discontinuity never is total—intimate social units, like the family, survive the greatest upheavals (may, indeed, be strengthened by them, as refuges of predictable order); so too do structures that are supposedly merely instrumental—for example, bureaucracies. As well, if learning is cumulative, older people should exhibit a good deal of orientational inertia even when traumatic socioeconomic change occurs. We may surely suppose that the more ingrained orientations are and the more they are consonant systems, the less susceptible they are to "disorientation"—the more mechanisms like perceptive distortion will

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be used to invest experience with accustomed meaning.

Governmental authority will, of course, survive cultural discontinuity. In fact, it is likely to become more powerful to the extent that internalized dispositions cannot govern actions and interactions. How then do people act politically if political culture is highly formless?

We can get useful clues to answers from the growing literature on an analogous experience: how children adapt to novel situations that they enter in highly discontinuous ways: going to school, for instance, or going from one to another type or level of schooling. Much of the literature on this subject (like Wakeford 1969 and Woods 1979) has been informed by Merton's (1949) path-breaking study of the bases of deviant behavior, which dealt in general terms with behavior under more or less "anomic" conditions.

Under conditions of cultural discontinuity, conformity with authority is still likely to occur, but it will tend to have certain characteristics. In Merton's technology, it will tend to be *ritualistic* or else *self-serving* (opportunistic and of dubious morality, as general culture defines morality). Ritual conformity is compliance without commitment. One does what the rules or rulers prescribe, not for any discernible reason but (quoting from a lower-class British pupil interviewed by Woods) "because I behave meself . . . I just do what I'm told . . . [I] ain't got much choice." Conformity of this sort may be supposed to occur frequently in cases in which the former political cultures and subcultures prescribed high compliance ("subject cultures," as Almond and Verba called them). Self-serving, opportunistic conformity bends norms and rules for private advantage—including that of getting ahead in the competition for political power. Charles Dickens observed a lot of that sort of behavior in his travels in America as he reports them in his *Ameri-*

can Notes. Thus, in regard to a very successful businessman, "He is a public nuisance, is he not?" "Yes sir," . . . "And he is utterly dishonest, debased, and profligate?" "Yes, sir." "In the name of wonder, then, what is his merit?" "Well, sir, he is a smart man" (1957, 246). I mention Dickens because one should especially expect "smart" conformity in immigrant societies or immigrant segments of society, where (as in schools) discontinuity occurs through movement into an unfamiliar but intact culture. Perhaps one should expect it even more in cultures greatly unsettled by upheaval. Thus, Burke presciently remarked (in 1790) that when cultural constraints are off, "the worst rise to the top" (1923, 45).

More commonly than conformity, one should expect what Merton called *retreatism* under conditions of cultural discontinuity. Retreatism involves withdrawing from the "alien" larger society into the smaller, more familiar worlds of family, neighborhood, village, and the like. In Almond and Verba's scheme of concepts, it should show up as increased "parochialism." In the small worlds of schools, retreatism tends to involve self-imposed isolation—for instance, into remote places and daydreams or what Woods calls removal activities—"unserious pursuits which are sufficiently engrossing . . . [to make participants] oblivious for the time being of [their] actual situation"—or both.

Rebellion against, and intransigent resistance to, authority are also likely responses to the experience of cultural decay. A voluminous literature links social, economic, and political discontinuities to political violence—from Marx to Moore and Skocpol. Rebellion and intransigence, however, are always likely to be costly and call for much energy; retreatist behavior into parochial worlds or ritualistic conformity are thus more likely, especially where governing power—if not authority—is strong.

What should follow over time from contextual and cultural discontinuity? If economy of action and predictability indeed are imperatives in individual and collective life, one should expect new culture patterns and themes to emerge. But if dispositions are formed by cumulative learning, they should emerge only slowly (over generations) and, in the transitional period, at great costs resulting from raw power, withdrawal, and (because of withdrawal) forced mobilization and rebelliousness against it. Thus, the process of reformation of political cultures should be prolonged and socially costly. This is all the more likely to be the case if parochial units remain intact refuges from discontinuities in society, economy, or polity.

The expectation is logical also if older people, as is likely, cling to long-fixed dispositions even in face of strong forces that might unsettle inertia. We might thus posit as a general expectation that in the process of cultural reformation considerable age-related differences should occur. In fact, age, in cases of pronounced discontinuity, might even be expected to be a major basis for subcultural differentiation. If indeed this were found to be so, the cultural perspective upon theory would be enormously strengthened over alternatives. Empirical work pertinent to the expectation, however, is oddly lacking; and as culturalists have built adult learning increasingly into their approach in order to accommodate ill-fitting facts, the incentive to inquire into age-related cultural differences, in both established and transitional contexts, has regrettably declined.

I want to make another point about the reformation of dispositions and culture patterns, more briefly. As the young should be more susceptible to reorientation than the old, so one should expect to find in social macrostructures particular segments that have traits especially conducive or susceptible to reorientation. By

"conductive traits" I mean structural or dispositional traits readily accommodated to new culture patterns or, indeed, anticipations of them. In Western traditional societies, for instance, there always existed a large island of achievement in a sea of ascription—the celibate clergy, which hardly could be ascriptively recruited. The clergy, in fact, played a considerable rôle in the emergence of modern political institutions—despite its stake in the distribution of traditional privileges. Similarly, socially "marginal" groups—groups that occupy the fluid interstices of established cultures—should be highly susceptible to reorientation, thus "vanguards" in the reorienting of unsettled societies. There is a good deal of literature making the case that this is indeed so (e.g., Rejai and Phillips 1979 and Wolf 1973).

Political Transformation

By *transformation* I mean the use of political power and artifice to engineer radically changed social and political structures, thus culture patterns and themes: to set society and polity on new courses toward unprecedented objectives. Transformation, typically, is the objective of modern revolutions. It can also be the objective of military conquerors and of nation builders or other modernizers. Revolutions, however, provide the most unambiguous and dramatic cases. I will therefore confine my remarks to them—though what is said about them should also apply to transformation attempted in other ways.

Hannah Arendt (1963) undoubtedly was right in arguing that attempts at revolutionary transformation are distinctively modern—that revolutions as we think of them (not mere rebellious attacks on authorities or their actions) begin with the French and American revolutions. As long as political and social structures were considered divinely ordained, or natural,

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or simply the ways of a folk, the idea of their deliberate transformation hardly could occur. "History" then could only be endless repetition or an intrinsic progress toward a preordained end. Societies and polities could no more be "transformed" than the heavenly bodies set upon new orbits. One of the decisive traits of modern societies then is the belief that a "new beginning"—a felicitous and not redundant expression—could be made in political and social life.

Initially, making a new beginning did not seem to call for much artifice—no more, perhaps, than a proper constitution. Achieving liberty or equality throughout society simply called for setting polities and societies on their inherently right course—right, given human nature. For reasons not necessary to sketch in the age of the "God that failed," *really* making revolution—not seizing power but the accomplishment of transformation—came increasingly to be seen as a task, and a difficult task, for political artificers. Unfortunately, systematic studies of that process are few, although the exceptions often have been notable: for instance, Massell's study of Soviet attempts to bring Soviet Central Asia into modernity (1974) and Kelley and Klein's study of the effects on inequality of the Bolivian Revolution of 1951 (1981). Inquirers into revolution still are hooked on the issue of their etiology.

Since revolutions are themselves major discontinuities and since they generally occur in periods of social or political upheavals, not least governmental breakdown (Edwards 1927; Brinton 1965), the expectations listed in the preceding section should apply to transformation. But I want to state here some expectations that follow from the culturalist perspective especially for processes of revolutionary transformation. Intrinsic interest and contemporary relevance aside, these processes seem to me especially critical for evaluating culturalist theories and their

bases. After all, transformative processes involve not only adjustment to necessity but also the deliberate engineering of great change, and they are typically backed by great power and control.

As a first expectation we may posit that revolutionary transformation is strictly impossible in the short run. Revolutions certainly bring upheaval. They may also be expected to bring about movement in the direction of their professed goals by readily accomplished actions—instituting wide suffrage, kicking out the landlords and redistributing land, ending feudal privileges and obligations, and the like. But if discontinuity begets "formlessness" of culture, then revolutionaries can hardly do much to reorient people in the short run (say, in a generation or so). Reorientation is, of course, the less likely the more intact is the prerevolutionary culture: the more it provides parochial refuges from transformative power or institutional centers of resistance to it. But even if revolution only reflects discontinuity instead of engendering it, the expectation stated still should hold.

If the conventional norms and practices of political life are disrupted by revolution, what can be put in their place? We may posit the answer that revolutionary transformation will initially be attempted by despotic or legalistic means. What, after all, can "order" societies and polities in place of conventional, internalized culture? Only brute power, or else the use of external legal prescriptions as a surrogate for internal orientational guides to behavior. "Revolutionary legalism" was in fact a device used early after the Bolshevik seizure of power, and it overlapped a good deal (even before Stalin) with attempts to "storm" society (especially its more backward parts) with head-on "administrative assault." Neither, according to Massell (1974), accomplished much toward the realization of transformation; responses to it, he writes, included "avoidance," "selective

participation," "evasion," "limited retribution," and "massive backlash."

"Legalism," it might be noted here, is likely to be a general response to massive cultural disruption, whether revolutionary or situational or both. Indeed, it can become, in highly unusual cases, a persistent surrogate for normative culture—indeed, a culture form. I have argued this elsewhere (1979b), defining "legalist" cultures as cultures in which legal rules are widely known, such rules are widely used (instead of justice or prudence) to justify political standpoints or decisions, legal actions are the normal mode of dealing with conflicts and disputes, and therefore laws deal in highly detailed—if possible comprehensive—ways with social interaction and tend to be punctiliously adhered to. Durkheim (1960) argued the even more general, related proposition that in the course of development civil law (which regulates social interactions) constantly grows, while criminal, or restitutive, law declines. His argument makes sense if indeed development "loosens" normative cultural prescription, as I argued, and lessens cultural similitude; as Durkheim argues.

The case I used to make this argument is contemporary West Germany. That, we should note, also is the case Rogowski (1974) mainly relies on to argue that re-orientation *can* occur rapidly—the crucial point in his critique of culturalist theory. Rogowski seems to me to miss the real import of "deviant cases"—that through their very abnormal characteristics they can be used to shed light upon the factors that condition typical cases.

What of the long-run prospects of revolutionary transformation? I suggest the expectation that the long-run effects of attempted revolutionary transformation will diverge considerably from revolutionary intentions and resemble more the prerevolutionary condition of society. The expectation is not that little change in "content" will occur: in who

holds power, gets privilege, and so on. No inevitable Thermidorean Reaction is posited. The argument is somewhat less categorical: reconstructed culture patterns and themes will diverge widely from revolutionary visions and will tend to diverge from them in the direction of the patterns of the old society and regime. The degree to which the expectation holds obviously depends on the extent to which the old culture was already in disarray.

Several points made earlier lead to this expectation. Culture must still be learned on a comprehensive scale, as in all societies; and although revolutionary teaching can no doubt play a considerable role in shaping the young, it can hardly replace socialization in small parochial units. Nor are teachers or role models likely to be, extensively, the sort of marginal individuals who are steeped in revolutionary dogma as a surrogate for convention—or people for whom the revolutionary vision has much meaning at all. Sheer cultural inertia will also play a role in the process of revolutionary decay; so will the tendency toward turning change into pattern maintenance—perhaps by a progressive transformation of revolutionary visions into mere revolutionary rhetoric; so—to the extent that the new rulers succeed in modernizing—will the tendency of modern cultures to be general, abstract, and (especially pertinent here) flexible; so will "retreatist" and "ritualist" responses to discontinuity; and so will the tendency of opportunistic conformists to get ahead, by scheming or approval, in unfamiliar contexts.

In fact, it may well be the case that the short-run effects of attempted transformation are greater than the longer-run effects. More can be done in upheaval than when life again acquires fixity. Kelley and Klein (1981) have argued precisely this point, on the basis of generalizing the case of the Bolivian Revolution of 1951.

Whether all this also entails the expectation that in the longer run incremental

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change will accomplish more than attempts at radical transformation we can perhaps leave an open question here. But note that the rulers of the Soviet Union came increasingly to view the achievement of cultural change as a matter for what they called "systematic social engineering" for—as Massell (1974) describes it—"a pragmatic commitment to relatively patient and systematic social action, wherein at least as much time and effort would be devoted to the building of bridges to traditional society . . . as to actual and direct confrontation with the traditional system."

Conclusion

It may well be the case that the political culture approach has been used to explain political changes in the sort of ad hoc and post hoc manner that saves—and thus weakens—theories rather than testing and strengthening them. Culturalists hardly have a monopoly on such theoretical legerdemain—certainly not when compared to rational choice theorists—when discomfiting facts confront them. But I have tried to show here that culturalists must have a strong propensity toward improvised theory saving when dealing with political change, since their assumptions lead, necessarily, to an expectation of cultural continuity—at any rate in a "pure" (abstract, ideal-typical) cultural world, where all matters falling under "*ceteris paribus*" are in fact "equal."

Nevertheless, it should be evident that a cogent, potentially powerful theory of political change can be derived from culturalist premises. The theory sketched here specifies that changes in dispositions, in response to contextual changes, should be pattern-maintaining changes or—if the contextual changes involve modernization—changes toward normative generality and flexibility; that in response to abrupt social discontinuities cultural dis-

positions should, for a considerable period, be "formless"—incoherent in individuals and fragmented in aggregates; that in such cases retreating into intact parochial structures occurs, while conformity should become ritualistic or opportunistic; that revolutionary artifice cannot accomplish cultural transformation in the short run; that such transformation will be attempted by despotic power or (mainly hopeful) legal prescriptions; and that, in the longer run, attempts at revolutionary transformation will tend to be regressive or at least have quite unintended outcomes. Note, however, that nothing here rules out engineered change, so to speak—attempted structural reforms of politics. In the modern world, political tinkering, on small or grand scales, is endemic. The theory simply states what should result from such tinkering.

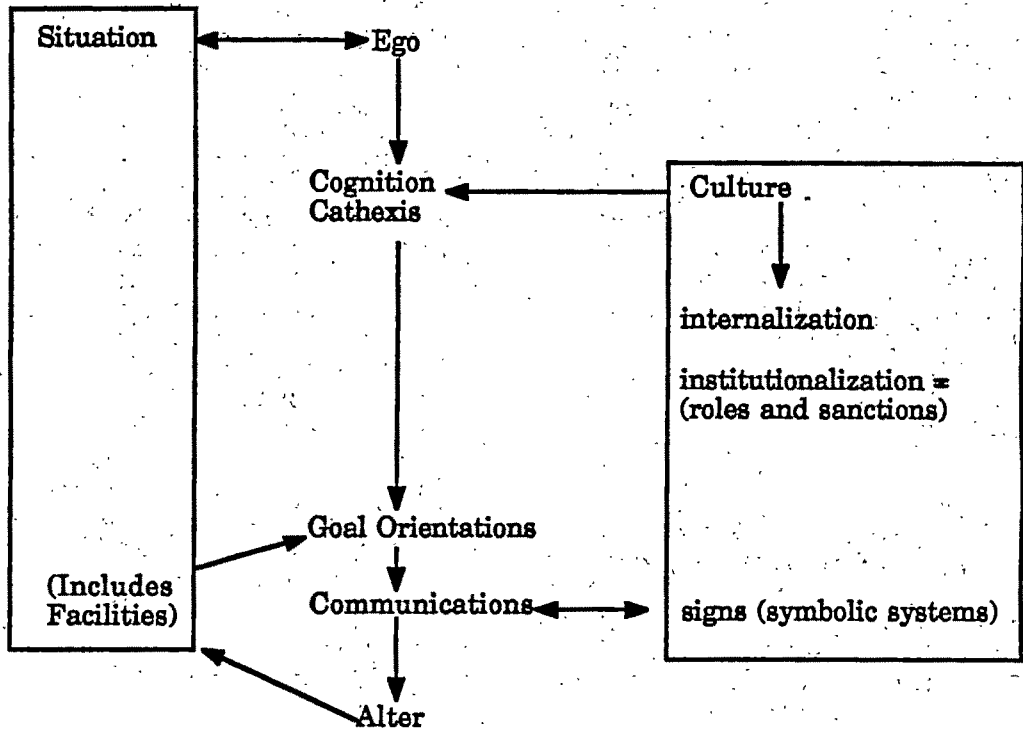
The problem of testing the theory against experience obviously remains, as do problems of operationalizing concepts for that purpose. But obviously theory comes first.

If the power of a culturalist account of political change is to be compared with that of different approaches to political theory and explanation, then general accounts of change, derived from non-cultural postulates and similar to that presented here, are needed. Political-culture theories, admittedly, have not heretofore met the challenge of developing a general theory of change; but neither have others.

Appendix: Culture

The term *culture*, unfortunately, has no precise, settled technical meaning in the social sciences, despite its centrality in them. The variable and ambiguous use of key concepts generates unprofitable arguments that are merely definitional. Hence I append a note that places my use of the term, as sketched in the first section, in its conceptual context.

Figure A-1. Interaction in the Action Frame of Reference



My use of the term *culture* tries to make explicit, at the axiomatic level, what is implicit (occasionally almost explicit) in the works of Almond and his various collaborators (Coleman 1960; Powell 1966; Verba 1963; Verba 1979). Their use of the concept seems to be based squarely on Talcott Parsons' "action frame of reference." Parsons first worked out that "frame of reference" as a way of synthesizing four apparently diverse, all highly influential, early modern social scientists: Marshall, Pareto, Durkheim, and Weber (Parsons 1937). He and collaborators developed action theory in a large series of works, the most useful of which probably is the multiauthored book, *Toward a General Theory of Action* (Parsons and Shils 1951).

The action frame of reference is based, at the microlevel, on Parsons' notion of an interaction, societies being complexes of interactions (some earlier sociologists called them acts of "socation"). The notion is depicted on Figure A-1. In brief translation, (1) ego (an actor) is in a "situation"—an objective context; (2) ego cognitively decodes that context and invests it with feeling (cathexis)—thus the context comes to have meaning for the actor; (3) the manner of investing situations with meaning is acquired through socialization, which consists mainly of early learning—this imparts the modes of understanding and valuing prevalent in societies or subsocieties or both. In aggregate, these may be called a society's "culture"; (4) socialization leads to the in-

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ternalization of cognitive and affective meanings (viz., the cultural becomes personal) and their institutionalization (the definition of expected behavior in social roles and that of sanctions in case of deviation from expected behavior—these make smooth and regular patterns of interaction possible; (5) cognitions and affective responses to them define goals and ways to pursue them; (6) cognitions, feelings, and goals are communicated to alter (another actor) through the use of “signs” (symbolic expressions of culture that make ego’s actions intelligible to alter)—but actions also depend on objective facilities that are part of any actor’s situation and that independently affect the choice of goals; (7) alter responds, changing the situation in some respect, so that the process resumes.

Note especially that the action frame of reference emphasizes neither subjective nor objective factors but rather how the two are linked in interactions. Culturalists focus on the matters in the box on the right, but they should also bring that on the left into interpretation and theories. This I have tried to do throughout this essay, emphasizing how culture conditions change in varying contexts of objective change.

Alternatives to the notion of culture I use come chiefly from cultural anthropology. I use the plural intentionally, because the meanings of culture vary a great deal in that field. One can probably subsume these meanings under four categories: (1) culture is coterminous with society: it is the whole complex of the ways of a “folk,” of human thought and action among particular people—Park (1937) comes close to that view; (2) culture is social life in its subjective aspects: the knowledge, beliefs, morals, laws, customs, habits of a society—one finds this meaning (and these illustrative words) in the seminal work of Tyler (1871) and, later, Benedict (1934) and Kluckhohn (1962); (3) culture is what differentiates

societies from one another, for the purpose of idiographic description but also for theorizing through comparisons and contrasts (agreements and differences)—I take the seminal work here to be Malinowski’s (1944); (4) culture is the distinctive, variable set of ways in which societies normatively regulate social behavior (Goodenough 1968; Sumner 1906).

The fourth set of meanings comes closest to that used here. My use of the concept of culture here seems to be justified by usage in political science and, more important, by its suitability to testing theories through the catholic deduction of unknowns once it postulates are explicitly stated. Anyway, my version of the concept is that about which theoretical conflicts have thus far occurred in political inquiry.

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THE DIALECTICAL LOGIC OF THUCYDIDES' MELIAN DIALOGUE

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If the realist tradition has underappreciated the formalizable quality of Thucydides' scientific investigations, neorealist teachers and writers have generally failed to see the normative and dramatical features of Thucydides' political science, each an expression of his dialectical epistemology and ontology. Nicholas Rescher's partial formalization of dialectics as a controversy-oriented approach to knowledge cumulation and Kenneth Burke's dramaturgical approach to textual understanding are both shown to fit Thucydides' argumentation in the Melian dialogue. Thus argumentation produces new knowledge about the inner determinants of Athenian imperialism; simultaneously it dramatically reveals the constituting practical rationale of Athenian actions to be unjust. Once Thucydides' determining essences of power politics are properly uncovered, their false "eternal, mathematical necessity" can be appropriately criticized. A case is thus suggested for a "neoclassical polimetrics" more fundamentally grounded in "political argumentation" about practical choices in particular contexts than in ahistorical laws, inductive statistics or deductive mathematics.

In writing his classic study of the Peloponnesian War, Thucydides sought "an exact knowledge of the past as an aid to the interpretation of the future, which in the course of human things must resemble if it does not reflect it" (1.22).¹ For twentieth-century realists, this has typically meant seeking timeless truths, or even mathematical laws, about the state's eternal, self-interested search for power, or the need to balance against it. Werner Jaeger's reference to "this political necessity, the mere mathematics of power politics" conveyed such a message to the German-speaking world of the 1930s. Indeed, in a book widely renowned for its penetrating account of Greek civic culture, Jaeger asserts that as evident in Sparta's fearful response to the growth of Athenian power, this "necessity" "is defined [by Thucydides] as the true cause . . . of the

[Peloponnesian] war" (1976, 488; for similar views of other modern realists see Wight 1978, 24 and Morgenthau 1978, 8-9, 38).

Would not any other social scientist in our own century, if desirous of emulating the increasingly universal validity of the natural sciences and committed to building up reliable knowledge grounded in the deductive certainty of axiomatic formal argument and the "positive" evidence of our senses, also want to claim as their own Thucydides' search for "the clearest data," and his standard of objective precision: "the accuracy of the report being always tried by the most severe and detailed tests possible"? Would they not also want to "have written [such a] work, not as an essay which is to win the applause of the moment, but as a possession for all time" (1.23)? Influenced by apparently similar contemporary epis-

temological trends (developmentally described as "positivism," "logical positivism," "logical or analytical empiricism," and "critical rationalism" in Alker 1982), leading expositors of "scientific" neorealism focus their frequent, typically positive citations to Thucydides' classic on such instrumental, lawlike, causal analyses of state actions, possibly extended to include the structural determinants of these actions (Keohane 1983, 1986; Waltz 1979). Despite their objections to the imprecision, logical inconsistency, and empirical nontestability of too much traditional realism, contemporary neorealists still seek timeless laws of power politics, independent of moral praise or blame; they wish these laws to be objectively and falsifiably delineated with what Jaeger took to be mathematical precision.

Thucydides' work is equally well known today for the fatalistic, moral cynicism of the Athenian statement in the Melian dialogue (or conference) that "right, as the world goes, is only in question between equals in power, while the strong do what they can and the weak suffer what they must" (5.89). Associated with doubts concerning the possibility of deriving constructive, practical "lessons from history," one finds aspects of this critical tone in denials by structural neorealists that some policies or states have a more moral character than others (Waltz 1979, 127, 187n.). More historically accurate (but almost as bleak) is Keohane's appraisal that "classical Realism, with its philosophical roots in a tragic conception of the human condition, directs our attention in the twentieth century to the existential situation of modern humanity. . . . But Realism, whether classical or structural, has little to say about how to deal with that situation" (Keohane 1983, 519).

Surely Thucydides is one of the first "scientific historians" (a phrase used in Jaeger 1976 and Strauss 1978, chap. 3; and supported by Keohane's arguments

[1983, 506-10] for continuities in the "hard core" of the realists' "research program" from Thucydides to Waltz). But I think his epistemological conception of "scientific history" or "scientific politics" can more accurately and suggestively be described as a morally engaged version of dialectical logic, whose closest descendant is argumentation theory (Barth and Krabbe 1982; Eemeren, Grootendorst, and Kruijer 1987). His historiography combines a commitment to factual accuracy with what we would now call a dramaturgical perspective on human affairs (Harré and Secord 1972). Ignoring its dialectical origins, we seem almost to have forgotten that his scientific political history is at the same time one of the greatest morality plays ever written, in essence a Greek tragedy.

To better understand the relevance of Thucydides' insights and historical-political science for today's problems, I shall focus on the dialectical ways in which he presents, reconstructs and uses the Melian dialogue. I shall amplify the argument that Thucydides consciously used a sophistic dialectic in the writing of this dialogue (Finley 1939, 1942). He did this with a style and degree of formalizable rigor that compares favorably with more conventional contemporary modes of logical and statistical analysis. In contemporary language, his polimetrics sought to uncover, through critical, rational argumentation, the inner determinative structures, the criticizable and partly changeable constitutive practices of Greek politics.

The Melian Dialogue As Partially Formalizable Dialectics

Neither Popular Rhetoric nor Apodictic but Formal Debate

There are several levels of discussion in the dialogue, each with different epistemo-

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logical import. The first level concerns the type of negotiation to take place between the Melians and the Athenians; it leads to agreement in favor of formal disputation.

The evidence for treating the Melian dialogue as a classical disputation or debate is explicit within Thucydides' text. Consistent with their oligarchic political character, initially derived from Sparta, their parent state, the Melians "did not invite [the Athenian] representatives to speak before the people [as would be the custom in a democratic state like Athens], but asked them to make the statement for which they had come in front of the governing body and the [oligarchic] few" (5.84). The Athenians put a good face on this choice by treating it as a preference for serious argument rather than for the set speeches of popular rhetoric by which the masses might be "led astray": they propose that the Melians "should instead interrupt us whenever we say something controversial and deal with that before going on to the next point" (5.85). The Athenians propose point by point philosophical debate about controversial matters rather than popular rhetoric.

The kind of argument envisioned is also not what the Greeks (and Aristotle) would call apodictic (deductive, axiomatic, or necessary) reasoning. I suspect their model of such reasoning was geometric deduction, which had already achieved significant results by the end of the fifth century B.C. My evidence is the Melian statement that disputants "should be allowed to use and to profit by arguments that fall short of a mathematical accuracy" (5.90). Since their lives depend upon their persuasiveness, they want to make arguments that they can not strictly prove but against which effective counter-arguments might not be made.

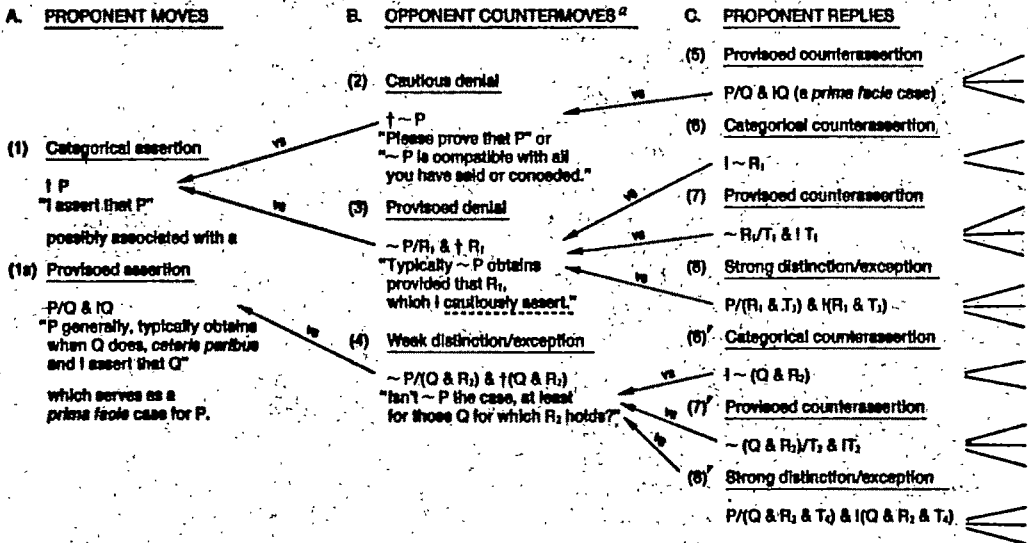
Both parties clearly have in mind some kind of uncoerced formal disputation (or "dia-logic," [Habermas 1971]) before a judge and an audience. To use Aristotle's terminology, we are in the realm of "dia-

lectic," which he considered appropriate for political inquiry, the realm of arguable opinions about contingent or "probable" truths. Here dialectical inferences "must start from premises that command general assent rather than universal or necessary truths" (Aristotle 1964, 83, 153). Except for his somewhat more positive, democratic conception of rhetoric, Aristotle's *Rhetoric* and his *Topics*—together the first great Western treatise on dialectical argumentation—will assert the same distinctions several decades later (Arnhart 1981, chap. 2).

Coercive Irrationalities in the Dialogue

This agreement on the desirability of a dialectical mode of political discussion occurs in the realm of practical reason, where arguments start from reasonable assumptions, where strict deductions are often not appropriate, where better arguments still do not claim deductive certainty. But this realm is not completely "dia-logical"; nor does agreement extend to a second level of argument about the topic of discussion. The Athenian proposal only to discuss matters of self-interest is described without objection as "scarcely consistent with" the expectation of a reasonable exchange of views in "a calm atmosphere." The Melians see the Athenian invasion of their island as a "present threat" of making war against them, evidence that they have come "prepared to judge the argument [them]-selves" (5.86). The Athenians do not deny this coercive element. A similar response occurs later (5.90), when the Melians note that the Athenians "force us to leave justice out of account and to confine ourselves to self-interest" right after the Athenians have asserted that for "practical people, the standard of justice depends on the equality of power to compel" (5.89). Evidently, Thucydides thinks like modern argumentation theorists who

Figure 1. Rescher's Simplified Formulation of Dialectical Move Sequences



Source: Rescher 1977, 6-15.

^aIt would also be possible to claim an entailment, such as $P \vdash S_1$ or (vis-à-vis move 1a) $Q \vdash S_2$, and then indirectly attack either P or Q with cautious or "provided" denials of S_1 or S_2 .

would consider this conduct "irrational in [such a] dialectical situation."³

I next want to show a continuity between past and present epistemologies that has been neglected by neorealist writers: despite the coercive irrationalities, the third level of substantive discussion in the Melian debate, the debate proper, can be partially formalized and scientifically rationalized according to Nicholas Rescher's pragmatic version of dialectical inquiry (Rescher 1977; another application is Alker 1984). I shall emphasize the practical, truth-revealing contribution of this mode of scientific discourse. Except for her neglect of formalizability, Jacqueline de Romilly makes the same point about dialectical creativity in clarifying Thucydides' achievement: "Thanks to the dialogue form, the only one in the whole of the History, the particular and the general mingle together. This form en-

ables maieutics to take the place of formal demonstration, so that the complete pattern Thucydides has in mind is revealed little by little, in all its aspects down to its deepest foundations" (1963, 274). The Greek root meaning of *maieutics* is particularly apt: "midwifery."

Rescher's Dialectical Logic

This demonstration requires first a summary of Rescher's formalization of the move and countermove possibilities within formal disputation, perhaps "the clearest, and surely historically the most prominent, instance of dialectical process."⁴ This formalization is illustrated in Figure 1. Rescher recognizes that other, more or less formal characterizations of rational argumentation exist (e.g., Lorenzen and Lorenz 1978). But this simplified version is adequate for his primary pur-

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pose: to exhibit the "social nature of the ground rules of probative reasoning," the establishing of our factual knowledge through a social process of controversy-oriented rational argumentation.

Rescher's dialectical logic starts from, and fits, a social-legal scenario involving four unequal, truth-seeking roles: the proponent, the opponent, the judge or "determiner," and an audience. The proponent tries to establish a thesis, which the opponent attempts to discredit by insightful questions, denials or counterassertions directed against the proponent's assertions or their deductive implications. The proponent, like a legal claimant under Greco-Roman law, bears the burden of proof. Not only must he or she make an assertion, or "commitment," as part of every turn and avoid repetitions, but these assertions should nontautologically develop the argument with new grounds, distinctions, supportive theses or conditional reformulations. (Reformulations, such as moves 1a, 5, 8, and 8' in Figure 1, are said to "discharge" the proponent's responsibility to defend a previous categorical assertion.)

The proponent's role, like his or her categorical assertions (symbolically preceded by exclamation points in the stylized tree of possibilities in Figure 1), is thus a "strong" one; the opponent's position is, relatively speaking, a "weak" one. Every unattacked assertion of the proponent becomes, at least temporarily, a "concession" by the opponent. Whenever the opponent makes a distinction as a way of limiting the scope of strong assertion, the stronger form of the assertion is also treated as a "concession." Although the opponent may develop counterarguments, he or she is not required to. The opponent's role requires only "weak" or "cautious" assertions (symbolically preceded by a dagger) at each turn. Characteristically, each cautious assertion may be reformulated as a question, that is to say, move 2 may be read as "Isn't the

opposite of your thesis consistent with everything you have so far asserted or conceded?"

Rescher's determiner makes judgments on either formal or material criteria: the former include illicit circularities in reasoning, inconsistencies and incoherencies within one's belief system, and formal logical errors; the latter focus on the plausibility or implausibility of the proponent's "living," i.e. undischarged or unconceded, commitments. Appropriate evidential plausibility standards for participants and judges include source reliability and authoritativeness; the relative strengths of supportive evidence for rival theses; and the simplicity, regularity, uniformity (with other cases), and presumptive normality of such theses, where these features are seen as supportive of inductive systematization. A proponent must deepen the grounding and improve upon the reasoning supporting his or her original theses; an opponent must be judged for success or failure in finding weak links in such support.

The judge additionally oversees the conduct of the argument. A major initial simplification (presumably adequate for Rescher's purposes in the early stages of his book) is the assumption that erroneous evidential claims either are not made by the participants or are immediately corrected by the judge. Thus the determiner role has implicitly limited the move possibilities of Figure 1, where, for example, no circularities of reasoning are exhibited. This assumption has the important consequence for Rescher's model of formal disputation that disputing parties address themselves to matters of evidential substance rather than to the proprieties of formal or evidential reasoning. Formally, this will mean that the major dialogical connective of presumptive support, the (weak or strong) assertion of P "provisoed" upon grounds Q, P/Q, is never directly attacked in his examples. Indeed, when such questions are directly

discussed in his later chapters, Rescher still does not provide a direct, formalized way of attacking P/Q . Only indirect challenges, involving the making of distinctions (or the allowing of exceptions) in the grounds supporting such assertions, are allowed. For example, although the strong exception move 8 of Figure 1, $P/(R_1 \& T_3) \& I(R_1 \& T_3)$, indirectly attacks the opponent's "provisoed" denial $\sim P/R_1$ in move 3, the focus formally is on the second part, the strong distinction $I(R_1 \& T_3)$, which "adds T_3 " to the opponent's weak assertion $\uparrow R_1$ in order to produce a strongly qualified reformulation, or "rewrite," of it.

Taking pains to subsume standard forms of sentential and syllogistic logic within his own approach,⁸ Rescher nevertheless emphasizes the differences between his formalizations and the standard contemporary one. Thus \sim symbolizes "negation" or "the denial of" the subsequent proposition, as it does in modern sentential logic, but for Rescher the weak assertion $\uparrow \sim P$ suggests a need for substantive "refinement," not necessarily total rejection. As we have just seen, Rescher's $\&$ also sometimes involves a much more substantial combinatorial rewriting than the modern, purely formal logical *and*.

The "provisoed" or presumptive inference from Q to P , P/Q , is very different from either the $Q \supset P$ (" Q implies P ") or $Q \vdash P$ (" Q entails P ") of standard modern symbolic logic. Consider the way P/Q is read: " P normally, generally, typically, or as a rule follows from Q , *ceteris paribus*." Because this key inferential relation is only presumptive, it does not unconditionally support detachment of a conclusion Q from $P \& (P \supset Q)$. Hence, "in dialectical (as opposed to deductive) reasoning an assessment of the cognitive standing of a thesis never leaves its probative origins behind altogether." Alternatively, without detachment each dialectical argument requires inspection of the entire history of its derivation.

A Partial Formalization of the Melian Dialogue

As a test of the applicability of Rescherian formalizations, I shall offer and then discuss a formalization of the first major substantive debate in the dialogue, whether the submission of Melos to Athens, either voluntarily or through coercion, can be said to be in each of their interests. An italicized Rescherian formalization of each of the seven "moves" or "turns" in that debate will be placed under the corresponding Crawley translation text, with my interpretive insertions in that text indicated by square brackets. (I leave the formal analysis of the second part of the debate, concerning the prudence of Melos's hope for deliverance, to others.)

Move 1. Athenians. (a) We will now proceed to show you that we are come here [seeking Melian submission, voluntarily, if possible] in the interest of our empire, and that [what] we are going say [is in your interest, i.e., is] for the preservation of your country;

Let P_1 = *Melian submission is in Athenian imperial interest* and P_2 = *voluntary Melian submission is in its (and Athens') interest*.

$IP = I(P_1 \& P_2)$

(b) as we would fain [preferably] exercise that empire over you without trouble [through Melos's voluntary submission],

Let Q = *imperially, Melos's voluntary submission is desirable*.

$P_1/Q \& IQ$

(c) and see you preserved for the good of us both.

Let R = *preservation of Melos via voluntary submission*.

$P_2/R \& IR$

Move 2. Melians. And how, pray, could it turn out as good for us to serve [submit, become slaves] as [it is] for you to rule?

Let $P_2 = P_{21} \& P_{22}$, where P_{21} = *Athenian interest in rule* and P_{22} = *Melian good from voluntary submission*.

$\uparrow(\sim P_{21} \& \sim P_{22})$

Move 3. Athenians. (a) [You would] avoid . . . suffering the worst;

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Let W = Melos's avoiding the worst.

$P_{22}/W \ \& \ !W$

(b) [we would gain a more valuable tributary] by not destroying [you].

Let X = greater Athenian tributary gain.

$P_{21}/X \ \& \ !X$

Move 4. Melians. [Why could we not remain] neutral, friends instead of enemies, but allies of neither side?

Let N = Melos's friendly neutrality and I = Athens' interest.

$\sim P_1/(I \ \& \ N) \ \& \ !(I \ \& \ N)$

Move 5. Athenians. (a) Your hostility cannot hurt us,

$I \sim (I \ \& \ N)$

(b) as your friendship will be an argument to our subjects of our weakness,

Let S = subjects' regard for Athenian power.

$\sim I/(\sim S/N) \ \& \ !(\sim S/N)$

(c) and your enmity of our power.

$I/(S/\sim N) \ \& \ !(\sim S/N)$

Move 6. Melians. Is that your subjects' idea of fair play—

Let $T = (\sim S/N) \ \& \ (S/\sim N)$, i.e., "that"

no distinction between people . . . unconnected with you and . . . your own colonists or else rebels whom you have conquered?

Let $\sim D$ = (making) no distinction.

$T \vdash \sim D \ \& \ !D$

Move 7. Athenians. (a) [Yes! Our subjects] see no difference between right and wrong;

$I \sim D$

(b) those . . . still . . . independen[t] do so because they are strong, and if we fail to attack them it is because we are afraid.

Let N_1 = still being independent, $\sim S_1$ = their strength, and N_2 = not being attacked by Athens; and $\sim S_2$ = Athens' being afraid to attack.

$\sim D/(N_1/\sim S_1 \ \& \ N_2/\sim S_2) \ \& \ !(N_1/S_1 \ \& \ N_2/S_2)$

(c) [So] that by conquering you we will increase [the] . . . size . . . [and] security of our empire. We rule the sea and you are islanders,

Let I_1 = increase in imperial size and security, I_2 = Athenian interest as sea ruler vis à vis island-

ers, and I_3 = particular Athenian interest vis à vis weaker islanders.

$I \vdash I_3$

(d) and weaker islanders too than the others; it is therefore particularly important that you should not escape.

$P_1/(I_1 \ \& \ I_2 \ \& \ I_3) \ \& \ !(I_1 \ \& \ I_2 \ \& \ I_3)$

The flow of Thucydides' argumentation evidences a truly remarkable fit with Rescher's rules of dialectical logic. As one kind of evidence, note the clear and repeated use of the specialized terminology of argumentation: *proceed to show*; *and how*, *pray*; *argument*; *distinction*; and *therefore* are terms of argumentative art that any broadly literate philosophical scholar will recognize. Secondly, there is the explicitly indicated "turn taking" mandated by Rescher; the more demanding requirement that previous arguments be met with new argumentation also holds. Although compound theses were not mentioned in Figure 1—they are an easy, natural and common complication of that formalism—the argument forms, from categorical assertions like (1a) to the "provisoed" counterassertions of (3a-b) and the weak distinction of move 4, clearly conform to Figure 1's evolving grammar of move, countermove, and reply possibilities.

Coincident with their major show of force, the Athenians are the proponent, they articulate the major theses of this discussion, and they offer an argument sketch for them. As opponents, the Melians regularly question, or weakly assert their views (2, 4, and 6). As I observed first hand in Uppsala at Lars Udehn's 1987 public doctoral thesis defense, the polite, cautious, carefully questioning, cooperative yet critical truth-seeking style of Melian interventions is still practiced in European universities 24 hundred years later! (Steven Lukes was the "opponent"; see also the neglected discussion of debates in Rapoport 1960.)

**Further Reflections on
This Correspondence**

Going into the details of the argumentation formulae above opens up a deeper examination of some key issues raised by Rescher's proposal. Thus one finds there strong support for the centrality of *ceteris paribus* inference in (at least classical) political argumentation, thought of as a kind of "provisoed" assertion that subsumes standard formal inference and encourages novel, more adequate substantive reformulations. As for Rescher's subsumptive approach toward standard formal logic, it is harder to map Thucydides' approach into the modern sentential calculus. But there are at least several cases where roughly equivalent versions of the same proposition, e.g., P_1 , P_2 , I, S, N, T, and D (and their negations) are logically substituted for each other in a modern way. Moreover, an apparently analytic claim appears when the Athenian interest in controlling islands is somehow deduced without further evidence from its interest and internal nature as a power who "rules the seas" (7c). (Aron 1984 [p. 29] nicely spells out the requirements for maritime domination.)

Beyond including something like standard deductive inference within his informal but formalizable dialectical logic, Thucydides here seems to have encompassed important aspects of the "internal relations" of Athenian domination. Briefly, "internal relations," like Hegel's master-bondsman relationship, are those that (at least partially) constitute the identities (or essences) of the entities being related. Various formalized in the Hegelian, Marxian, and analytical traditions, these identity-affecting logical relationships are usefully clarified in Bhaskar 1986, Elster 1978, and Ollman 1971. Since the argumentation shifts to another topic at this point, one is led to believe that at least the sketch of a deeper, provisionally plausible (and in effect conceded) explanation has been introduced.

Unlike standard formal logic, Thucydides' logic—his dialectic—is not tautological. Rather it is a rule and standard-governed, hence partially formalizable, practical epistemology. Within it there is always the possibility of new supporting or critical reformulations: both P_1 and P_2 are attacked and defended in a "provisoed" way, with (7d) being a very significant restatement of probative grounds for P_1 , clearly more persuasive than those offered in the preliminary argument sketch.

In this light, the Melian attempt to reformulate Athenian imperial interest so as to make it consistent with Melian neutrality—the weak distinction of move 4—is an especially creative, revealing, as well as practically crucial, reformulation.⁷ Not only does it make possible the knowledge enhancement due to the "internal relations" account of (7c), but practically it is a valiant, if ultimately inadequate and tragic, effort to deter Athens' attack.

Note that in the subsequent round the Athenians are put on the defensive, to the extent that they are induced explicitly to assert the denial of the Melian neutrality thesis. The argumentative requirement to support this new, negative claim produces (5b-c), which in turn allow the most revealing—and for a relatively neutral Greek determiner, probably decisive—move by the Melians: their inferential assertion of the Athenians' underlying warrant of no difference, no differentiation, no distinction in conduct expected from neutrals, colonists, and rebels, which at (7a) the Athenians clearly accept. For a society in which (according to Aristotle), justice is defined among educated citizens in terms of proportionality, or appropriate distinction, not only does this admission speak against the Athenians on this point, but it contradicts their own preference not to talk of the justice of their actions.

Rescher talks of dialectical negation as more than displacement, as "refinement." The formally correct law of double nega-

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tion ($\sim \sim p = p$) is pragmatically abandoned, replaced by the possibility of an original thesis being *aufgehoben*, preserved in a refined form through a feedback "cycle of probative dialectic" that arises out of constructive, critical debate (1977, 67). His claim that his controversy-oriented dialectics produces cumulative, new knowledge, is either clearly or tentatively supported by each of the admissions (7a-d) of the seventh round of the Melian debate.

Rescher's preference for a dialectics of probative reasoning, one that does not challenge "provisoed" assertions per se, however, seems somewhat too restrictive. If one recognizes that P_1 might aptly be respecified as I/M, where M = (Melian) submission, one sees more clearly the intimate connections between the earliest and later stages of the argumentation. Indeed, the challenges to P_1 can all be said to challenge this implicit assertive connective. (See Devereux 1985 for the next steps here; reliable formalizations concerning contextually inferred elements are not, and may never be, easy to come by.)

Consonant with the possibility of deeper, more internally or characteristically revealing reformulations, a contemporary restatement of Thucydides' dialectical logic is as follows: Think (like Thucydides) of reality as layered and define scientific progress as the uncovering of the deeper, dialectically inner, determining layers or characteristic dispositions. (Systemic, political, economic, sociocultural, and ecological relationships may also play such inner, identity-affecting roles.) Recognizing that political systems are open and "autopoietic" (Bhaskar 1986), hence not apodictically decomposable, it is appropriate that investigative inferences are all in the classically tentative, *ceteris paribus* mode of reasoning rather than the ontologically flat statistical sense of *ceteris paribus* relationships going back to Ando and Fisher 1962 and Ando, Fisher, and Simon 1963. Be-

cause classical *ceteris paribus* reasoning corresponds closely to the "default reasoning" and "nonmonotonic inference" of contemporary artificial intelligence research on commonsense reasoning, which allow rationally defensible, nonadditive reconceptualizations, I am optimistic that Rescher's approach can be improved upon by more rigorous, internality-sensitive formalizations of dialectical, political reasoning (see Winograd 1980 and Reichman 1985).

Other Dialectical Elements in Thucydides' Historiography

We have now a better understanding of Thucydides' "logic," in the classical, dialectical sense of his (only partly formalizable) standards and procedures of valid argumentation and knowledge cumulation. In the one case where we have carefully examined its exercise, it has moved us more and more toward his ontology of things, his conception of the inner character of imperial systems, states, and statesmen. We have not yet clarified and explained the exceptional, dramatic, moral force of the Melian dialogue. Nor have we linked Thucydides' exceptional historical analysis of the Melian episode to the rest of his book. Each of these concerns also has a dialectical aspect.

Events Data and Speeches in Thucydides' Science

Let us begin with the way Thucydides interrelates events and speeches. Just as Finley observes (Thucydides 1951, xii), Thucydides' historiography "uniquely combined opposite tendencies of dialectic and observation, of generalization and observation." Throughout the book, speeches regularly come before the great actions (or "motions"). This sequencing is an explicit aspect of his interpretive, explanatory methodology: "My habit has been to make the speakers say what was

in my opinion demanded of them by the various occasions, of course adhering as closely as possible to the general sense of what they really said" (1.21).

But this search for contextual appropriateness does not override the awesome sense of free, responsible, criticizable moral agency that this sequencing also conveys. Given the likelihood that many of the speeches were written or rewritten after the narrative was nearly finished (as Jaeger [1976, 342-47] and Romilly [1963, 275-86] argue), dialectically it is highly plausible to look for Thucydides' own critical interpretations of events, their motives, and consequences in the oppositions apparent in almost all of them. Indeed, the central role of the Melian massacre in postwar denunciations of Athenian imperialism suggests to Romilly a distinctive, quasi-legal significance for Thucydides' last revisions of the work, making it a "study [of] the policy of conquest as a whole, in relation to certain criticisms that had recently been made of it" (p. 286).

Does not Thucydides' account of Athens' response to the Mitylene revolt (a more serious affront than Melian neutrality, but occurring earlier in the war [3.1-50]) contrast favorably with Athenian behavior at Melos? Cleon, "the most violent man at Athens, and at that time by far the most powerful with the commons" argued for the same standard as implemented in Melos—killing all male defenders and enslaving the others—with similar statements like "if, right or wrong, you determine to rule, you must carry out your principle and punish [all] the Mitylenians as your interest requires"; but Diodotus recommends "moderate chastisements," killing the oligarchic leaders of the revolt, not potentially pro-Athenian democratic masses, who had indeed turned against the Mitylenian oligarchs. Diodotus carries the day in a close vote with a final appeal that "good policy against an adversary is superior to the

blind attacks of brute force" (3.49). Given Thucydides' stated preference for a much more limited form of democracy (8.97), Cleon's excesses may be seen as structurally linked to a commons that is too large.

Even more momentous comparisons critically link Athenian rationalizations in the Melian episode with Pericles' original statements of Athenian war aims and the immediately following Sicilian debate between Nicias and Alcibiades, the principal instigator of the fateful Sicilian invasion. Nicias argues against "risking things present for the sake of things future and uncertain," against the "madness of attacking a land which, if they prevail, they cannot hold," and against falling "sick of a fatal passion for what is beyond your reach" (6.1-13). Alcibiades, on the other hand, not long after having helped undermine Nicias's interim peace with Sparta by a spiteful trick (5.44), boasts that his "folly" (or "madness") brought benefits in alliances against Sparta and asserts that the Sicilian cities are unpatriotic, inhabited by "motley rabbles." He defends his extravagant sending of a record seven chariots to the Olympic games, scorns Nicias's do-nothing policy, calls for supporting their allies on distant Sicily, suggests that the initiation of a second front will "humble the pride of the Peloponnesians," and concludes that "a city not inactive by nature could not choose a quicker way to ruin" than inaction: "The safest rule of life is to take one's character and institutions for better and for worse, and to live up to them as closely as one can" (6.17-20). The interest of the multitude in the allies' exaggerated (and not disinterested) reports of great wealth in Sicilian temples and treasuries helps Alcibiades carry the day.

How clearly this debate, which repeats the theme of post-Periclean Athens' inherent aggressiveness, ironically replays the devastating strictures against groundless hopes in the Melian dialogue! How Alcibi-

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ades' remarks contrast unfavorably with Pericles' speech at the beginning of the war! Like Nicias much later, Pericles had been "more afraid of [Athens'] own blunders than of the enemy's devices," possessing many "reasons to hope for a favorable issue, if [the citizens] can consent not to combine schemes of fresh conquest with the conduct of the war" (1.143). On this contrast, Thucydides himself makes a rare explicit judgment, not in a speech, but in comments that constitute his encomium for Pericles at his death, 2½ years after the war began:

The correctness of his previsions . . . became better known by his death. He told them to wait quietly, . . . to attempt no new conquests. . . . [Rather,] what they did was the very contrary, allowing private ambitions . . . to lead them into projects whose success would only conduce to the honor and advantage of private persons. [Moreover,] committing even the conduct of state affairs to the whims of the multitude . . . produced a host of blunders, and amongst them the Sicilian expedition; though this failed not so much through a miscalculation of the power [of the Sicilians] . . . as through a fault in the senders, . . . choosing rather [than continuing to help] . . . to occupy themselves with private cabals for the leadership of the commons, . . . [which] first introduced civil discord at home. (2.65-66)

The above extensive and rather exceptional quote confirms, then, the interpretation we have made on other grounds, that deliberative speeches like the Melian dialogue are to be compared for their implicit judgmental implications about the motives of the principal actors in Thucydides' history (see Mefford 1985).

Thucydides' Symbolic Oppositions and Principled Contradictions

Frequently, the debaters are leaders of different city-states or their contending factions; sometimes they are spokespersons for—or almost mythical symbols of—a frail but larger collective will politically composed out of contradictory inputs. Finley elaborates an oppositional

theme we have already seen in the Melian dialogue's explanatory account (Thucydides 1951, xii-xiii): Athens is characterized as a naval power, based on an extensive commercial economy and political democracy (excluding women and slaves but typically including many of the militarily and economically necessary oarsmen); Sparta's nature is that of a land power, based on helot agriculture, controlled by a conservative oligarchy and a weak, rotating monarchy. Athens "encourages enterprise and initiative; Sparta emphasizes tenacity and tradition." These differences are discussed at length in the introductory sections (5.84-85) and the later parts (5.97, 99, 107-11) of the Melian conference.

Strauss puts some of these oppositions (and others) into a partial hierarchy of dialectical distinctions: "Just as humanity divides itself into Greeks and barbarians, Greekness in its turn has two poles, Sparta and Athens. The fundamental opposition of motion and rest returns on the level of Greekness; Sparta cherishes rest whereas Athens cherished motion" (1978, 157). Even more remarkable (and extreme) is his argument, "[In] the Peloponnesian War . . . one sees Greeks at their peak in motion; one sees the beginning of the descent. The peak of Greekness is the peak of humanity. The Peloponnesian War and what it implies exhausts the possibilities of man. . . . All human life moves between the poles of war and peace, and between the poles of barbarism and Greekness. By studying the Peloponnesian War Thucydides grasps the limits, . . . the nature of all human things. It is for this reason that his work is a possession for all times (1978, 157).

Despite their ethnocentric excess, consonant with our Rescherian analysis, these truly remarkable quotations point to the deepest answers Thucydides gave to the quest signaled by his words quoted in my opening paragraph. Raymond

Aron has made rather similar points: "Politics is dialectic when it unfolds between men who mutually acknowledge each other. It is war when it brings into opposition men who . . . wish to remain strangers to one another. . . . By the same token, we perceive why war is the completion of politics and at the same time its negation" (1984, 23). The common thought is at once dialectical in its revelatory search for fundamental, substantial oppositions and transformations and hermeneutical in its reconstructive suggestions of a grammar of motives, and of opposed organizing principles spanning or generating the space of possible human political activity (see Alker 1982 and Habermas 1973).

Thucydides' Greek Tragedy

At last we are ready for the crucial second thesis in this reconstruction of the Melian dialogue: Thucydides' scientific study of power politics is essentially dramatic and ultimately tragic. Because the failures of both the Melians (a Spartan colony) and their opponents, the Athenians, are revealed in that dialogue, a larger Greek tragedy is suggested by it.

The first argument is a textual one. At the same time that it is a turning point in Thucydides' entire "scientific history," the Melian dialogue is pure drama (cf. Aron 1984, 28). To quote Cornford, at the point in the Melian conference where the dialogue begins, "the historian changes from narrative to full dramatic form, prefixing, as in a play, the names—'Athenians,' 'Melians'—to the speeches" (Cornford 1971, 174–87). The dialogue was in fact a principal feature of Greek tragedy of the period as practiced by Euripides and others. By Thucydides' shifting into this form, Cornford argues, the dramatic ironies of this pivotal turning point are highlighted. These concern both the blind hubris, Eros, insolence, and pre-Christian

hope infecting both the Athenians and the Melians.

The second argument for the dramatic nature of Thucydides' Melian dialogue is more substantive, with essentially the same tragic conclusion. It begins by noting how Thucydides' use of dialectical substance, "the overall category of dramatism," conforms to the dramatist's search for the roots of human action in verbal terms, for instance, symbolic oppositions, and principled contradictions. Strauss and Aron have already shown us how, "when men are treated in terms of other things, men may even be said to speak for the dumb objects [or forces] of nature" (Burke 1969, 33).

Burke then offers a directly relevant, dialectical conception of tragedy (and science):

Galileo speaks of experimental testing as an "ordeal." Stated broadly, the dialectical (agonistic) approach to knowledge is through the act of assertion, whereby one suffers or calls forth as counter-assertions the kind of knowledge that is the reciprocal of his act. This is the process embodied in tragedy, where the agent's action involves a corresponding passion, and from the sufferance of the passion [by the original agent or the empathetic observer] there arises an understanding of the act, an understanding that transcends the act. In this final state of tragic vision, intrinsic and extrinsic motivations are merged. . . . Although purely circumstantial factors participate, . . . they bring about a *representative* kind of anecdote . . . that belongs with the agent's particular kind of character. (1969, 33)

As a Burkean "representative anecdote," the Melian episode reveals the blindness of "tyrannical Eros," of blind Athenian hubris and its insolent defiance of the gods. (Cf. Burke 1969; Cornford 1971; and Strauss 1978, 225–27; Alker [1987] suggests formally reconstructing Thucydides' narrative in terms of the fundamental tragic notion of a "mythical actor's self-annihilation.")

Burke even claims that "we can . . . catch glimpses of a relation between dialectics and [Platonic] mathematics . . . in the fact that *mathemata* means both

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things learned . . . and the mathematical sciences" and that "we can discern something of the 'tragic' grammar behind the Greek proverb's way of saying 'one learns by experience'; '*ta patemata mathemata*,' the suffered is the learned" (1969, 39-40). As Strauss says, the Peloponnesian "War surpassed the Persian War in regard to human suffering," caused (intrinsically) by men and (extrinsically) by nature (1978, 150).

It turns out, then, that the Melian dialogue is a key scene in a classical morality play about might and right, not simply an eternal statement of the mathematical truths of *realpolitik*. An increasingly blind, arrogant, lustful, imperious Athens will pay for its failings with the lives of many of its citizens and, eventually, with its independence as well. Like the most thoughtful modern realists, Thucydides joins normative, descriptive and explanatory investigations pointed toward the suffering-based "learning" of moral lessons.

On the Appropriation of the Classics

In a recent, postmodernist treatise, *On Diplomacy*, James Der Derian has attempted "to explain a[n] international political system by studying the genesis of its internal relations, which are seen as expressions of alienated powers." A student of a leading classicist of our era, Hedley Bull, Der Derian, too, cites the authority of Aron and Thucydides as support for his own approach, quoting a passage pointing ironically to subsequent Athenian injustice: the Corcyrans say to Athenian and Corinthian representatives on the eve of the outbreak of the Peloponnesian War, "Every colony, if it is treated properly, honours its mother city, and only becomes *estranged* when it has been treated badly. Colonists are not sent abroad to be slaves of those who remain

behind, but to be their equals" (1987, 2, 6; Der Derian's emphasis). As equals, they could exhibit their practical, moral capacities for arriving at reasonable, just bases for concerted political action in times of need. His book thus suggests that certain aspects of a neoclassical orientation can legitimately be attributed to the present effort as well.

Neoclassical Polimetrics

I have objected to the view of mathematical *realpolitik* derived from Thucydides' work and attributed to him by many of Thucydides' realist and neorealist interpreters. By showing how much of his analysis they (and deductive behavioral modelers like Zinnes [1975]) have lost, I have tried to motivate and to legitimate a more comprehensive form of rigorous, partly formal, political analysis. The dialectical methodology of Thucydides' classic work has been available in principle since the dawn of scientific history, yet the above formal analysis of the Melian dialogue still seems, to this author, full of as yet unrealized potential.

For one trained in the powers of formal analysis, that exercise suggests what a neoclassical polimetrics of political activities and constitutional structures would look like (cf. Alker 1975). It would be a polimetrics dialectically grounded in human experience, focused on contextually appropriate practical-normative standards of just conduct and institutional worth. Whether addressing empirical topics or arguing policy alternatives, its preferred "logic" would be argumentation theory, using classical *ceteris paribus* reasoning. Quasi-experimental inference, conventional mathematical probability and statistics, and deductive modeling would be seen as special cases or components of this more general approach. (Dunn [1982] anticipates many of these arguments; Campbell [1982] concedes most of them.)

We have seen how political argumentation, if grounded in a cooperative, uncoerced, truth-seeking orientation and used skillfully to ask the right questions—those that critically probe the essential justifications—can suggest key determinants and consequences of socio-political identities, actions, relations, institutions, or contexts. Polimetrics built up from a foundation in political argumentation can help reveal the underlying, contestably legitimate political orders (constitutions), the principled practices and constitutive relationships that, if known, will make possible further, even more penetrating, policy-relevant discussions of a critical and reconstructive sort. Its cumulative results are unlikely to be timeless laws; its universals are more likely to reflect hegemonic or pluralistic political achievements.

Der Derian's genealogical approach is justified as an effort to refute the existence of any ahistorical, timeless, defining essence of international politics. To study discursively, genealogically, historically the internal relations of power politics is to deny its eternal reality. My own re-reading of Thucydides' Melian dialogue suggests a similar, open-ended, dialectical epistemology of practical knowledge cumulation, ideally to be combined with competing sets of hypotheses concerning the inner and outer determinants of Athenian (and Spartan) actions. It thus locates itself where any epistemologically self-conscious contemporary approach to international politics ought to be—in the midst of contending claims about lessons of the past and possibilities for the future.

Thus we too may participate in the engaged, dramatic, but relatively objective and rigorous kind of historical political analysis that Thucydides was engaged in. Like the Athenians and their Greek friends before the war, we can scientifically appeal to dialectical judgements, relatively unconstrained argument and counterargument, "impartial laws" . . . [and]

differences settled by arbitration" (1.76–77). We can join different sides of the inner contradictions of "Greeks" and "barbarians," enlisting Thucydides' (or Euripides' even more antimilitaristic) insights in our own political-scholarly debates. By dialectically engaging ourselves in the endless, passionate search for objectively accurate and motivationally superior historical accounts, we critically renew the past and help create the future.

Notes

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1. The customary form of citation is to the books and paragraphs of the Greek text. Except where a secondary source is cited, my quotations are taken from the Crawley translation (Thucydides 1951), now in the public domain.

2. Jaeger (1976, 483) argues that Hecataeus first took "the scientific and rational approach to the facts of human life as the essence of history," while Herodotus gets the credit for introducing "the religious and dramatic element" into history. A more globally valid view should take into account Chinese Confucianism, Legalism, and Taoism (Freiberg 1977).

3. Barth and Krabbe (1982, 58) associate with the opponent's role "an unconditional right to criticize" proponent's assertions, and reconstruct the pragmatic rules of logical argumentation to include rule FD E5super: "When party N performs a speech act that is not among those permitted to N by the rules of this system of formal dialectics, or if it performs a non-permitted, nonverbal action that reduces the other party's chances of winning the discussion, then N has lost all its rights in the discussion and N's behavior is to be called *irrational with respect to the present dialectical situation* by the company that has adopted this system of formal, dialectics" (p. 63).

4. My partial summary of Rescher's (1977) argument, including quotations, are taken primarily from chapter 1, pages 1–34; see also chapters 2 and 5.

5. Indeed, Rescher suggests that his formal dialectic

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tics goes nicely with Aristotle's formal syllogistic reasoning, modified to include an "all-things-being-equal" operator (1977, 13-14). He follows Aristotle on this point, who argues that syllogistic principles apply both to axiomatic demonstration and to dialectic (Aristotle 1964, 153-59).

An even more ambitious synthesizing move between the dialectical and standard, that is, formal and deductive, logical traditions is due to Paul Lorenzen, who defines the standard logical constants $\&$, \vee , \sim , $\text{if} \dots \text{then}$ —dialectically (i.e., argumentationally), in terms of strip rules for their use in "critical dialogues" between proponents and opponents. "Such a 'social' definition of the logical constants may be regarded as a theoretical elaboration of Wittgenstein's notion of a *language game*." It suggests a new "dialogical or argumentational 'garb'" for "modern propositional and predicate logics" shown by Paul Lorenzen to be demonstrably equivalent to them. Lorenzen equates "classical logic with a logic of cooperative debates (dialectics, in Plato's and Aristotle's sense), and [twentieth-century] intuitionistic (constructive) logic with a logic of competitive debates (eristics)" (Barth and Krabbe 1982, 12, 24, 55). The set of 21 definitions and 40-odd rules proposed for bringing both Cartesian monological axiomatics and post-Hegelian associative thinking into the discursive, socially and rationally arguable realm is given in chapter 3 of the Barth and Krabbe text, from which one rule was cited in note 3. The relevant original papers are collected in Lorenzen and Lorenz 1978. Further integrations and elaborations, of direct relevance to political science, are clearly needed.

6. Apparently, Sergeev's recent, rich, philosophically informed formalization of Thucydides' argumentation omits the discussion of internal relations (Sergeev 1986). His published paper cites the European literature of note 3 and the 1980 version of the present paper.

7. "Recognition of the central role of distinctions in the dialectical enterprise—based on the division (*dihairesis*) of key concepts—goes back at least to the Socrates of Plato's *Phaedrus* and is doubtless present in the theory and practice of the early Sophists" (Rescher 1977, 12, n. 13). Later (pp. 66-67), Rescher's description of "constructive negation" exactly fits the creativity of Melian distinction making: "When P/Q is succeeded by $\sim P/(Q \& R)$, there is not just the displacement from P to $\sim P$, but also the refinement (amplification, improvement) from Q to $(Q \& R)$. . . : it advances the discussion and shifts the issue onto a more sophisticated ground."

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CAN LIBERALS PUNISH?

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Liberalism doubts that a sound theory of politics can be built from a theory of the human good. In pursuit of its authorized ends, the liberal state can establish incentives and disincentives, it can require reparations, and it can restrain dangerous persons. But can liberals punish? By distinguishing punishment from related phenomena and comparing its presuppositions with central tenets of liberalism, I tentatively conclude that they cannot. An analysis of efforts by leading liberal theorists to come to terms with punishment confirms that suspicion: their theories do not authorize punishment or do so at the expense of their more fundamental principles. I conclude that the ability to punish criminals is part of a larger moral experience and capacity that is lost with liberalism's expulsion of the human good from politics, and further, that without reference to the human good, liberalism's central concepts of liberty and equality are left weak and empty.

Liberalism is a doubting philosophy of politics. It doubts that the human good, or human excellence, exists; or if it does exist, it doubts that we can know it; or if it does exist and we can know it, liberalism doubts that we can ever know it well enough or gain sufficient consensus about it to provide a stable base for political life. As the founders of liberalism knew only too well from the religious wars, when the right way to live becomes the focus of politics, differences of opinion may yield to conflicts of arms, and anger can be spurred to fanaticism. Thus liberalism builds its theory of politics as distant as possible from a theory of the human good. Though liberalism is doubting, it is not apparently disabled—certainly not, it would seem, in practice. Under its title has grown the most powerful and prosperous nation in the world's history. Where it has prevailed, science and technology have flourished, politics have been

stable, and life in general has been healthy, peaceful, and comfortable.

Nor does its doubt seem disabling to its theory of politics. Indeed this very doubt about a human good emboldens its claim to reveal a dignified and stable base for political life.¹ This confidence expresses itself in a variety of forms: Locke's natural rights of life, liberty, and estate; utilitarianism's greatest happiness of the greatest number; Kantian and—more significantly today—neo-Kantian moral rights of equal concern and respect. To be sure, there is disagreement among these theories: for Locke and his progeny, the state is confined largely to the task of securing physical protection of the individual and establishing rules for the acquisition, protection, and transfer of property; while for neo-Kantians and utilitarians the state may, even at some expense to Lockean rights of property, pursue ends expressing an aggregation of the people's "personal preferences."² But

more striking is the agreement;³ and this consists in an affirmation of the principles of liberty and equality, the importance of the individual over the community, and the idea of the neutral state, that is, the idea that the state should set its policies as far as possible from any particular notion of the human good. On this neo-Kantians and Lockians agree. For Ronald Dworkin, the fundamental neo-Kantian right to equal concern and respect means that government "must not distribute goods or opportunities unequally on the ground that some citizens are entitled to more because they are worthy of more concern. It must not constrain liberty on the ground that one citizen's conception of the good life . . . is nobler or superior to another's" (Dworkin, 1978, 272-73). Similarly for his fellow neo-Kantian, Bruce Ackerman, "Neutrality [always capitalized in his work] forbids citizens from justifying their rights by asserting the possession of an insight into the moral universe intrinsically superior to that of their fellows" and "forbids the legal recognition of any right that requires its holders to justify its possession by declaring themselves intrinsically superior to their fellow citizens" (Ackerman 1984, 99).

And for the natural rights tradition of Locke, as articulated by Walter Berns, free and equal people form a covenant where "each man agrees that his opinions of good and bad, right and wrong, justice and injustice, are just that—private opinions—and the principle of equality requires him to acknowledge that other opinions, even if they are contrary to his own, have as much (or as little) dignity as his own" (1982, 60). Since judgments about the human good are regarded as merely personal and inherently arbitrary,⁴ laws, especially the fundamental law, must be "neutral."

So, in pursuit of its authorized ends, the liberal state can enact and enforce laws; it can make people whole whose rights have

been trespassed; it can restrain or incapacitate dangerous persons; it can establish incentives and disincentives for engaging in certain activities. But can liberals punish? Can they punish without overreaching the bounds of the individual upon whom they set their state, without appealing to notions of a human good that they set apart from their state, or without vindicating the anger that their peaceable form of politics seeks to tame?

The Meaning of Punishment

To answer this question we must consider what punishment is, as distinguished from similar phenomena, such as taxes, reparations, and penalties. Bright-line distinctions are not to be expected, for the phenomena blur and blend at the margins; yet their central and distinguishing features can be made out rather clearly. Taxes (or licenses), like punishments and penalties, may discourage activities, but they do not usually prohibit them. Licenses or taxes in effect say, "If you wish to engage in conduct A, then you must pay fee X or take exam Y"; punishments and penalties say conduct A is not to be done. Of course, the distinction might blur. Taxes may be prohibitive and the penalties may be trivial, so in reality the real messages are reversed. But nominally and usually, penalties and punishments are easily distinguished from taxes.

Penalties, of course, lie closer to punishment than do taxes but are still distinguishable. While penalties say that some conduct is not to be done, typically, as for example with speeding tickets for marginally exceeding 55 mph, that message is given with less seriousness than in the case of punishments; the hardship imposed, the shame attached, the rigor with which we are held to the mark, and the vigor with which violators are pursued all are

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less. And not uncommonly the penalty is accompanied with other implicit messages that bushion the impact of the penalty, assuring you that though you broke the law, you are really not a bad person; that anyone could have done this sort of thing, that it could have happened to anyone, or that the penalty is a risk you run for engaging in a certain line of conduct and for public benefit you have to be held to the mark. As suggested by these messages, the lawmaker, in framing penalties, will often consider strict liability a reasonable option; whether you did the action or not becomes the only question, not whether you were justified or whether you had an excuse. But where punishments are involved, justification and excuse are essentially considerations, and strict liability is virtually unthinkable.

The availability of strict liability for penalties points to its essential difference from punishment in objective and focus. The focus of a penalty is the future and its principal object is to deter. This can be done better if would-be violators know that they can't slip through the system by faking an excuse or justification. Because the offender may have had good reason or excuse, strict liability must occasionally be unfair. But exactly because the offense in question is not thought "serious" in the sense of reflecting deeply on the character of the offender, the loss in fairness is balanced by the gain in deterrence.

If the primary focus of a penalty is the future (detering future offenses), the primary focus of a punishment is the past (what was done and why it was done). In looking back, punishment resembles reparation. But reparation looks back in order to make the victim whole, and it does this typically as a matter of private law; reparation is not essentially a concern of the public. Punishment, if it does make the victim whole does so metaphorically and as an incident of its essential public concern. In sum, punishment appears distinct from taxes, licenses,

penalties, reparation, and methods of deterrence; apart from its similarities, there is something left over, something that cannot be made up by extracting elements from the other phenomena similar to it.

Perhaps the most useful access to the phenomenon has been given in Joel Feinberg's identification of the essence of punishment with its public, authoritative expressive function, its "expression of attitudes of resentment and indignation, and of judgments of disapproval and reprobation" (Feinberg 1970, 98). Though its two elements, attitudes of indignation and judgments of reprobation, function together, the expressive function can be clarified if we consider them separately.

Attitudes of resentment and indignation direct anger toward the person *responsible* for the crime. If the person has a complete excuse (e.g., good faith mistake or total accident) or a justification (e.g., self-defense), anger is inappropriate. Only if the person is responsible do we feel anger and seek revenge. As Walter Berns has said, "We can become angry with an inanimate object (the door we run into and kick in return) only by foolishly attributing responsibility to it, and we cannot do that for long, which is why we do not think of returning later to revenge ourselves on the door" (1979, 153).

Anger, according to Aristotle, is "an impulse, accompanied by pain, to a conspicuous revenge for a conspicuous slight directed without justification towards what concerns oneself or towards what concerns one's friends." It is also, he noted, accompanied by a "certain pleasure" arising from the anticipation of revenge (1954, 1378). While anger may be for a conspicuous slight to oneself, in a narrow and personal sense, the anger that underlies punishment at law is for the wrong done more broadly to "what concerns oneself" or one's

"friends." This anger displays a caring for persons and matters beyond one's narrow self-interest and expresses the desire to vindicate them. It does not fix to the harm a person or a principle suffers, at least not directly, but to the cause of that harm in the action of a responsible human being and the attitude displayed in that action for the things we care about. ("Contempt, spite, or insolence" are the three forms of slighting mentioned by Aristotle.) Punishment expresses and satisfies that longing for vindication.

As James Fitzjames Stephen wrote, the criminal law gives "shape to moral indignation, and hatred of the criminal. . . . The sentence of the law is to the moral sentiment of the public what a seal is to hot wax" (1883, 81). The criminal sanction gives proper shape or form to anger, measuring it according to what Feinberg identifies as the second element expressed by punishment, the "judgments of disapproval and reprobation." That judgment is essentially one of desert, that the deed deserves hard treatment, shame, and reprobation in proportion to the gravity of the crime. This stern, measured judgment makes punishment what it is. "One may lose more money on the stock market than in the courtroom; a prisoner of war camp may well provide a harsher environment than a state prison; death on the field of battle has the same physical characteristics as death by sentence of law" (Gardner 1953, 193). It is the condemnatory judgment alone, expressing the ideas of desert and righteous vindication, that renders "mere" hardship a punishment.

But what is meant by the term *desert* in this context? As William Galston has noted "every desert-claim is of the form: A deserves x in virtue of f " (1980, 170). Here x , as suggested above, is the hard treatment embodying the vindictive resentment and reprobation of the political community. But the f , the nature of the crime, and the relation of x to it

need to be explored further to understand the "fitness" implied in the word *desert*. Recalling Aristotle's definition of anger, we see that it is directed to a "slighting"—an act destructive of what we care about, with an attitude of insolence, contempt, or spite. What is crucial is not the destruction alone but the attitude, or state of mind, with which it is done. A killing may be an occasion of regret but not anger if it is totally accidental; it may even be the occasion for celebration under certain conditions. Only if the killing displays the wrong attitude toward life does it occasion the desire for revenge and a sense of "fitness" of punishment.

It is proper then to see punishment and blame as the opposite of rewards and praise.⁸ We praise and reward people to express our appreciation of their deeds and our admiration of the beauty of character their deeds bespeak, elevating, displaying, preserving, rendering more secure the things we care about. We punish and blame people to express our resentment and disapproval of their deeds and our detestation of the ugliness of character their crime bespeaks. At both ends of the spectrum we intend the deserved treatment to penetrate deep to the self, to mark our judgments of the person in virtue of the deed. In this way rewards differ from compensation and incentives; and punishments differ from reparation and disincentives. Compensation and incentives, reparations and disincentives all do not, and are intended not to, reflect deeply on the person; they are largely external to the self. Though liberalism has gone far to diminish the range of the self that can be publicly recognized, for most citizens the self appears more expansive and they wish to mark its dimensions; they wish to erect monuments of gratitude to reward the greatest of heroes with immortality through fame (Hamilton, Madison, and Jay 1961, 432, 437) and to punish the vilest of criminals with death. Punishment

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thus invokes a vision of the moral order where the virtuous flourish and the wicked suffer (cf. Feinberg 1970, 217; Ross 1930, 56-65).⁶

Why Liberals Can't Punish: Prima Facie Considerations

My purpose has been not so much to justify punishment as to describe the phenomenon and thereby indicate the sort of justification it requires. But if this description is accurate, we can readily see why punishment should trouble liberals. First, punishment expresses and satisfies righteous anger; liberalism, as witness to the horrors of religious warfare, seeks to eliminate as far as possible the role of anger in political life and refuses to countenance its expression. Liberalism does this by taking out of political life the things men and women love most and thus would be most likely to become angry about—God, country, virtue—and focusing instead on the relatively tame concerns of individual, material well-being (see, e.g., Berns 1979, 150-51; Franklin 1964, 149-50; Montesquieu 1949, bk 20, chap. 1). Punishment transcends these concerns, for we punish not just to discourage future wrongs to our well-being but to express our indignation and reprobation at the wrong that has been done.⁷

Second, punishment concerns the public, the community;⁸ liberalism builds from the individual. Punishment vindicates the community in its standard of right, not the individual in his or her personal concerns or even personal rights. Liberalism has no trouble explaining reparation and restraint (see, e.g., Locke 1952, 2.8, 11), for these do build from material concerns and rights of the individual; but punishment outstretches these.

Third, punishment inflicts pain on the criminal distinct from what is necessary

for deterrence or rehabilitation, and it does this in virtue of the character displayed in the criminal act; it thus requires what Dworkin has called an "external preference." Liberalism opposes these (Dworkin 1978, 275; Hart 1962, 65). As Mill stated liberalism's "one very simple principle," "the sole end for which mankind are warranted, individually or collectively, in interfering with the liberty of action of any of their number is self-protection. That the only purpose for which power can be rightfully exercised over any member of a civilized community, against his will, is to prevent harm to others" (Mill 1959, 13). More recently, Feinberg has persuasively extended the liberal case for "the moral limits of the criminal law" from Mill's "harm principle" to what he calls the "offense principle" (Feinberg 1985). But whether harm or offense to the individual marks the outer limits of liberalism's use of the criminal sanction, that authority cannot reach beyond the hard treatment necessary for reparation and restraint; it cannot warrant the distinct pain resulting from the "external preference" expressive of reprobation and resentment.⁹

Fourth, and closely related to the preceding points, the pain that punishment delivers to the criminal bears a statement about the human good and excellence and this individual's relation to it; policy based on such visions and desert in virtue thereof are precisely what liberalism seeks to avoid.¹⁰ In fact it is this point that undergirds the others; for it is this vision of human virtue and vice that fuels the anger, that sustains community judgment, and that vindicates the pain visited upon the criminal through punishment.

How Do Liberals (Try To) "Punish"?

If I am correct so far, punishment is fundamental to our social experience, but

the principles of liberalism disable it from punishing. My characterizations of liberalism, however, might underestimate its variety and resourcefulness. Thus it is useful to consider the distinct accounts of punishment offered by leading liberal thinkers to determine first whether liberals do in fact authorize punishment (as distinct from "penalties") and second if they do, whether it is not with serious damage to other principles that they affirm.

Although Locke uses the words *retribute* and *punishment*, he makes no pretense that his theory of politics would authorize punishment as I have described it. Natural rights authorize one to "retribute to [a criminal], so far as calm reason and conscience dictate, what is proportionate to his transgression, which is so much as may serve for *reparation* and *restraint*; for these are the only reasons why one man may lawfully do harm to another, which is that we call punishment" (Locke 1952, 2.8, my emphasis). Not surprisingly those who have followed in his footsteps do not authorize punishment either. Richard Posner, the leader of the law and economics movement, finds an adequate explanation of all fundamental concepts of criminal justice, whether ancient (1981, 207-27) or modern (1985, 1193-1231); in terms of whether they are "functionally efficient," given the society's stage of development, towards the larger goal of "wealth maximization." The neo-Lockean Robert Nozick, while placing significantly greater emphasis than Posner on individual rights, similarly avoids real punishment. For him the criminal law properly begins only at the point where the mechanisms of private, tort law prove inadequate for making individuals whole. This is the point where there is "uncompensable fear"¹¹—where reparation is rendered impracticable by the nature of the wrong (such as violent assault) or the number of people involved (the countless third parties suffering fear

of the assault, especially if they know there will be no greater penalty than medical costs and lost time compensation). But the ends of private law—reparation and restraint—remain the same; criminal law is simply a continuation of the same by other means (1974, 54-78).¹²

In marked contrast to Locke, neo-Lockeans, and utilitarians, Kant eschews any essential concern with deterrence or any other consequence of punishment and embraces instead a purely retributive account; he thus challenges the thesis that liberals cannot punish. Vindication of the moral law must be the focus and "woe to him," he warns, who rummages through the serpentine paths of utility looking for advantages and disadvantages of punishment (Kant 1965, 100). For Kant, retributive punishment follows from his categorical imperative as an immediate corollary. As a criminal you act directly contrary to a principle you could deem universal law, and your measure of punishment is directly proportional to your distance from that categorical imperative. "Any undeserved evil that you inflict on someone else among the people is one that you do to yourself. If you vilify him, you vilify yourself; if you steal from him, you steal from yourself; if you kill him, you kill yourself" (Kant 1965, 101). By inflicting on criminals evils proportionate to what they have done to their victims, the state vindicates the universal moral law.

One might object that Kant has misapplied his own categorical imperative. Instead of saying, of a murder for instance, "If you kill him, you kill yourself" and that capital punishment vindicates the moral law, one might ask, purporting greater faithfulness to the categorical imperative, "Could I affirm capital punishment as universal law, knowing that it could be applied to me?" That was the general line of argument pursued by the criminologist Cesare Beccaria in his argument against capital punishment (1963);

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and as we shall see, it is the course taken by contemporary neo-Kantians. For Kant, however, this objection confuses the ideal self, the *homo noumenon* of the categorical imperative, with the ordinary self, the *homo phenomenon* of experience; it confuses the personality of the lawgiver with that of the subject. The latter should have no voice in the enactment of the law, for as subject of the law, one would have interests that would corrupt what Kant calls the "holy" character of legislation, transforming the categorical imperative to a merely hypothetical one. Thus in determining the moral content of the law as well as the punishment of the violations of it, one must rigorously adhere to the perspective of the holy legislator, not the interested subject (Kant 1965, 105).

If this sharp distinction between the self of concrete experience and the ideal self distances Kant from contemporary liberals and seemingly bolsters the claim that at least this liberal can punish, it also points to a more fundamental deficiency in Kant's liberalism. The ideal self is so abstracted from human experience, having no interest, desire, longing, or inclination whatsoever to corrupt the purity of its will—having no "nature"—that it seems no longer human. With no knowledge of what is good for humankind, or what constitutes human happiness, the will of the ideal self may be "free," but it also has no direction; thus any action, as long as it allows for self-consistency, meets the criterion of the categorical imperative (Sidgwick 1890, 210).

More radically stated, a purely free will is both senseless and worthless. Your will is free only if it flows from what is truly you; it is not free if it results from something alien to you. Thus prior to freedom, as Bergmann has observed (1977, 37), is the question of identity; without identity, freedom has no sense. Without identity, freedom is also worthless, for we value freedom only because, by virtue of our

identity, we have purpose and make distinctions of rank among our purposes (Taylor 1979, 183).

By expunging from it every interest and inclination, Kant sought to establish and maintain the purity of the ideal will (Kant 1959, 5, 10, 24, 29, 31). But he also drained it of its human identity and thus of its ability to identify a crime; that is, to know an action as a crime, we must be able to identify at least something of what is good for humans. Consider Kant's statement quoted above: "Any undeserved evil that you inflict on someone else among the people is one that you do to yourself. If you vilify him, you vilify yourself; if you steal from him, you steal from yourself; if you kill him, you kill yourself." To gather Kant's meaning, we must import the knowledge that he excludes from the categorical imperative. For only if we know that reputation, property, and life are human goods can we say that vilification, stealing, and murder *are* evil. Without such knowledge, these actions might be thought blessings that should be honored rather than punished; without such knowledge, we cannot even distinguish honor and punishment. Far from accounting for punishment, Kant cannot even account for crime.

For sound reason, then, unadulterated Kantianism is less influential today than neo-Kantianism. The latter sacrifices the purity of the categorical imperative in order to endow its subject with human identity and human purposes; at the same time it seeks to preserve Kant's universalism by defining moral law as that which a person would enact, knowing that he or she might occupy any place in society, have any interest, and hold any traits compatible with human identity. On the question of liberty, then, neo-Kantians agree with Kant, holding that each adult is entitled to as much liberty as is compatible with equal liberty for all. Of those thinkers that we can describe roughly as neo-Kantians, H. L. A. Hart has given the

most sustained effort to come to terms with the phenomenon of punishment.

In his 1962 lectures, *Law, Liberty, and Morality*, Hart noted the tension between punishment as "emphatic denunciation" and liberal principles limiting the criminal sanction to the prevention of harm to others. Emphatic denunciation as a justification for punishment "represents as a value to be pursued at the cost of human suffering the bare expression of moral condemnation, and treats the infliction of suffering as a uniquely appropriate or 'emphatic' mode of expression" (p. 65). For the retributionist, the suffering of the criminal is good; for Hart it is an evil, justifiable only if it produces less harm and suffering for others. Yet in the series of articles published as *Punishment and Responsibility*, he also noted that a simple focus on preventing harm would support indefinite sentences, widespread preventive detention, and the expansion of strict liability, eliminating excuses and justifications (1968, 28-53, 159-85). In other words, the simple goals of preventing harm, if unrestricted, would threaten liberty and sacrifice fairness.

Hart's solution was to distinguish between (1) the "general justifying aim" of the criminal law (preventing harm) and (2) the "distribution" of the criminal sanction, that is, determining who is liable to the criminal sanction and to what degree. The general justifying aim would render us all vulnerable to the criminal sanction; we may be the hapless victim of circumstance whose exemplary suffering at the hands of the state will secure others from harm. But impinging on this general aim is what Nozick would call a "side-constraint" in the distribution of the sanction. Only when a person has chosen inexcusably and without justification to commit the offense in question (and is in this sense responsible for it), is there "title" to impose the sanction (1968, 4). Our innocence thus protects us from what otherwise would be our vulnerability to

the criminal sanction, allowing Hart to maintain that liberty and fairness are the real substance of this side-constraint; for it functions to maximize "within the framework of coercive criminal law the efficacy of the individual's informed and considered choice in determining the future and also his power to predict the future" (1968, 46; see also 44-48, 180-85).

The point is neatly made and seems at first to accord with our moral sense. Yet there are two problems with Hart's account. First, if "efficacy of the individual's informed and considered choice" is our goal, not just "within the framework of coercive criminal law" but in life generally, it is not clear that we should limit the criminal sanction to those who have chosen to commit a crime. After all, reflective choices in life can be, and probably are today, more often thwarted by unpredictable criminal actions than by unpredictable criminal sanctions. Depending on the social circumstances, we might maximize our general ability to effect informed and considered choices by lessening our individual control over the "distribution" of the criminal sanction—say by eliminating excuses or allowing an occasional innocent to be sacrificed for the appearance of swift and certain sanction—in order to gain a greater security against crime. A 5% increase in the risk of imprisonment may be offset by a 50% decrease in the risk of being robbed, raped, and murdered.

Second, even if Hart's "distribution" principle would work as he intends and not as I have described, he really would not be "punishing" the criminal. In terms of its forward thrust or positive justification, the sanction is applied to you as a criminal not because you have committed a crime, but because your suffering would be useful. The positive justification remains the same whether you commit the crime or not. The difference is that the protective side constraint of innocence is withdrawn. If a jury were to speak

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honestly of its Hartian judgment, it would not pronounce its conviction of the criminal as *guilty* but as *not innocent*.¹³

Probably no liberal thinker has given more thorough development or more rigor of analysis than John Rawls to the basic neo-Kantian project of endowing the subject of justice with a human identity, of correcting the impersonality of Kant's categorical imperative without at the same time sacrificing its impartiality. His account of punishment thus merits our attention. The main idea of Rawls's theory of justice as fairness is sufficiently familiar that I need only note a few central points. The basic principles of justice as fairness are said to be those that free, equal, and rational actors would adopt in the "original position." In that position, we abstract ourselves from everything alleged to be morally arbitrary—the distribution of genetic endowments, positions in society, conceptions of the good, psychological propensities—and choose behind a "veil of ignorance" the principles of justice for the society we are to inhabit, knowing only that we will have some conception of the good and some combination of other morally arbitrary features without knowing which will be ours. The subject of justice is thus given a human personality and the categorical imperative is rendered hypothetical, but impartiality and universality are preserved. In such a position, according to Rawls, the rational actor would affirm two fundamental principles: first, "Each person is to have an equal right to the most extensive basic liberty compatible with a similar liberty for others" and second, "Social and economic inequalities are to be arranged so that they are both a) to the greatest benefit of the least advantaged [the so-called 'difference principle'] and b) attached to offices and positions open to all under conditions of fair equality of opportunity" (Rawls 1971, 60, 83).¹⁴

For the most part, Rawls assumes "strict compliance" with the principles of

justice; that is, his is an "ideal theory" in which there is no injustice, whereas punishment for him belongs to "partial compliance theory," or how to deal with injustice (1971, 8, 315). Though his two discussions of punishment then are brief and incidental to the elaboration of his two principles of justice, each is nonetheless revealing. In the first account, his larger topic is "the rule of law," and his concern is to show how central precepts of this concept follow from the first principle of justice, equal liberty. Even with rules deemed fair from the perspective of the original position, if there are no authoritative sanctions, instabilities to social cooperation will result. If you cannot assume that others will abide by the terms of the social contract or other implicit and explicit agreements, your own fidelity might set you at a disadvantage. Thus to give effect to the principles you would choose in the original position as well as later implicit and explicit agreements, you would affirm the use of sanctions against those who fail to abide by their agreements. And to prevent those sanctions from unduly restricting your liberty, you would as a general principle limit their application to instances when you have chosen, without excuse or justification, to break a rule. Thus as in Hart's argument, which Rawls closely and explicitly tracks, punishment is understood as an extension of liberty: "I have maintained that the principles justifying these sanctions can be derived from the principle of liberty. The ideal conception shows in this case anyway how the nonideal scheme is to be set up; and this confirms the conjecture that it is ideal theory which is fundamental. We also see that the principle of responsibility is *not founded on the idea that punishment is primarily retributive or denunciatory*. Instead it is acknowledged for the sake of liberty itself" (1971, 241, my emphasis; see also 576).

Hart's explanation obviously fits with

Rawls's theory of justice as fairness. Indeed Rawls may be said to supply the larger justifying framework that Hart merely implies. But Hart also drew a conclusion that Rawls merely implies: "punishments" in the neo-Kantian account become indistinguishable from "tariffs": "The pains of punishment will for each individual represent the *price* of some satisfaction obtained from breach of law" (1968, 47, my emphasis). Such a statement, Hart acknowledges, "can sound like a very cold, if not immoral attitude toward the criminal law, general obedience to which we regard as an essential part of a decent social order"; but we feel the sting of this cold immorality only because we have in mind criminal laws of "whose operation we approve." If instead we think of the criminal laws of "South Africa, Nazi Germany, Soviet Russia," he tells us, the account looks much better (1968, 47). That certainly is true. But for an account of punishment purporting to derive from a theory of justice and an extension of liberty, it is a poor recommendation that it becomes palatable only if set in the context of tyranny.

In Rawls's second account of punishment, he defends his "difference principle" (that inequalities should be distributed in a way that supplies the most benefit to the least advantaged) against the contrary notion that "income and wealth, and the good things in life generally, should be distributed according to moral desert" (1971, 310). This latter principle is strikingly similar to that which we previously noted as attached to true punishment—the virtuous should flourish and the wicked suffer. And as Rawls admits, it is a supposition of common sense. But "justice as fairness," he tells us, "rejects this conception," for it would not be chosen in the original position. There one would affirm differences in the distribution of shares not in order to recognize desert but to encourage people to do things socially useful. Since we abstracted from everything

that could serve as a basis for desert when we placed ourselves in the original position, it is hardly surprising that justice as fairness would have nothing to do with moral desert.¹⁵ Even a person's conscientious effort cannot serve as a basis of desert, for it too is largely determined by "natural abilities and skills and the alternatives open to him," that is, by things that are morally arbitrary (1971, 312).¹⁶

However well these points allow Rawls to distance his theory from the commonsense view, they do not really refute that view; and this fact poses a special challenge to his theory of justice, for the "original position" in itself is not his final test of morality. Instead, the original position obtains moral force only as it brings our conceptions of the principles of justice into "reflective equilibrium" with our "considered convictions" of what is just in a wide range of applications (1971, ix, 20). To achieve that equilibrium we are permitted to adjust our considered convictions as well as our understanding of the principles of justice (and thus the characterization of the original position). All is tentative as we move back and forth adjusting principles and convictions until equilibrium results. Since the commonsense view of distribution is obviously a widespread considered conviction, it would seem that Rawls must either (1) show that upon mature reflection we will find the commonsense view unacceptable or (2) adjust the principles of justice and the conditions of the original position to accord with common sense. In fact, Rawls does neither of these. Instead he maintains that the commonsense view derives from an entirely distinct sector of the moral universe, that of retributive justice. The account of punishment that ensues is much as I have described it: desert-based, retributive, and responsive to the marks of "bad character" displayed in the criminal act.

Dividing the moral universe might allow a semblance of equilibrium within

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this part of Rawls's analysis, but it also reveals a teetering disequilibrium between his two accounts of punishment. In the first account, as we noted, Rawls agrees with Hart that punishment is *not primarily retributive* but is instead a scheme of "penalties that stabilize a scheme of cooperation." Here, in this second account, he maintains that punishment is *essentially retributive* and "not simply a scheme of taxes and burdens designed to put a *price* on certain forms of conduct and in this way to guide men's conduct for mutual advantage" (Rawls 1971, 314-15, my emphasis). In the first account he says that "the ideal conception [of strict compliance] shows . . . how the nonideal scheme [of partial compliance] is to be set up; and this confirms the conjecture that it is ideal theory that is fundamental." Here he maintains that "criminal justice belongs for the most part to partial compliance theory, whereas the account of distributive shares belongs to strict compliance theory and so to the consideration of the ideal scheme. To think of distributive and retributive justice as converses of one another is completely misleading" (1971, 315). In the first account, the original position maintains its claim to give a comprehensive account of justice, but allows only for penalties. In the second account, the original position accounts only for a portion of justice and cannot itself account for punishment.

The retreat of claims for the original position in the second account may have been strategic; but it is hardly convincing, for the moral universe cannot be so neatly divided. If you deserve punishment by virtue of the bad character you display in a criminal action, as Rawls in his second account suggests (1971, 315), it is hard to see why you do not deserve rewards for the good character you display in an act beneficial to society (cf. Sandel 1982, 89-91). And if these points are true, the just moral world should in some way resemble that where the virtuous flourish

and the vicious suffer—and not simply that where all inequalities work to the benefit of the least advantaged.

The Pathology of Liberalism

Though liberals can deter, they cannot punish. But if they acknowledge deterrence as their primary goal, they cannot effectively deter either. A simple cost-benefit analysis—the risk of being caught and punished against the gains of crime—should convince most that crime pays and pays well. We abstain from crime—at least most of us still do—not simply because it might be disadvantageous but because it is wrong. The criminal sanction must express that conviction. It cannot simply say that crime is against your interest; through retribution and emphatic denunciation, it must also express, reinforce, and sustain the sentiment that crime is wrong (Durkheim 1984, 31-67; Goodhart 1953, 92-93; Wilson and Herrnstein 1985, 493-98). As political institutions in liberal societies are more self-consciously explained and justified through liberal theory, the question thus becomes more pressing whether liberalism can defend itself. This practical question invites the consideration of whether liberalism suffers a deep theoretical defect.

As a doubting theory, liberalism expresses itself in negative terms; its banner ideals of liberty and equality are pressed against things. Liberty is pressed against arbitrary restrictions; equality is pressed against false claims of superiority—noble birth, race, gender, social status. In the name of these ideals, enormous energy and capacity has spilled forth to create the wonders of Western liberal democracies. But when it is finished rejecting false claims, what is left that liberalism affirms? Depending on their starting points, liberals seem inclined to admit that one or the other of their ideals is empty.

Thus Dworkin announces, upon reflection, "It seems to me absurd to suppose that men and women have any general right to liberty at all, at least as liberty has traditionally been conceived by its champions" (1978, 267). An astonishing statement, but as Dworkin develops the point, it seems correct. For all laws do in some way diminish liberty—restricting a citizen "from driving his car uptown on Lexington Avenue," for example—and we see no need to justify such laws by any greater claim than that the general interest is thereby served. Where we do see liberty trumping the general interest in the strong sense of a "right" to liberty—for instance, permitting a citizen to speak "his mind on political issues"—there is something *behind* the claim to liberty and it is that something rather than liberty itself to which we have a right.¹⁷ For Dworkin, drawing deeply upon Rawls's *Theory of Justice*, it is equality, or more precisely equal concern and respect, that must fill the empty concept of liberty (1978, 272).

But through a meticulous analysis, Peter Westen (1982a) reached the conclusion that *equality* is the empty term. All claims of equality take the form that some persons or things are equal in virtue of some trait; and because of that trait they should be treated the same. The moral force of such claims then rests entirely with the relation of the trait to the treatment; once we make that claim, there is no more work for equality to do. All claims of equality are really invitations to look for a trait in virtue of which the persons or things are equal; it is the traits that really count, not the claim of equality. Again, a superficially astonishing, but deeply persuasive statement.¹⁸ In fact Rawls—whose theory led Dworkin to conclude that liberty rested on equality (Dworkin 1978, 150–83)—would seem not to disagree, at least not in his more recent work, for there he derives equality from certain attributes of liberty. People are *free* "in virtue of what we may call

their moral powers," that is, "a capacity for a sense of justice and a capacity for a conception of the good." People are *equal* "in virtue of having these powers to the requisite degree to be fully cooperating members of society" (Rawls 1985, 233). In other words, contrary to Dworkin's understanding of Rawls, Rawls himself (at least as of 1985) holds that liberty does the work; equality simply affirms that one has those traits to the requisite extent. Dworkin, however, must be forgiven for thinking that equality is more fundamental; for in *Theory of Justice* Rawls had placed precisely these same traits under the heading of equality, not liberty (1971, 19, 505–12). The point, however, seems clear: liberty and equality are themselves empty. When liberalism is pressed for its substance, it has to look elsewhere, to a conception of human nature or the essential self,¹⁹ which for the neo-Kantians seems largely determined by the idea that the citizen of the just state has a "capacity for a conception of the good."

Even here, what promises substance turns hollow upon scrutiny. For there is no intimation of any truth beyond mere conceptions of the good, no essence to which they point; conceptions are not to be ranked in value (Rawls 1971, 19). Human happiness remains to be invented *ex nihilo* by each for him- or herself (cf. Nietzsche 1954, 128–31). Ackerman states the case most dramatically: "But can we *know* anything about the good? . . . The hard truth is this: There is no moral meaning in the bowels of the universe" (1980, 368). This point is thought to support liberty, because if we know nothing about the good or if the good does not exist, then all restrictions on our liberty are arbitrary. And it is thought to support equality, because it claims to superiority by virtue of good character or superior knowledge of the good are groundless. But in fact the point is poised on the brink of nihilism; for if there is no good or knowledge of it, then there is no reason

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for me *not* to impose my will on yours. And the only meaning of equality is that we are equally worthless (Galston 1982; Sandel 1982).

With atrocity as well as nobility of character and deed both emerging from things "arbitrary from a moral point of view" (Rawls 1971, 15), the hero will get a pension but not a reward, and the villain will get a penalty but not a punishment; both will get what they are "entitled" to and neither will get what he deserves, for "the notion of desert seems not to apply to these cases" (Rawls 1971, 104). Whether you are to become a hero or villain is shrouded by the veil of ignorance. The thought recalls a comment by Nietzsche: "There is a point in the history of society when it becomes so pathologically soft and tender that among other things it sides even with those who harm it, criminals, and does this quite seriously and honestly. Punishing somehow seems unfair to it, and it is certain that imagining 'punishment' and 'being supposed to punish' hurts it, arouses fear in it. Is it not enough to render him *undangerous*? Why still punish? Punishing itself is terrible" (Nietzsche 1966, sec. 201).

In light of this latent nihilism, one might be tempted to conclude that liberalism has done well only because it has been able to feed its empty ideals on the corpse of ancient virtue. But this conclusion, while apt regarding liberal theory, is unfair to liberal practice; for in practice virtue has never been allowed to die. Liberal politics remain capable of punishing their criminals, of expressing righteous anger and reprobative judgment; and conversely they honor their heroes with office, fame, and glory. Yet as they have grown more self-conscious in the defense of their institutions and looked simply to liberal theory for their sustenance, liberal politics have grown in self-doubt, as certainly they must if they heed the advice of leading liberal theorists.

At its inception, liberalism was properly horrified by religious warfare, torturous treatment of criminals, the cramping of the human spirit, and more—all done in the name of some notion of human excellence. It was prudent then to lower the horizons of political concern. But when lowering becomes leveling, new problems arise, not just in the tedium of the new moral landscape, the incompetence of the political vocabulary to meet the range of our moral experience, and the emptiness of its theoretical account but also in the practical inability of liberalism to defend itself against those who would violate its laws.

Notes

1. For the most part, classical and contemporary liberals recognize unequivocally this link between skepticism regarding the human good and confidence regarding their affirmative principles. Dworkin denies the link, but that denial seems clearly to result from his confusion of *good* and *right*. For instance, in his essay, "Liberalism," Dworkin maintains that liberalism cannot rest on "skepticism about theories of the good," for "its constitutive morality provides that people must be treated as equals by their government, not because there is no right, but because that is what is right" (1985, 203). In other words he fails to understand that the charge of skepticism claims not that liberalism has no idea of what is right but that this idea of right is itself derived from skepticism about the human good (Brubaker 1985). He repeats the error in his most recent work (1986, 441).

I cannot consider here all varieties of liberalism. I do not, for instance, consider recent endeavors of thinkers such as William Galston (1980) or Rogers Smith (1985) to place a theory of the human good within liberal theory; nor do I consider older efforts, such as John Stuart Mill's, to distinguish between higher and lower pleasures (1959, 76; 1969, 10:255). To date, such efforts have had little impact on the mainstream of contemporary liberalism, largely I believe because they run contrary to its central animating spirit. I am concerned with Locke, neo-Lockeans, Kant, and especially the neo-Kantians. These compose the mainstream of Anglo-American liberalism, extend a broad front, across the "left-right" continuum, and on key points, as will be suggested below, present a striking agreement.

2. This, as opposed to external preferences, or preferences for goods or opportunities for another. This distinction is drawn from Dworkin (1978, 275).

Like all attempts to state the line between individual rights and public authority, this distinction proves problematic upon analysis. See, for instance, Dworkin's discussion of whether people who adore "Sarah" may have their preference counted that Sarah's preferences should count for twice as much as those of anyone else (1985, 361-72). I use it merely to suggest the general direction in which the neo-Kantians wish to establish the authority of, and limits to, public action.

3. Excepting cruder forms of utilitarianism, which I shall leave aside in this discussion.

4. "The mind has a different relish, as well as the Palate; and you will as fruitlessly endeavor to delight all men with Riches or Glory . . . as you would satisfy all Men's Hunger with Cheese or Lobsters. . . . Hence it was, I think, that the Philosophers of old did in vain enquire, whether Summum Bonum consisted in Riches or bodily Delights, or Virtue, or Contemplation: And they might have reasonably disputed, whether the best Relish were to be found in Apples, Plumbs, or Nuts; and have divided themselves into Sects upon it" (Locke 1979, 2.21.55, p. 269).

5. This is not to claim that there is a perfect symmetry; for, as Aristotle notes, "to inflict a just penalty or punishment is indeed an act of goodness; but it is also an act which is forced on the agent, and it has value only as being a necessity. Acts done with a view to bestowing honours and wealth on others are in a different category: they are acts of the highest value" (1958, 1332a).

6. For some, the tie to this ethical view undermines the case for punishment as I have described it; for it is thought absurd, and correctly so, that we could in determining our sentences decide on "the right amount of suffering to inflict in a given case [by assessing] the character of the offender as manifested throughout his whole life and also his total lifelong balance of pleasure and pain" (Feinberg 1970, 116-17). But when we reward and punish, these "whole life" concerns enter our judgments only at the margins. We hesitate to honor a battle hero if, but for this single shining moment, he or she was an insolent and selfish coward; we are reluctant to slap a lengthy sentence on the bribed public official if, but for this solitary lapse, he or she was a dutiful, honest, and faithful public servant. But the essential focus is on the deed, the character that is displayed in performance of that deed, villainous or heroic, and the treatment deserved in virtue of that deed.

7. In cases of outrageous crime, to prevent the natural anger that liberalism refuses to countenance from swelling into a rage that would threaten the stability of the polity and hence the rights of individuals, liberals might find it expedient to punish. But that is not to say that they could then consider themselves justified in any positive sense, only that they were trying to prevent the greater evil of threats to individual rights.

8. Cf. Durkheim 1984, 63: "[Punishment's] real function is to maintain inviolate the cohesion of society by sustaining the common consciousness in all its vigor."

9. One way of extending the authority of liberalism to enable it to punish would be through the concept of "profound offense"; but realizing its threat to the foundations of liberalism, Feinberg explicitly rejects it as a basis for criminal sanction (1985, 50-96).

10. Adhering to its commitments to individualism and against external preferences, liberalism might in a paradoxical manner still attempt to account for community and desert and from such an account to justify punishment; that is, liberals may be said to constitute a community in their common commitment to individualism or to have a notion of desert in their valuing of the inner disposition not to judge individuals on the basis of their inner dispositions. In a similar sense, one may say that a relativist is really an absolutist because he or she believes relativism to be the truth. But the concepts thus formed of community, desert, or absolutism are understood only in reference to the principles that contradict them; they cannot form the foundation of anything that assumes genuine community, desert, or absolutism. To their credit, the liberal thinkers considered below largely avoid this paradoxical morass.

11. Nozick's focus on "fear" as the exclusive emotion of concern is itself revealing. Directing its force inward upon the individual self, fear forms the emotional base for the enactment of penalties and prohibitive taxes, but it cannot explain punishment. Anger, directing its force outward to the source of harm, forms the emotional base for punishments.

12. In a later work, Nozick does define punishment in terms that are largely compatible with the description I have given, but he makes no significant attempt to link this description with his justifying principles (1981, 363-97).

13. As will be pointed out below, such an account of the criminal sanction renders "punishment" a mere "tariff." It should be noted that in the *Concept of Law*, Hart persuasively argues, from the "internal perspective," for the distinction between punishments and taxes (1961, 39). But as Neil MacCormick observes in his intellectual biography of Hart, when it comes to punishment, Hart is not consistent in his "hermeneutic method" (1981, 143). The transformation of *guilty* to *not innocent* is also accomplished by Norval Morris (1974, 75) and Herbert Packer (1968, 67-69). Ely encounters similar difficulties (1980, 135-79; Brubaker 1982, 228-31).

14. I quote Rawls's first statement of these principles since it is clearer than his more technical and precise second formulation (1971, 302).

15. On the need for a *basis* for desert, see Feinberg 1970, 55-94.

16. Note also Rawls's comments on the man of "superior character": "his character depends in large

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part upon fortunate family and social circumstances for which he can claim no credit" (1971, 104).

17. "If we have a right to basic liberties . . . because an assault on basic liberties injures us or demeans us in some way that goes beyond its impact on [a general right to] liberty, then what we have a right to is not liberty at all, but to the values or interests or standing that this particular constraint defeats" (Dworkin 1978, 271).

18. Though obviously it is not persuasive to everyone. See, for instance, the articles by Burton (1982), Chemerinsky (1983), and Greenawalt (1983) and the replies to each by Westen (1982b, 1983a, and 1983b).

19. In his more recent work, Rawls professes that his theory of justice is "political not metaphysical." But I do not see how he can avoid making at least implicit "metaphysical" claims either about human nature or history. Either he rests his argument on certain ideas of the nature of man or on a moment in the course of human history. It seems to me that *Theory of Justice* (1971) points towards the former and "Justice As Fairness: Political Not Metaphysical" (1985) points towards the latter.

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EXPLAINING GOVERNMENT GROWTH IN THE U.S. STATES

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Despite the explosive increase in the research program on government growth in recent years, little work has been done on government growth disaggregated to the subnational level. I examine the empirical validity of five competing models of government growth for the fifty U.S. states from 1945 to 1984: Wagner's Law, fiscal illusion, party control, bureau voting, and intergovernmental grant. Government size is defined in terms of state government spending as a proportion of total state economic output, with separate implicit price deflators being employed for the public and private sectors. Based on a longitudinal test of these competing models, the analysis uncovers strong empirical support for the bureau voting and intergovernmental grant models, moderately weak support for the Wagner's Law model, and virtually no support for the fiscal illusion and party control explanations. These findings have important implications for the study of government growth in general and, more specifically, in the states.

One of the outstanding features of U.S. political life in the postwar era has been the growth in the size of the public sector. While there has been general agreement about the existence of government growth, there is substantially less agreement surrounding extant explanations of the observed patterns of government growth since 1945. In recent years a myriad of alternative explanations of government growth have been proposed (Borcherding 1977; Larky, Stolp, and Winer 1981; Lowery and Berry 1983; Tarschys 1975). Furthermore, numerous scholars have developed an extensive set of models of government growth and tested them competitively and empirically, both in the United States (Berry and Lowery 1984a; Kau and Rubin 1981; Lewis-Beck and Rice 1985; Lowery and Berry 1983) and in several nations (Cameron 1978; Mann 1980; Peacock and Wiseman 1967).

Despite the great volume of research written on government growth at the national level, little is known about patterns of government growth disaggregated to the subnational level. The small body of research that does exist suffers from numerous shortcomings: (1) general neglect of basic, rudimentary patterns of government growth over time; (2) a reliance on cross-sectional designs inappropriate for the study of dynamic government growth processes; and (3) a failure to develop alternative models of government growth and test them competitively. Before a more comprehensive understanding of the broad phenomenon of government growth can be achieved, we need to extend the research program to include subnational political systems as well.

I test competing explanations of government growth in the fifty U.S. states from 1945 to 1984. There are three major reasons why such a research endeavor is

important. First, the states provide an excellent comparative setting within which the study of government growth can be conducted. The existence of fifty political systems with differing political, social, and economic contexts provides an excellent laboratory for the development and competitive testing of various models of government growth under different settings. Second, the study of government growth in the states is an important undertaking with value for students of state policy and politics. To date, the states have been virtually ignored as a focus of research on government growth. Currently very little is known about even basic patterns of government growth in the states, to say nothing about the empirical validity of state-level government growth models. Third, government growth in the states may have important implications for the overall growth of the public sector in the United States during the 1970s and 1980s. Such implications are particularly likely, given the New Federalism agendas of the Nixon and Reagan administrations and the associated transfer of some federal government responsibilities to the state and local governments. In other words, overall growth in the public sector may be driven more by state government growth patterns in recent years than in the past.

Competing Models of Government Growth

Several general models of government growth found in the literature can be applied to the growth of the public sector in the states. Before I describe these models, three caveats should be noted. First, the set of government growth models presented below does not constitute an all-inclusive list. Several models cited in the literature cannot be readily applied to the states, either for theoretical reasons or because the lack of available

data prevents an empirical test of the explanation. Second, the model specifications presented below are very simple. I would argue, however, that such simple specifications are in keeping with the theoretical orientations presented throughout the literature and that a competitive test of these simple models allows the researcher to develop a baseline model or set of models upon which more comprehensive multivariate models can be built. Third, theoretical processes suggested by these competing models need not be mutually exclusive. It is possible that some of the independent variables included in these separate models are interrelated theoretically and empirically and that the specification of the relationship between one independent variable and government growth carries with it an implied specification of some other relationship.

Wagner's Law

In an early classic work on government growth, Wagner (1877, pt. 1) posited that government growth was a function of factors associated with increased industrialization, economic affluence, and population growth. These factors were seen as being associated with government growth for two reasons: (1) demand for government goods and services was viewed as income-elastic (i.e., demand increased at a faster rate than did economic affluence); and (2) increased industrialization and population were assumed to be associated with increased levels of societal interdependencies and externalities requiring increased governmental regulation and intervention. A complete specification of Wagner's Law similar to that tested by Lowery and Berry (1983) would require the inclusion of variables for industrialization, income, population, and population density in a model of government size. Based on the limited availability of such data across the fifty states, the fol-

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lowing specification of Wagner's Law can be tested:

$$\text{GOVSIZE}_t = a + b_1(\text{INCOME}_{t-1}) + b_2(\text{POPUL}_{t-1}),$$

where GOVSIZE is the ratio of state government expenditures to total output of each state's economy in the year t ; INCOME is the value of total personal income in each state; and POPUL is the total state population. If Wagner's Law is supported empirically, one would expect both coefficients to be in the positive direction.

The Fiscal Illusion Explanation

A second explanation suggests that government growth can be attributed to a reliance by state governments on forms of revenue collection that hide the costs of providing public goods. Such a revenue system, with its emphasis on hidden taxes, causes citizens to underestimate the cost of governmental goods and services and hence demand more than they would if they could accurately estimate these costs (Goetz 1977).

Several aspects of a revenue system are seen as having illusory effects. First, withholding provisions, which involve the government's deducting taxes from citizens' paychecks, are seen as illusory (Enrick 1964). Second, some have argued that indirect taxes (such as corporate income taxes) have an illusionary (and hence expansionary) impact because (1) they are merely passed on to consumers as increased prices and (2) citizens are unable to estimate the "tax element" of the price they pay for private goods (Goetz 1977). Third, increased complexity of the tax system (as characterized by reliance on many different taxes) is seen as illusory, primarily because taxpayers are less likely to be able to comprehend its complexity and estimate the "true" costs of government services effectively.

It is difficult to specify in one simple but

comprehensive model all of the illusory aspects of a state's tax system. However, in this analysis I rely on a derivative of the fiscal illusion model presented by Lowery and Berry to provide a partial test of the fiscal illusion explanation of government growth:

$$\text{GOVSIZE}_t = a + b_1(\text{INDTAX}_{t-1}) + b_2(\text{CORPTAX}_{t-1}) + b_3(\text{COMPLEX}_{t-1}),$$

where INDTAX is the proportion of state government revenues collected through the personal income tax, which relies on the withholding mechanism; CORPTAX is the proportion of state government revenues collected through corporate income taxes, representing the indirect tax illusion; and COMPLEX denotes the complexity of the state tax system. It is expected that all coefficients will be in the positive direction.

The Party Control Explanation

The party control explanation posits that government growth is related systematically to control of governmental policy-making institutions by the liberal (usually the Democratic) party within the state political system. Shifts in control of the governor's office and state legislature in the Democratic (i.e., liberal) direction are expected to result in increases in state expenditures as a proportion of the total state economy; while shifts in the Republican (i.e., conservative) direction are expected to result in either decreases or smaller increases in relative government size.

The thesis that party control of state government has important policy implications is not universally accepted. Such a thesis makes the crucial assumption that the party system within a state is policy-differentiated—that is, that the competing

political parties-in-government take on different policy positions (Garand 1985). In other words, it is assumed that party labels have policy relevance and that the political parties within a state political system stake out significantly different positions on issues relevant to state expenditure decisions. The degree to which this assumption is validated empirically in the states is subject to considerable debate (Dawson and Robinson 1963; Downs 1957; Garand 1985; Winters 1976). However, it would seem that an empirical test of the party control explanation is in order; this explanation can be specified as follows:

$$\begin{aligned} \text{GOVSIZE}_t = & a + b_1(\text{GOVSIZE}_{t-1}) \\ & + b_2(\text{GOVERNOR}_{t-1}) + b_3(\text{LEGIS}_{t-1}) \\ & + b_4(\text{GOVERNOR}_{t-1} \cdot \text{LEGIS}_{t-1}), \end{aligned}$$

where GOVERNOR is a binary variable indicating Republican control of the governor's office and LEGIS is the degree of Republican control of the state legislature. If the party control model is supported empirically, one would expect the coefficient for the lagged value of GOVSIZE to be positive and all three coefficients for the party control variables to be in the negative direction.

The Bureau Voting Explanation

This explanation ties government growth to the advantages in voting power that government employees have over other citizens. This thesis assumes that government employees are self-interested actors who desire increased government spending for their own well-being (Niskanen 1971). Further, it is assumed that government employees are more likely to turn out to vote (Bush and Denzau 1977) and that subsequently the spending preferences of government employees—reflected in the candidates advantaged by government employee vote choices—are more likely to have an impact on public

policy than the spending preferences of other citizens. Although the direct empirical evidence is not very supportive of this explanation (Courant, Gramlich, and Rubinfeld 1980; Kau and Rubin 1981; Lowery and Berry 1983), the model has not been tested in a full range of political systems. Hence, the model can be specified as follows:

$$\text{GOVSIZE}_t = a + b_1(\text{GOVEMP}_{t-1}),$$

where GOVEMP is the number of government employees as a proportion of state population.¹ If the bureau voting explanation is to be validated empirically, it is expected that b_1 will be greater than zero.

Intergovernmental Grant Explanation

This explanation attributes government growth in the states to the influx of intergovernmental grants from the federal government. Whether or not intergovernmental grants have contributed to growth in the size of state governments is subject to some debate. Some scholars argue that intergovernmental grants provide state governments with additional dollars and that the influx of these dollars contributes to growth in the size of the state public sector relative to the size of the total state economy. Others posit that intergovernmental grants perform a replacement function; that is, such grants are utilized to reduce the state's revenue burden while at the same time allowing the state to maintain its current spending levels. In order to estimate the impact of intergovernmental grants on state government growth, the following model can be specified:

$$\text{GOVSIZE}_t = a + b_1(\text{FEDGRANT}_{t-1}),$$

where FEDGRANT is federal grants-in-aid to state governments as a proportion of total state expenditures. If this explanation is correct, it is expected that b_1 will be positive.

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Data and Methods

The data employed in this study were collected as part of a long-term project on dynamic policy processes in the states (Garand 1985, 1987; Lowery, Konda, and Garand 1984). With only a few exceptions, annual data for each of

the variables in this analysis were collected for the 40-year time period from 1945 through 1984. A listing of the variables and a brief description of their empirical indicators used to estimate the parameters of the government growth models are presented in Table 1.

Table 1. Description of Empirical Indicators Used to Test Competing Models of Government Growth

Explanation	Variable	Description of Indicator
	SEXPEND _t	total state government expenditures
	INCOME _t	total state personal income
	SLDEF _t	implicit price deflator for state and local government purchases of goods and services (1972 = 100) (source: Bureau of Economic Analysis, U.S. Department of Commerce)
	GNPDEF _t	implicit price deflator for gross national product (1972 = 100) (source: Bureau of Economic Analysis, U.S. Department of Commerce)
	GOVSIZE _t	ratio of total state expenditures (deflated by the implicit price deflator for state and local government purchases of goods and services) to total state personal income (deflated by the implicit price deflator for gross national product)
		$\text{GOVSIZE}_t = \frac{\text{SEXPEND}_t / \text{SLDEF}_t}{\text{INCOME}_t / \text{GNPDEF}_t}$ $= \frac{\text{SEXPEND}_t}{\text{INCOME}_t} \cdot \frac{\text{GNPDEF}_t}{\text{SLDEF}_t}$
Wagner's Law	INCOME _t	total personal income (deflated by the implicit price deflator for gross national product)
	POPUL _t	total state population
Fiscal Illusion	INDTAX _t	total state personal income taxes divided by total state revenue
	COMPLEX _t	Herfindahl index of revenue concentration (see Wagner 1976)
	CORPTAX _t	total state corporate income tax divided by total state revenue
Party control	GOVSIZE _{t-1}	lagged measure of government size
	GOVERNOR _t	1 = Republican control of the governor's office; 0 = Democratic control of the governor's office
	LEGIS _t	1 = Republican control of both chambers of state legislature; .5 = split control of the state upper and lower houses; and 0 = Democratic control of both chambers of state legislature
Bureau voting	GOVEMP _t	total number of state and local government employees divided by total state population
Intergovernmental grant	FEDGRANT _t	total federal intergovernmental grants-in-aid to state governments divided by total state expenditures

The Dependent Variable

The dependent variable (GOVSIZE) measures the level of state government spending as a proportion of total state economic output. Two major issues are raised by such a strategy for the measurement of government size. The first issue pertains to the indicator employed to measure the size of the total state economy. At the national level, most studies of government growth utilize a measure of the gross domestic product (GDP) or gross national product (GNP) (Berry and Lowery 1984a; Lewis-Beck and Rice 1985; Lowery and Berry 1983; Mann 1980). However, there is no equivalent measure of total economic output available systematically across all fifty states for the 40-year time period of this analysis. I utilize total state personal income as a surrogate measure of state economic output. While such a measure is not a perfect indicator, patterns of state personal income over time should be relatively sensitive to changes in overall state economic output over time, and one can argue that such an indicator reflects generally the underlying concept of the size of each state's economy.

The second major issue concerns the use of differential price deflators for the public and private sectors. Many scholars (Cameron 1978; Lewis-Beck and Rice 1985) assume that the price deflators for government spending and the private economy are identical. In measuring government growth, these authors do not deflate the dollar amounts used in the numerator and denominator, primarily because to do so would involve multiplying the ratio of government spending to total economic output by a value of one (i.e., CPI/CPI). However, other scholars have argued that this strategy for measuring government size fails to take into account the differential price deflators characterizing the public and private sectors (Beck 1976; Berry and Lowery 1984a, 1984b; Garand 1987; Lowery and Berry

1983). According to Lowery and Berry (1983, 680), the size of government (relative to the magnitude of total economic output) may grow for two reasons: "(1) the scope of government activity may broaden (i.e., government may increase the amounts and types of goods and services it provides), and (2) the cost of providing a constant level of goods and services may rise relative to the prices of goods and services in the private sector" (emphasis original). In other words, if the inflation rate is higher in the public sector (i.e., if prices rise at a faster rate in the public sector), the share of total economic output going to the governmental sector will increase even if the scope of government activity remains constant. Government can grow as a result of the higher relative cost associated with goods in the public sector when compared to the cost of providing goods in the private sector. Hence it is necessary to adjust the government size measure to take into account the differential price deflators characterizing the public and private sectors. This can be done by measuring government size following the convention utilized by Lowery and Berry (1983):

$$\text{GOVSIZE}_t = \frac{\text{SEXPEND}_t / \text{SLDEF}_t}{\text{INCOME}_t / \text{GNPDEF}_t}$$

$$\text{GOVSIZE}_t = \frac{\text{SEXPEND}_t}{\text{INCOME}_t} \cdot \frac{\text{GNPDEF}_t}{\text{SLDEF}_t}$$

where SEXPEND is state expenditures; SLDEF is the implicit price deflator for state and local government purchases of goods and services (1972 = 100); INCOME is total state personal income; and GNPDEF is the implicit price deflator for gross national product (1972 = 100).²

The Independent Variables

The description of empirical indicators for the independent variables is presented in Table 1 and is generally straight-

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forward. Several issues and caveats should be noted. First, in several cases data constraints either make impossible the measurement of indicators of all variables relevant for the government growth models or prevent the specification of a full range of models similar to that presented by Lowery and Berry (1983). Second, some of the data are not available for all years in the analysis; data on state government employment necessary for testing the bureau voting explanation are available only from 1952 to 1984. Third, in all cases the independent variables are lagged to capture the lag time between state policy decisions and their outcomes. It would be inappropriate to specify government growth models that suggest that current government size is a function of current conditions, given the fact that decisions underlying government size are made prior to the actual policy outcome.

Model Estimation

Each of the government growth models is estimated separately for each of the fifty states utilizing ordinary least squares (OLS) regression. Unfortunately, time series models like those presented here are often contaminated by the effects of autocorrelated error terms—a form of model misspecification that renders OLS regression inappropriate (Ostrom 1978). In order to correct this problem, a two-stage diagnostic and correction procedure is utilized (Hibbs 1974; Ostrom 1978).³ The end result is a series of parameter values uncontaminated by the effects of autocorrelated error terms.

Empirical Results

Because of space limitations, it is not possible to present the complete parameter values for all of the models examined in this study. Instead, Table 2 presents a summary of these results for the five gov-

ernment growth models tested.⁴ This table indicates the degree to which the coefficients associated with each of the models are (1) in the direction consistent with theoretical expectations and (2) of a magnitude sufficient to suggest an adequate level of substantive significance.⁵

Wagner's Law

The results presented in Table 2 suggest substantial variation across the states in the level of empirical support for Wagner's Law. First—and contrary to expectations—in the results upon which Table 2 are based a majority of the coefficients for INCOME (28 out of 50, or 56%) and several of the coefficients for POPUL (6 out of 50, or 12%) are in the negative direction. For the states with negative coefficients for the INCOME variable, the results suggest that demand for public goods is income-inelastic (i.e., increased income is not associated with greater demand for public goods relative to demand for private goods). The negative coefficients observed for the POPUL variable in some states indicate that increased societal interdependencies associated with higher population do not—as it was expected they would—coincide with higher levels of governmental spending relative to the size of the total state economy. Overall, there is a significant number of coefficients that deviate from the expected direction, though a majority (66 out of 100) are in a direction consistent with expectations. Second, when the stricter (though arbitrary) criterion—namely, that the coefficient be in the proper (positive) direction and exceed the value of its standard error—is utilized, one finds that 34% (17 out of 50) of the INCOME coefficients, 78% (39 out of 50) of the POPUL coefficients, and 56% (out of 100) of all coefficients meet this standard. Third, in many cases states with positive coefficients for one of the independent variables exhibit negative coefficients for the other.

**Table 2. Applicability of Government Growth Models
to Data from Individual States**

State	Bureau Voting	Intergovernmental Grant	Wagner's Law	Fiscal Illusion	Party Control
Alabama	*	*		*	
Alaska	*				
Arizona		*			
Arkansas		*			
California	*	*			
Colorado		*			
Connecticut	*				
Delaware	*	*			
Florida		*			
Georgia	*	*			
Hawaii					
Idaho	*				
Illinois	*		*	*	
Indiana	*	*			
Iowa	*	*	*		
Kansas		*			
Kentucky	*	*			
Louisiana					
Maine	*	*			
Maryland	*	*			*
Massachusetts	*	*	*		
Michigan	*	*			
Minnesota	*	*			
Mississippi		*			
Missouri	*				
Montana	*	*			
Nebraska	*		*		
Nevada	*				
New Hampshire	*				
New Jersey	*				
New Mexico	*	*			
New York	*	*	*		
North Carolina	*	*			
North Dakota		*			
Ohio	*				
Oklahoma					
Oregon	*	*			
Pennsylvania	*	*	*		
Rhode Island	*	*	*		
South Carolina	*				
South Dakota	*	*	*		
Tennessee	*	*			
Texas	*	*			
Utah	*	*			
Vermont		*			*
Virginia	*	*			
Washington	*	*			
West Virginia	*	*			
Wisconsin	*	*	*		
Wyoming	*	*			

Proportion of slope
coefficients in proper
direction and exceeding
one standard error

38-50 (.76)

35-50 (.70)

9-50 (.18)

2-50 (.04)

2-50 (.04)

Note: * means that all coefficients for model are in proper direction and exceed associated standard error.

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Using an even stricter criterion, one finds that only 18% (9 out of 50) states have a set of two coefficients that meet this standard. Overall, then, the empirical support for Wagner's Law as applied to government growth in the states is, at best, quite modest. It is clear that the level of support for this explanation is not of a similar magnitude to that observed in Lowery and Berry's (1983) research at the U.S. national level.

Fiscal Illusion Explanation

In general, Table 2 suggests only weak inconsistent support for the fiscal illusion thesis. First, many of the coefficients are in the incorrect (negative) direction for IND TAX (16 out of 44, or 36%), CORPTAX (18 out of 46, or 39%), and COMPLEX (19 out of 50, or 38%); overall, 53 out of 140 coefficients (38%) are in the negative direction. Second, when one employs a stricter standard, one can observe that less than a majority of the coefficients for IND TAX (21 out of 44, or 48%), CORPTAX (12 out of 46, or 26%), and COMPLEX (20 out of 50, or 40%) are in the expected positive direction and exceed the value of their associated standard errors; overall, only 53 out of 140 coefficients (38%) meet these criteria. Furthermore, only in Alabama and Illinois are these standards met for all three independent variables; in all other states at least one coefficient is either in the negative direction or is relatively unstable (as indicated by the relative size of its standard error), though in 16 out of 50 states (32%) the standard is met for two out of three coefficients. Overall, then, the results presented here do not appear to offer much systematic support for the fiscal illusion thesis as an explanation of government growth in the states.

Party Control Explanation

An examination of summary findings presented in Table 2 indicates little sup-

port for the party control thesis of state government growth, once the impact of previous levels of government size are taken into account. First, a substantial number of coefficients are in the positive direction, indicating a higher level of government growth during periods of Republican party control of state governmental institutions. The numbers and proportions of negative coefficients are as follows: GOVERNOR (18 out of 45, or 40%), LEGIS (19 out of 30, or 63%), and the GOVERNOR · LEGIS (8 out of 23, or 35%), 23, or 61%). When the stricter standard is applied, one finds that very few coefficients are both negative and have magnitudes in excess of the value of their standard errors: GOVERNOR (4 out of 45, or 9%), LEGIS (6 out of 30, or 20%), and GOVERNOR · LEGIS (8 out of 23, or 35%). Finally, none of the states has a complete set of three coefficients that meet both criteria of negative valence and relative stability, although two states (Maryland and Vermont) have their only coefficient meeting these standards. Clearly, very little empirical support is uncovered for the party control explanation of government growth in the states. It would appear that there is no universal tendency for states to exhibit higher levels of government growth during eras of Democratic control of state policy-making institutions.

Bureau Voting Explanation

The results depicted in Table 2 are generally strongly supportive of the bureau voting model of state government growth. First, 45 out of 50 coefficients (90%) are in the expected positive direction. Second, 38 out of 50 coefficients (76%) are positive and exceed the value of their associated standard errors. In other words, in three-fourths of the states the relationship between government employment and government size is positive and stable enough to be considered as providing adequate support for the

bureau voting model. All in all, there appears to be substantial support for the bureau voting explanation of state government growth.

Intergovernmental Grant Explanation

The intergovernmental grant explanation suggests that government growth is a function of the influx of intergovernmental grants from the federal government, which are (the thesis posits) utilized by state governments to increase (in a cost-free manner) the level of public goods provided in that state. The summary of empirical results shown in Table 2 suggests support for this model. First, 46 out of 50 coefficients (92%) are in the expected positive direction, indicating that increases in federal grants as a proportion of state self-generated revenue is associated with higher levels of overall government size. Furthermore, when one includes a criterion of relative coefficient stability, one finds that 35 out of 50 parameter values (70%) are both positive and are larger in magnitude than their associated standard errors. It would appear that for over two-thirds of the states the infusion of intergovernmental grants has contributed a relatively stable positive impact to the growth of state government's share of total state economic output.

Overview

Based on Table 2 and the foregoing discussion, it is clear that there is substantial variation in the degree of support for competing models of government in the states. The bureau voting and intergovernmental grant models appear to provide the closest and most consistent approximations of observed patterns of state government growth over time, while the Wagner's Law explanation seems to generate modest levels of empirical support. The fiscal illusion and party control

explanations find little support whatsoever. It should be noted that for a significant number of states more than one model provides an adequate level of explanatory power to the patterns of government growth over time. In some states as many as four models are supported by the empirical analysis. There are two potential reasons for this. First, it is possible that some of the competing explanations are not mutually exclusive, and that the independent variables represented in the competing models are highly correlated and empirically indistinct. Alternatively, it is possible that several sets of explanations serve as distinct components that act in an additive (or perhaps interactive) manner in influencing government size and must therefore be built into a comprehensive multivariate model for each state. Future research will explore these alternatives in developing a more complete model of government growth in the states.

Conclusion

I have explored the determinants of government growth in the fifty states. The models estimated give a relatively clear view of the relative merits of competing models of state government growth. Some of the models of government growth must either be revised or eliminated from the research program as it applies to the states. The party control and fiscal illusion models, while plausible on theoretical grounds, do not appear to provide systematic explanations of state government growth. On the other hand, the bureau voting and intergovernmental grant models seem to be well supported empirically, while Wagner's Law is supported only modestly. Interestingly, these findings stand in sharp contrast to those of Lowery and Berry (1983), who find that Wagner's Law provides the best explanation of federal government

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growth from 1948 to 1979. In the present analysis, the truncated specification of Wagner's Law finishes a poor third in terms of its empirical validity in the states.

The strong empirical support found for the bureau voting explanation suggests some important implications for the working of Democratic government in the states. Interestingly, this model is cited by Lowery and Berry (1983, 672-75) as being an "excessive government" explanation of government growth, indicating that the process reflected in this model assumes that "the institutions of government are far from neutral in determining the size of the public sector. In particular, this set of explanations assumes that these institutions operate to expand the government's share of the economy beyond the size demanded by the public." Furthermore, one can plausibly suggest that the intergovernmental grant model also falls into the category of excessive government explanation, given that the influx of federal grants may result in increases in the size of government beyond that demanded by the public. In any event, if these models are supported empirically (as they appear to be), they may lend support to the view that state governments are composed of self-interested actors who utilize at least some of the tools at their disposal to increase the relative size of the state public sector.

Notes

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1. The bureau voting thesis suggests that the relative strength of government employees in the electorate should have a positive impact on state government growth. However, data on the size of the electorate for each state-year case are not readily

available, so I employ population in the denominator as a proxy for the size of the electorate. While the measure of bureau voting strength employed in this model is not perfect, it does (I believe) capture the general concept.

2. The implicit price deflator for state and local government purchases of goods and services is not measured separately for each state-year case. Instead, this is a general price deflator appropriate for state and local spending as a whole. The values of this index are the same across all states for any given year. While a separate price deflator for each state would be ideal, I am aware of no such indicator available for the full time period under study.

3. First, the autocorrelation function (ACF) and the partial autocorrelation function (PACF) for each model are examined in order to discern the existence and order of the underlying autoregressive process. The second stage involves the proper estimation of parameter values for the government growth models. If a pattern of autocorrelated error terms is not observed, OLS parameter estimates are used, for they accurately estimate the parameters of the regression model. If, on the other hand, autocorrelated error terms are in evidence, a two-step full-transform method equivalent to generalized least squares (GLS) is employed. For the party control model the inclusion of a lagged endogenous variable ($GOVSIZE_{t-1}$) makes the two-step full-transform method inappropriate. In order to provide parameter values uncontaminated by serial correlation, an instrumental variable-pseudo-GLS estimation procedure is employed. This is the same procedure utilized in the government growth research by Lowery and Berry (1983) and is described in detail by Ostrom (1978). However, in only two of the fifty states (New York and Tennessee) does the party control model exhibit patterns of autocorrelated error terms, so it is only in these two states that the special estimation procedure is employed.

4. The tables reporting the complete set of parameter values upon which Table 2 is based are available upon request from the author.

5. In the tables upon which Table 2 is based, I report unstandardized regression coefficients, their standard errors, and their probability values. However, the use of significance tests to make inferences about population parameters is the subject of a substantial amount of debate in the social sciences. One might argue that the data used in this analysis constitute the population of observations from 1945 to 1984 for each state. Because unstandardized regression coefficients reflect actual population parameters (and not *estimates* of those parameters), one might argue that this makes unnecessary the use of standard significance tests. On the other hand, it is recognized that many social scientists find that significance tests are appropriate, or at least helpful, in evaluating relationships within populations. In deference to this position and primarily for illustra-

tive purposes I present significance test results for parameter values in the tables used to construct Table 2 (available upon request).

If one does not employ tests of statistical significance in evaluating models involving population data, what criteria can be utilized to evaluate the parameter values of these models? What is necessary is a criterion that gives substantive significance to the parameter values associated with the variables for these government growth models. In evaluating these parameter values, I employ an (admittedly) arbitrary evaluative criterion: if a coefficient exceeds the value of its standard error, I assume that the coefficient represents a stable impact of the independent variable on government size and hence that it can be characterized as indicating an adequate level of substantive significance. The standard error of an unstandardized regression coefficient reflects the stability (or consistency) of the impact of a one-unit shift in the independent variable, controlling for the effects of any additional independent variables. This is important because one's confidence in the substantive significance of a coefficient increases as the level of fluctuation in the impact of the independent variable declines. While this standard is less strict than that employed by scholars using conventional significance tests to evaluate the empirical validity of models, it does represent one plausible indicator of the degree to which one can be confident in the strength of the findings.

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SYMPOSIUM

THE RETURN TO THE STATE

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Three important questions are raised by the "return to the state" movement of recent years. First, are the pluralist, structural functionalist, and Marxist literatures of political science societally reductionist, as this movement contends? Second, does the neostatist paradigm remedy these defects and provide a superior analytical model? Third, regardless of the substantive merits of these arguments, are there heuristic benefits flowing from this critique of the literature? Examination of the evidence leads to a rejection of the first two criticisms. The answer to the third question is more complex. There is merit to the argument that administrative and institutional history has been neglected in the political science of the last decades. This is hardly a "paradigmatic shift"; and it has been purchased at the exorbitant price of encouraging a generation of graduate students to reject their professional history and to engage in vague conceptualization.

Arguments for a return to the concept of the state have been made intermittently ever since Frederick Watkins (1968) prematurely solemnized its burial 20 years ago. The writers who have advocated a return to the state have had different goals and agendas. In the cases of Nettl (1968), Stepan (1978), and Nordlinger (1981) the argument seems to be one of emphasis. The campaign carried on under the auspices of the Committee on States and Social Structures of the Social Science Research Council to "bring the state back in" speaks of "paradigmatic shifts" (Evans, Rueschemeyer, and Skocpol 1985). The "pluralist-functionalist" and Marxist paradigms, which are described as the waning paradigms of contemporary political science, are said to be societally reductionist, according no autonomy to state structures and politics and hence fundamentally lacking in explanatory power. The statist paradigm remedies this defect and will replace these earlier approaches.

I respond to three questions raised by this polemic. First, are the pluralist, structural functional, Marxist, and other literatures of political science societally reductionist? Second, does the "statist paradigm" remedy these defects? Third, regardless of the substantive merits of these arguments, are there heuristic benefits flowing from this critique of the literature?

Arguments for a Return to the State

I propose to review these criticisms, and make a search of the literature to determine the extent to which this view of "mainstream" political science is sustained by evidence. I cover ground similar to that covered by G. David Garson (1974) in an article published more than 10 years ago and in a subsequent book (1978). He provided a review of the history of interest group theory using as his data base all of the articles and reviews which had ap-

peared in this *Review* since it began publishing in 1906. He also included some 20 frequently cited books dealing with group theory. His principal conclusion was that political science "grows" largely reactively, in response to outside stimuli: "the rise of economic groups, the tides of progressivism and disillusion, the birth and excitement of modern psychology," and the like (p. 1519), rather than through the self-conscious testing of analytical models.

Using group theory as exemplifying this disciplinary propensity, he pointed to the early polemic between the "classic" sovereignty and "classic" pluralist theorists. The classic theory of sovereignty overstated its case, although if one looks carefully at this nineteenth-century literature one will find careful qualifiers of the plenary power of the state. The assertion of the absolute authority of the state ran into the anomalies of the empirical and normative limits on state authority—both the conservative ones of religion and property and the liberal ones of the rights of association, assembly, petition, and the like. Classic pluralism similarly overstated its case, rejecting the very notion of state sovereignty, and characterizing the state as one association among many. By denying any sovereignty whatever to the state, Laski (1917) and the other early pluralists deprived themselves of an agency capable of remedying injustice and providing basic security. Garson points out that the history of political science would have been quite different if these alternatives had been formulated as analytical models, as ideal types, rather than assuming that they modeled reality or that reality ought to be made to conform to them. The contemporary statist in their posing of the pluralist and statist alternatives make a similar mistake.

In a more recent article Andrew McFarland (1987) reviews the history of interest group theory in the period since World War II. Prior to the mid-1960s, he points

out, "the work of Dahl, Truman, Lindblom, and others provided a coherent and convincing theory of power in America. Dahl's polyarchy and the implicit 'economic theory of democracy' provided a theory of elections. Truman's description of American institutions and politics as a complex texture of decentralized bargaining among a myriad of interest groups was readily assimilated into Dahl's pluralist discussion of power. Lindblom's theory of incremental decision making showed why policies emerging from decentralized bargaining within a polyarchy might be both more effective and representative than policies emerging from central government direction" (p. 129).

The disillusionment and demoralization in U.S. politics in the 1960s and 1970s shook the credibility of this model; and in its place a "plural-elitist" model, developed in the work of Lowi (1969) and others, came to be an influential view in the discipline. In this model special interests captured particular areas of public policy, forming "iron triangles" or "sub-governments," not subject to popular control. This pluralist revisionism of the 1970s and 1980s reacted only to the interest group theory of the 1950s and 1960s, and as I shall suggest it failed to register the fact that its pessimism was similar to the pluralist pessimism of the 1930s, which was based on a many-times replicated appreciation of the fact that the power of business interests was substantially more equal than other interests.

A third model associated with the work of James Q. Wilson (1980), McFarland calls the "triadic model." In this model the policy process is viewed as specialized by issue areas. Normally interest groups mobilizing to lobby the government in a particular direction are opposed by countervailing groups, and "state agencies are normally assumed to have a significant degree of autonomy . . . as well as a capacity to give some coherence and continuity to the system" (p. 141). The triad

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consists of the two interest group camps and an autonomous set of state agencies, varying across issue areas.

The important point made by Garson and McFarland from the perspective of the present statism-pluralism polemic is that with the exception of the classic pluralism interlude of the turn-of-the-century period and a few formulations in the 1950s and 1960s (Latham 1952; Golembiewski 1960), the various versions of interest group theory have operated with some measure and form of governmental autonomy.

The intellectual history that I shall review is readily accessible. In an article, "The State," published in the original *Encyclopedia of the Social Sciences*, the political theorist, George Sabine complained a bit about the concept. "The word commonly denotes no class of objects that can be identified exactly, and for the same reason it signifies no list of attributes which bears the sanction of common usage. The word must be defined more or less arbitrarily to meet the exigencies of the system of jurisprudence or political philosophy in which it occurs" (1934, 328). Thirty-four years later in the successor compendium of social science knowledge, *The International Encyclopedia of the Social Sciences*, Frederick Watkins observed that the complexity of the interaction of government and people is such that "political scientists prefer to use other terms in describing the phenomena that once was subsumed under the concept 'state'" (1968, 156).

Watkins acknowledges that the concepts of the state and sovereignty made sense in the century-and-a-half between the Treaty of Westphalia and the French Revolution, during the Age of Absolutism, when Louis XIV could say "l'Etat c'est Moi," and Frederick II could speak of himself as "der erste Diener des Staates." Democratization led to a devaluation of the normative concept of the state. In a democratic society it is hard to distinguish

the state from the citizens in whose name its authority is being exercised: "Sovereignty thus conceived is little more than a bloodless legal fiction. Attention shifts from the state to the government, which though it makes no claim to sovereignty, does all the actual ruling" (p. 153).

If there is any point in the use of the concept, according to Watkins, it is in the Weberian sense "that the distinctive feature of the state, as compared with other associations, is its attempt to monopolize coercive power within its own territory. . . . From the standpoint of a purely descriptive political science it is sufficient, therefore, to define the state in terms of the limit, and to study the conditions that accompany the greater or lesser degrees of monopoly that have been achieved in particular times and places. The difficulty is that it places excessive emphasis on the coercive aspects of political life" (pp. 153-54).

As the state concept fell into disuse in mainstream political science it was replaced by such terms as *government*, and later by *political system*. The tendency to abandon the state concept and replace it by other concepts was attributable to the enormous political mobilization that took place in the Western world in the nineteenth and twentieth centuries and the proliferation of new political institutions—political parties, pressure groups, the mass media, and the like—that accompanied it. The concept of the political system included the phenomena of the state—the legally empowered and legitimately coercive institutions—but it also included these new extralegal and paralegal institutions of political parties, interest groups, media of communication, as well as social institutions such as family, school, church, and the like, insofar as they affected political processes. Political system theory and structural-functionalism were not reductionist of the state and governmental institutions. They grew out of a realism that recognized the processual

character of politics, and examined institutions—legal, paralegal, and informal—in terms of what they actually did. Structural-functionalism was just that; and system theory was a formal acknowledgment that the political process was a set of interdependent subprocesses.

For the Marxists and neo-Marxists, however, the state continued to be a central concept, the instrumentality through which the capitalist class dominated the social order. For the Marxists the assimilation of the state into the political system and its disaggregation into a host of interacting phenomena in mainstream political science represented a fudging of the reality of class struggle. As I shall suggest, for the Marxist intellectual tradition the relation of the state to class struggle continues to the present day to be a central polemical issue. And Skocpol's notion of the autonomy of the state has to be seen in the context of that Marxist, neo-Marxist, and post-Marxist polemic.

The first major assault on this abandonment of the state concept in mainstream political science was that of J. P. Nettl, whose article, "The State As a Conceptual Variable" (1968) appeared in the same year as the publication of Watkins's encyclopedia article. Nettl explained the neglect of the concept of the state in Britain and the United States, and particularly in the United States, by the fact that the phenomena of "stateness" are weak in those countries. But the general point made by Nettl regarding the differing salience of the state in the United States and in continental Europe has been appreciated at least since Tocqueville and has been part of the conventional wisdom of comparative government and politics. Though he advocates a return to the state concept, he does not effectively deal with Sabine's and Watkins's uneasiness about the ambiguity of the term.

He offers a definition of the state with four components. First, "it is a collectivity that summates a set of functions and

structures in order to generalize their applicability" (1968, 562). It is not clear which structures and which functions Nettl includes in this collectivity, although he refers to bureaucracies, parliaments, and even political parties. Second, the state is a unit in international relations; all independent nations, whether having strong or weak states, have this unified international aspect. Third, the state is autonomous; it is a distinct sector of society. And fourth, the state is a sociocultural phenomena, by which Nettl means that the individual members of the state have a generalized cognition and perception of it.

"Stateness," according to Nettl, is a quantitative variable, and the strength and weakness of the state in individual societies can be compared through functional analysis. Nettl includes among these functions, that of central administration, which varies in strength and scope across countries. Thus Anglo-American central administration is weaker than the continental European pattern. Law enforcement patterns differ in strong and weak states; thus in Britain and the United States law and law enforcement have more autonomy than is the case in continental Europe. The legal professions are more clearly independent of government. He also suggests the hypothesis that the strength and the weakness of central administration are related to the strength and weakness of party systems.

Nettl's contribution is an argument to go beyond the mainstream Anglo-American view of the state as the "general area of central government in contradistinction to society" (p. 591) and to examine the proposition that "more or less stateness is a useful variable for comparing western societies, and that the absence or presence of a well-developed concept of state relates to and identifies important empirical differences in these societies" (p. 592).

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When all is said and done, Nettl has disaggregated the state into a variety of components, which he treats as quantifiable and measurable variables. The all-important question for his thesis, how the state concept relates, aggregates, summates, integrates—and so on—such phenomena as bureaucracies, courts, armies, parliaments, parties, interest groups, media of communication, and public opinion is not answered by Nettl. What political phenomena does it include? What does it exclude? How should one weight these components? Like Sabine and Watkins, Nettl is telling us that to measure stateness it is necessary to operationalize and disaggregate the concept. What the state is depends on which operational measures one decides to use. Nettl's argument that nations vary in their degree of stateness, by which he means the scope and extent of governmental power and authority, was similar to the arguments then being made by political scientists using other terms such as *public policy*, *governmental output*, and *governmental performance*. Lowi's threefold classification of policy areas—regulative, distributive, and redistributive—had already been on the table for four years at the time of Nettl's writing and was soon to be used by the former in cross-national comparison. The comparative historical, and economic public policy movements were getting under way in the middle and late 1960s; and they would successfully bring to earth those cross-national contrasts that Nettl was emphasizing in his notion of "stateness."

Another predecessor claimed by the contemporary statist movement is Alfred Stepan whose *State and Society: Peru in Comparative Perspective* appeared in 1978. Stepan is also critical of the "reductionism" of the liberal pluralist and classic Marxist approaches to the state and politics. But he does not go quite as far as the more recent statist. While his area specialty is Latin America, his reduction-

ist critique is addressed to comparative politics generally. The particular point he makes with respect to Latin American studies is that there is an alternative body of theory—the organic-statist-corporatist theoretical tradition—that is more appropriate for the analysis of Latin American and Ibero-European politics, since this theoretical tradition developed in response to the institutional traditions and characteristics of these areas. He describes the liberal-pluralist approach to the state as integrally individualist and as fully committed to the doctrine that there can be no "general good" other than that which flows from the pursuit of the interests of individuals: "A methodological and normative assumption among both political and economic thinkers in the liberal pluralist tradition is that it is undesirable to use the concept of the general good" (p. 7). Stepan also argues that liberal-pluralism treats the state as a dependent variable, that such theories tend systematically to draw attention away from viewing the state as an autonomous agency. This distortion of liberal pluralism in Stepan's work is shared with the contemporary statist movement, and I will return to this question of the place of the general welfare, the state, and the government in pluralist theory and research. It should, however, be pointed out that Stepan's book reflects a more substantial search of the literature than the later work in the new statist tradition.

It is not clear where to locate Eric Nordlinger in this polemic (1981). He stops short of unfurling a new paradigmatic banner (1987). He is empirical and "positivist" to the core. In asserting the autonomy of the state he by no means excludes the power of societal factors. Rather he attributes a societal reductionism to his mainstream colleagues and represents his own work as an important effort to carve out a place for the initiative of governmental officials in the making of public policy. If one corrects for his misreading

of the pluralist tradition, there is no change of paradigm here but rather a research program of considerable promise intended to distinguish among polities according to the degree to which state (governmental) personnel take the initiative in the making of public policy and the factors and conditions that explain these differences in degree. I do not believe that a research program of this kind would strike the typical political science practitioner as a revolutionary departure from mainstream research designs, which, as I shall show, typically accord quite substantial policy initiative and autonomy to political executives, high-level bureaucrats, legislative committees and leaders, and the like.

Nordlinger avoids vague and amorphous formulations. Unlike the statist he offers a definition of the state in operational terms: "The definition of the state must refer to individuals rather than to some other kinds of phenomena, such as 'institutional arrangements' or the legal normative order. Since we are primarily concerned with the making of public policy, a conception of the state that does not have individuals at its core could lead directly into the anthropomorphic and reification fallacies. . . . Only individuals have preferences and engage in actions that make for their realization. And only by making individuals central to the definition can Hegelian implications (substantive and metaphysical) be avoided when referring to the state's preferences" (1981, 9).

Nordlinger has three other components of a definition of the state. The state should be defined to include not only the government and the bureaucratic agencies that derive their authority from it, "it should include all public officials—elective and appointive—at high and low levels—who are involved in the making of public policy" (1981, 10). It should avoid all characterizations that may vary from case to case, such as legitimacy and sover-

eignty. Finally, a definition should seek neutrality; it should avoid attributing specific functions and purposes to all states, such as the preservation of stability or the reproduction of capitalism.

With this carefully empirical set of defining characteristics, Nordlinger attacks what he alleges to be the "near unanimity . . . of all variants of liberal and Marxist writing on democratic politics" on the question of the dominance of societal constraints on the making of public policy. He refutes these imputed theoretical positions by asserting that there are three levels of state autonomy in policy-making and then illustrates them through an analysis of the secondary literature. The lowest level of state autonomy (type 3) refers to situations in which state and society preferences do not diverge and the state acts on its own preferences; that is to say, the particular policy adopted is the one preferred by state officials even though it does not encounter opposition from society. The second level of autonomy is one in which society's preferences differ from the state's preferences, but state officials persuade society to adopt state preferences. The third and highest level (type 1) is represented in situations in which the preferences of state and society diverge; but the state nevertheless enacts a policy consistent with its preferences. This extreme case, crucial to the support of Nordlinger's theory of state autonomy, is illustrated by analysis of the various ways in which the state can play on social divisions and employ the formidable powers of government. In a later paper (1987) Nordlinger offers a fourfold classification of state-society relations in terms of high and low state autonomy and societal support, that has interesting research possibilities. Though Nordlinger fails to—and indeed cannot—document his characterization of liberal scholarship as societally reductionist, there is no doubt that he has made an important contribution to empirical theory. His analysis

is rigorous and heuristic, full of interesting research leads.

Pluralism As Theory and in Empirical Research

The view associated with the Committee on States and Social Structures of the Social Science Research Council (SSRC) is a more integral critique of the so-called waning orthodoxies or paradigms of contemporary political science. These orthodoxies are "pluralism-structural functionalism" and Marxism. They are said to treat the state as a dependent variable; its actions are explained by the interplay of interest groups or of social classes. Thus Theda Skocpol, in her introduction to the first book published under the auspices of this committee, states that the previously dominant pluralist-functionalist approaches viewed government "primarily as an arena within which economic interest groups or normative social movements contended or allied with one another to shape the making of public policy decisions. Those decisions were understood to be allocations of benefits among demanding groups. Research centered on the societal "inputs" to government and on the distributive effects of governmental outputs. Government itself was not taken very seriously as an independent actor, and in comparative research variations in governmental organization were deemed less significant than the general functions shared by the political systems of all societies" (Evans, Rueschemeyer, and Skocpol 1985, 4; cf. Skocpol 1982).

This attribution of society-centeredness to the earlier literature is repeated at other points in this first book of the SSRC Committee (Evans, Rueschemeyer, and Skocpol 1985, chaps. 4, 10). Stephen Krasner, another committee member, comments along these same lines in a review article: "Pure interest group versions of pluralism virtually ignore public actors and institu-

tions. The government is seen as a cash register that totals up and then averages the preferences and political power of societal actors. Government may thus be seen as an arena within which societal actors struggle to insure the success of their own particular preferences. The major function of public officials is to make sure that the game is played fairly. If public institutions are viewed as figurative cash registers or as literal referees, there is no room for anything that could be designated as a state as actor with autonomous preferences capable of manipulating and even restructuring its own society" (1984, 226). According to Krasner, where pluralists do recognize the initiating role of political leaders, they reduce state institutions to "individuals acting in roles" not restrained by institutional "imperatives and restraints." This is a more complex version of the reductionist argument; for Krasner it is an individualist as well as a societal reductionism.

In making the case for this reductionist treatment of the state and government in pluralism and structural functionalism neither Skocpol nor Krasner provides us with a literature search commensurate with the scope and variety of the political science literature about which they are generalizing. They seem to claim that the literature of political science may be subsumed under these two categories of pluralism-structural functionalism and Marxism. Would they include the substantial literature on Communist countries? Surely this literature, even that part of it that experimented with pluralist models, took governmental actors seriously, and dealt with the coercive, extractive, and regulative powers of government in addition to the allocative. Would an examination of the comparative government and politics literature and the country studies of Britain, France, Germany, Italy, and so on, with their emphasis on the power of political executives

and bureaucracies, sustain Skocpol's complaint regarding the neglect of state agencies in mainstream political science? In U.S. studies how would they deal with the substantial literature on the presidency, the Congress, and the courts? How would they dispose of the flurry of publications on military regimes in third-world countries that appeared in the 1960s and 1970s?

If we look at the larger specifically pluralist literature, can we sustain this statist critique? Suppose we examine the ideas of the original statist in order to put both pluralism and statism into historical perspective. And then let us look at the empirical research done by the pluralists. Does it indeed conform to the society-centered reductionist pattern presented by Skocpol, Krasner, and their collaborators? Does it conform to the on-the-whole similar portraits presented by Nettl, Stepan, and Nordlinger?

It is sobering that although their central theme is the affirmation of statism as an alternative to pluralism, neither Skocpol nor Krasner nor the other antipluralists cited here seem to have looked into the original confrontation of statism and pluralism at the end of the nineteenth century and in the early decades of the twentieth. And although their argument for a return to the state rests on the claim that the pluralist empirical literature reduced the state and government to an arena and treated the government as a dependent variable (to the extent that it treated it at all) they simply fail to cite or to discuss the very substantial pluralist "interest group" literature of the last three quarters of a century.

It would not have taken a major search to raise serious questions about the validity of these statist characterizations. There are excellent reviews of the early statist and pluralist literature in the two encyclopedias of the social sciences—articles on the state by George Sabine (1934) and Frederick Watkins (1968) and articles

on pluralism by Francis Coker (1934) and Henry Kariel (1968), all political theorists of repute. Sabine traces the emergence of the state concept from Machiavelli's *il stato* in *The Prince* and the first adoption of the term *secretary of state* for ministerial offices in the reign of Elizabeth. Its usage spread with the rise and diffusion of absolutism. And the notion of sovereignty developed along with it. The emergence of the state in the course of the sixteenth to nineteenth centuries was associated with expansion, aggression, and war making. As a process, the western European state arose out of a logic of aggression or defense against aggression. It was a consequence of the growth of armies and the recruitment and training of civil bureaucracies concerned with the extraction of resources, the procurement of supplies, and the regulation of behavior—all primarily in relation to this military-defense-expansion set of goals. The potential progressiveness of the state resulted from this concentration of power and its separation from the person of the ruler or rulers. As compared with the complicated dynastic, sacred, and secular particularisms that preceded it, it had the potentiality for providing greater personal security, a larger internal market, and perhaps improved systems of law and justice.

The notion of state sovereignty survived the crises of the democratic revolutions. What happened was that the "people" became sovereign or "parliament" became sovereign. But the state was conceived as unitary, as above society, and as legitimately penetrative of the entire territory and population under its jurisdiction.

It was this conception of the centralized, superimposed absolute state, embodied in the sovereign and the agencies of the "crown," that was the target of the pluralists of the late nineteenth and early twentieth centuries. Opposition to this all-powerful notion of state sovereignty came both from the Right and the Left. From the conservative side J. Neville

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Figgis (1914) affirmed the autonomous rights of churches and neighborhood communities. From the Center and the Left Ernest Barker (1930), A. D. Lindsay (1929), and Harold Laski (1919) affirmed the autonomy of economic and professional groups and trade unions. The English theorists drew on German and French authorities—Otto Gierke (1900), Emile Durkheim (1950), and Leon Duguit (1919)—for authority for their anti-monism. What is crucial for the argument of the contemporary statist is Francis Coker's (1934) unequivocal statement that

Neither the pluralists nor their theoretical fore-runners, however, really make the groups independent of the state. Gierke maintained that the state is sovereign—supreme in legal and moral right—where general interests require the exertion of organized social power for their maintenance. Paul Boncour regarded the state as the sole organ of national solidarity, with a duty to prevent any group from acting oppressively toward the public, other groups or its own members. Durkheim ascribed to the state the function of defining general policies, leaving to the several associations only the task of diversifying, under state supervision, the application of the policies according to the special requirements of the several associations. Figgis described the state as the *communitas communitatem* and assigned to it distinctive tasks and a superior authority as the chief agency of social adjustment and coordination. . . . Barker warns against carrying the recent pluralist trends too far; the state as the most embracing scheme of social life must be allowed to adjust the relations of associations to one another and to their members in order to preserve the equality of associations, and protect the individual from the possible tyranny of his own group. (p. 172)

Laski—the most extreme of the anti-monists—goes far towards recommending a powerful state that would own monopolistic industries; regulate the rest of the economy; set basic standards for hours, wages, and working conditions; and fix prices for necessities. Coker concludes,

It appears then that when the pluralists set forth their abstract theory they deny the sovereign power of the state or else characterize the power of something which is properly only ultimate and reserved; but when they devise the specific insti-

tutional arrangements to carry out their theory they assign to the state numerous tasks, in laying down general policies and seeing that they are observed, which obviously require, not an ultimate and reserved, but a very direct and constantly exercised power. . . . They would retain the state but deprive it of sovereignty. It appears, however, that they accomplish this compromise only in words. They allow the state to secure its funds through compulsory taxation, retain the whole traditional system of a compulsory allegiance applied to all members of the community and assign to this comprehensive and coercive association extensive duties in directing the economic and social life of the community. (1934, 173)

In the *International Encyclopedia of the Social Sciences* (1968) Henry Kariel gives a version of pluralist theory somewhat more supportive of the reductionist government-as-arena model. But here he is referring to the earlier formulations of the theorists of pluralism. He reports that by the 1930s "it appeared necessary to some of the pluralists to reintroduce what they had previously banished; a unified purpose above and beyond the will of a plurality of groups. . . . They had rejected such ideas as common good, community interests, and general will. Yet they found it scarcely possible to conceive of the political process without the purposeful, helpful hand of the state, especially as domestic group competition, and foreign threats endangered the viability of a pluralistic political order. Thus both Laski and Cole were finally driven to recognize needs more fundamental than a vibrant group life. They were to argue for both leadership to give expression to these needs and a state equipped to satisfy them" (p. 507). Kariel insists, however, that a latent integral pluralism persists in some unspecified part of mainstream political science.

The pluralist writings that are viewed as coming closest to the portraits presented by the statists are those of Arthur Bentley (1908, 1967), David Truman (1951), and Earl Latham (1952). Bentley's *Process of Government* received almost

no attention for the several decades between its publication in 1908 and the appearance of Truman's book in 1951. In his introduction to the 1967 edition of Bentley's book, Peter Odegard remarks on the fact that Bentley provides little empirical data on interest group activity, despite the fact that pressure groups and the lobby were high salience issues at the turn of the century. His book was essentially theoretical; it came close to presenting a formal model, an analytic reduction of institutions to observable behavior with interest groups the all-powerful explanatory variable. Peter Odegard argues that had the book been more balanced as between modeling and empirical data and had the model been presented as a model rather than as an abstraction of reality, Bentley might not have been so long neglected by scholars. Thus the large U.S. pressure group literature that appeared in the 1920s to 1940s was not influenced by his work; and the pressure group literature of the 1950s and 1960s sometimes cited Bentley, but it did not adopt his group reductionist and behaviorist model. The empirical pressure group research of the first decades of the twentieth century originated in the muckraking tradition of the turn-of-the-century period.

David Truman, the leading group theorist of recent political science, utilized Bentley as an analytical model but devoted almost half of his trail-breaking book to the interactions of interest groups and governmental processes. Though governmental institutions are treated from a group perspective, there is clear recognition of their autonomous decision-making capacity. Thus in his chapter on the executive he points out that

The importance of the executive branch, however, does not derive simply from its size or from the variety of its activities. It is of far greater significance that the operation of these activities necessitates choices among alternate lines of action, the exercise of discretion. . . . Both the newer and the expanded older functions of government create the necessity for a large measure

of administrative discretion, and the tendency is toward the widening of such powers rather than toward their restriction. . . . The obligation to remain minimally accessible to all legitimate interests in the society can supply him with a measure of independence and a persuasive power that effectively supplements his formal authority. . . . Access to the governor or president is not sought by interest groups simply out of habit; the means of leadership in the hands of these officials are not negligible. The president's roles as chief executive, chief legislator, chief of state, and commander of the armed forces are real, not nominal. (1951, 396, 427).

It is not possible save through distortion to describe these several chapters depicting the interaction of groups and government, as simply reflecting the arena or the cash register model. Executives, congressmen, and judges have both authority and discretion.

In his article, "Political Group Analysis" in the *International Encyclopedia of the Social Sciences* (1968), Truman refers to this "referee" model of governmental processes as an oversimplification leading "to the error of treating a limiting case as if it were the norm. This obvious error occurs with great frequency in the literature, despite the fact that even our rough knowledge of the distribution of cases of the relation between interest group and government demonstrates that this oversimplification fails to square with the evidence concerning a vast range of instances" (pp. 243-44).

Earl Latham (1952) does present an integral group theory of U.S. government. He does this by characterizing governmental institutions as groups themselves; and argues the case for a group politics equilibrium model of the policy-making process. But he runs into problems with governmental agencies as groups. He finds that there is a unilateral, asymmetrical quality about government as groups. The policeman can stop traffic; drivers cannot stop the policeman. He gropes in an effort to accommodate the peculiar properties of officials. He rejects Krasner's metaphor of the pluralist state

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as a cash register more than 30 years before Krasner uses it: "In these adjustments of group interest, the legislature does not play the part of inert cash register, ringing up the additions and withdrawals of strength; it is not a mindless balance pointing to and marking the weight and distribution of power among the contending groups" (p. 391). Thus both houses of Congress are groups with senses of identity and with their own interests. Executives, legislators, and judges are "officials"; if they are members of groups, their groups are not of the same order as nonofficial groups. He writes, "The groups so privileged collectively make up the instrumentalities of the state, and such groups are distinguished from others only in their possession of the characteristics of officiality" (p. 389).

The reductionist model, while applicable to the work of Bentley and Latham, can only with difficulty be applied to that of Truman. And it completely fails to capture the empirical interest group research of the 1920s to 1960s, the very literature described as society-centered by the neo-statists. In demonstrating that this literature operated with a very lively sense of the autonomy and discretion of government, both empirically and normatively, I shall cite a selection of Americanists—in particular Pendleton Herring; Elmer Schattschneider, V. O. Key, Jr.; Robert Dahl; and Bauer, Pool, and Dexter—and Harry Eckstein, Henry Ehrmann, and Joseph La Palombara among the Europeanists. On the question of societal reductionism more broadly in the political science literature of the 1960s and 1970s, I shall review briefly some of the U.S. and comparative government text literature of this period.

One of the most influential political science studies of the 1930s was Pendleton Herring's *Public Administration and the Public Interest* (1936). His principal question was how administrative organization in a democracy could resist the onslaught

of particularistic groups and its own conservative propensities and contribute to the general welfare. In the light of the conflicts of the New Deal period he points out that the democratic state must somehow deal with the conflict between capital and labor. "It cannot afford to await the survival of the fittest. The exigencies of the age demand positive state activity in order that democracy itself may continue" (p. 379). He continues, "Unless we are to rest resigned with this situation upon the supposition that this is the inherent nature of the democratic state, there is need for promoting a purpose of the state over and above the purposes of the medley of interests that compose it" (p. 380).

On the autonomy of the state and the need for increasing this autonomy Herring writes, "The need of attempting to formulate an official program in the public interest by a responsible administrative agency arises from the experienced strength of minority groups in pressing their case by propaganda and organized agitation. . . . The offering of positive proposals by a responsible administration is then the first goal to seek. . . . The increased power of the president means, of course, an increase in the importance of the bureaucracy. . . . If the existence of the democratic regime rests upon the assumption that the state exists not for the welfare of any one class but for the benefit of the people as a whole, this great and growing bureaucracy must be guarded from domination by economic groups or social classes" (pp. 383–84). This whole concluding chapter of Pendleton Herring's work of the mid-1930s might well serve as a bible for the contemporary state autonomy movement, even to the point of the use of the same vocabulary.

Elmer Schattschneider, a leading authority on U.S. politics and pressure groups of the 1930s and 1940s, describes the U.S. presidency as the principal rallying point for the great public interest of the nation, the point at which the issues of

public policy are discovered and exploited (1942).

V. O. Key, Jr.—the leading U.S. theorist of parties, elections, public opinion, and pressure groups—whose views are crucial for the statist critique of pluralism, has a long concluding chapter, "Pressure Groups and the General Welfare" in his text on political parties, first published in 1948 and appearing until well into the 1960s. He argues that public officials and political leaders "can frequently obtain acceptance of a policy more nearly reflecting the general interest by going over the heads of pressure group leaders to workers, businessmen, to farmers who have ordinarily a greater sense of community responsibility than do their hired men in the offices of pressure societies" (1952, 174). To illustrate the capacity of government to resist special interests, Key cites major statutes regulating the interests of powerful groups in the 1930s—among them the Public Utility Holding Company Act, and the Food, Drug, and Cosmetic Act—where powerful organized interests were present and actively lobbying against the legislation and where there were no organized groups representing the ordinary citizens whose interests were protected by these acts. These last two examples would come close to representing Nordlinger's type 1 cases of state autonomy.

The two classic political process studies of the 1960s—Dahl's *Who Governs?* (1961) and the Bauer, Pool, and Dexter *American Business and Public Policy* (1963)—both reported results reflecting the autonomy of governmental officials and governmental agencies. In their conclusions on the relative importance of "societal" and governmental agencies in the enactment of the Reciprocal Trade Agreements Act of 1954–55, Bauer, Pool, and Dexter point out that "a Republican administration"—normally most susceptible to the pressure of business interests for protection from foreign competition—

"... felt free to adopt as the most cherished item of its legislative program a Roosevelt measure which transferred tariff making from the legislative to the executive branch" (1963, 466), where, it should be pointed out, tariff making would be less exposed to group pressures. Indeed, their study of business interests and tariff policy in the post-World War II period records the attenuation of "societal" influences and the attainment of "state autonomy" with respect to this field of foreign economic policy. Dahl's study of redevelopment policy-making in New Haven in the 1960s has the well-deserved reputation of having moved the study of policy-making forward by demonstrating the strong relationship between political structure and process and the substance of public policy. Rather than being societally reductionist, Dahl's study was directed against the community power model of C. Wright Mills, Floyd Hunter, and others, which was reductionist, attributing generalized and dominant political influence to the upper business and professional interests. Dahl demonstrated that New Haven had a number of different policy processes that were triggered by different kinds of issues. In the case of redevelopment in New Haven, he shows that Mayor Lee initiated this policy long before there was any interest group or popular movement favoring urban renewal. Krasner (1984) acknowledges that this was the case, but insists that Dahl has reduced "state institutions" to "individuals and roles," failing to take into account the constraints of institutions. I will deal with this version of the reductionist thesis in the context of how statisticians define state autonomy.

In an authoritative work on governmental regulation in the United States, James Q. Wilson (1980) writes in his introduction, "Today the federal government is active with respect to virtually the full range of human affairs, much of its power is exercised, and many of its pur-

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poses are defined, by a large bureaucratic apparatus" (p. vii). In his concluding discussion of the origins of regulation he shows how neither a societal nor a statal "paradigm" would capture reality: "The Civil Aeronautics Board (CAB) was created to help business, but the Occupational Safety and Health Administration (OSHA) was formed over the objections of business. The laws administered by the Environmental Protection Agency (EPA) were enacted over the opposition of those segments of the economy to be regulated. . . . The Shipping Act that was to be administered by what later became known as the Federal Maritime Commission (FMC) was passed over the opposition of the shipowners whom it was to regulate, but with the support of shippers it was supposed to help. Though the Sherman Antitrust Act did not have to overcome well-organized business opposition, neither was the chief impetus for its passage the demand of business" (p. 364).

During the 1920s and 1930s, at a time when U.S. pressure group studies were already thriving, European scholars viewed pressure groups in reductionist and normative terms. The political scientists of the Left tended to reduce pressure groups to class phenomena. Conservative European political scientists viewed pressure groups as political pathologies. Only the state and the bureaucracy had legitimate power and were capable of acting in the general interest.

After World War II a number of empirical studies of interest groups in Britain, France, Germany, Italy, and other countries began to appear. For the purposes of our argument here it may be sufficient to review three exemplars of this literature: Harry Eckstein's study of the British Medical Association (1960), Henry Ehrmann's study of organized business in France (1957), and Joseph La Palombara's (1964) study of Italian interest groups.

In the introduction to his study of the British Medical Association Eckstein

points out,

Pressure is concentrated upon the executive in Britain, first because of the logic of cabinet government in a political system having two highly disciplined parties; such a system precludes any consistently successful exertion of influence through members of Parliament, or, less obviously perhaps, through the political parties. Secondly, pressure is focussed on the executive because the broad scope and technical character of contemporary social and economic policies has led to a considerable shift of functions to the bureaucracy. . . .

"The predominance of skeletal legislation is not due solely to circumstance (e.g. technical legislation) but also reflects the power of the executive which likes to have a relatively free hand." (1960, 18, 20)

He points out that pressure groups are anxious not to get on the wrong side of the government because of the crucial powers that they enjoy over their affairs. Commenting on Bentley's one-sided interpretation of group influence, Eckstein observes, "The state in Britain today disposes directly of 40 percent of the national income, and that fact speaks for itself. We may regard political systems as amalgams of potential and actual pressure groups, groups which from a political standpoint are merely 'categoric' groups, and groups which have actually been drawn into politics, chiefly through the impact of public policies, either policies actually adopted or policies which are threatened. In short, we can usefully stand Bentley on his head to supplement Bentley right side up; if interaction among politically active groups produces policy, policy in turn creates politically active groups" (p. 27).

According to Eckstein the influence and effectiveness of pressure groups is substantially dependent on governmental policy and organization. His approach is neither societally reductionist nor statist reductionist. Like almost all of his pluralist colleagues he sees the policy process as a set of interactions between state and society.

Ehrmann's (1957) study of business groups in France emphasizes the tradition

of central administrative power. He notes the importance of the nationalized industries under the Fourth Republic, the control of money and credit, exports, and convertability, governmental price fixing, and the like. He describes the transformations of the Fifth Republic as shifting power from the parties and parliament to the executive and bureaucracy. In language that would gratify the most "statist" of the neostatists Ehrmann catalogs the major triumphs of President de Gaulle in battles with organized interests:

In regard to major decisions, e.g. the Algerian or the European policies, the liquidation of the African Community, or the preparations for an atomic striking force, the interested lobbies (some of them solidly organized and amply financed) were generally ignored by the Elysee and had difficulties in obtaining a hearing in the offices of the Prime Minister. . . . In two of his most resounding conflicts with the National Assembly the President of the Republic wanted it to be known that he denied to the deputies the exercise of rights granted to them by the wording of the constitution because parliament was in fact obeying the injunction of the agricultural lobby. Such affirmations of the regime's fundamental hostility towards intermediaries standing between 'le pouvoir' and 'le peuple' were designed to dramatize its intentionally authoritarian style. (1963, 278)

To similar effect La Palombara (1964) in his study of Italian interest groups has a whole chapter dealing with the factors that enable the Italian bureaucracy to resist the pressure of interest groups. Among these factors is the culture of the bureaucracy, the sense that the official has of the general or the public interest as he encounters the overtures, blandishments, and threats of pressure organizations: "The typical bureaucrat recognizes that he is involved in a highly politicized process and that the concept of public interest is one of the few meaningful weapons he can utilize in order to assert a bureaucratic decision that is not tied to narrow, particularistic interest group considerations" (p. 384). The socialization of the bureaucrat in the administrative

organization also contributes to the capacity to resist the pressure of special interests. La Palombara concludes from his interviews of Italian officials that "when bureaucrats in large number define their role as requiring the objective application of the law, they generally do so with pride and with a sense of having to protect the bureaucracy—and therefore the nation—against the irrational and particularistic forces at large in the country" (p. 388).

The literature that we have reviewed on pluralist theory, group theory, and the activities of interest groups in the United States and Europe gives the statist polemic the best chance of being sustained. It is after all this literature that has been arguing the case for the influence of "societal" organizations in the making of public policy. Though this literature does by and large stress the importance of interest and pressure groups in policy-making, it clearly does not support the reductionist thesis. Autonomous government agencies are present and important throughout this literature. The pluralist "paradigm" is not the one-sided one of Skocpol, Krasner, and others but rather a two-directional one with the state influencing the society, as well as the society influencing the state.

If the statist polemicists cannot be sustained with respect to that literature most likely to support their thesis, how does it fare with respect to the more general political science literature of the pluralist era, for example in the U.S. and comparative government text books of the 1950s and 1960s? A few examples may make the point. The two leading U.S. textbooks of the 1940s to 1960s, written by William Anderson and the perennial (Frederick) Ogg and (P. Orman) Ray, give great stress to the powers of the U.S. presidency. Anderson speaks of the U.S. executive as "the government in a very real sense." He has "the power to formulate programs of action" (Anderson and Weidner 1953, 578). Ogg and Ray speak of the

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U.S. president as "the most powerful elected executive in the world. He is at once the chief formulator of public policy as embodied in legislation, leader of a major political party boasting thousands of functionaries and millions of adherents, chief architect of U.S. foreign policy, . . . director of one of the most gigantic administrative machines ever created, . . . commander in chief of more than two million men in uniform," and so on (1959, 258).

Austin Ranney in a text of the 1960s describes the president as making a great deal of law: "He is generally regarded as our 'chief legislator,' . . . mainly because he has taken over most of the initiative in the nation's statute-making process" (1966, 442). In the 1963 edition of another leading text on U.S. government, Burns and Peltason's *Government by the People*, the authors also speak of the president as the chief legislator (pp. 424-25). Indeed, without exception, the texts of the pluralist era in U.S. political science refer to the president as the chief legislator and policymaker of the nation.

The texts in comparative government during these decades gave similar stress to the importance of the political executive. Carter and Herz (1972) speak of the political executive in democracies as laying down "the essential lines of policy in every field of domestic and foreign affairs; he must coordinate the innumerable agencies of what, in most modern countries, has become the biggest business establishment"—"the executive branch of government. . . . he must deal with a vast variety of interest groups and be able to resist their often powerful pressures" (p. 152). And in the Samuel Beer and Adam Ulam text edition of 1964, the chapter on Britain describes the British Cabinet as the very center of power and the higher civil service as the initiator and drafter of almost all legislation (p. 103).

The conventional wisdom of the political science literature of this period em-

phasized the importance not only of the political executive but of the bureaucracy. Thus Robert Putnam exclaimed, "Public bureaucracies, staffed largely by permanent civil servants, are responsible for the vast majority of policy initiatives taken by governments. Discretion not merely for deciding individual cases, but for crafting the content of most legislation has passed from the legislature to the executive. Bureaucrats, monopolizing as they do much of the available information of shortcomings of existing policies, as well as much of technical expertise necessary to design practical alternatives, have gained a predominant influence over the evolution of the agenda for decisions" (1973, 17).

It might be expected that the political sociologists would be more likely to manifest the society-centeredness that the statisticians have attributed to the pluralists and the structural functionalists. Since sociologists specialize in social structure and processes, it might be expected that they would incline toward explaining the state and politics in terms of society. In a series of papers on the scope and content of political sociology going back to the 1950s, S. M. Lipset and William Schneider trace the history of this debate from the state-centeredness of the Hegelians to the society-centeredness of Proudhon and Marx. In Max Weber and the later political sociologists the causal arrows go both ways, and both concepts—state and society—are disaggregated into their component parts. Lipset and Schneider argue, "Political sociology can be defined as the study of the interrelationship between society and polity, between social structures and political institutions. It is important to note that this definition does not assign causal priority to society over polity; political sociology is not solely the study of the social factors that condition the political order. Indeed political institutions are themselves social structures, and hence are often the in-

dependent (that is) causal factors that affect nonpolitical social structure" (1973, 400-401).

A Note on Marxism and Structural Functionalism

The evidence on the pluralist literature, both theoretical and empirical, clearly cannot support the neostatist critique of society-centeredness. But the neostatists also included structural functionalism and Marxism in their polemic against society-centeredness. With respect to Marxism, they are without question correct. Skocpol argues that "at the theoretical level, virtually all neo-Marxist writers on the state have retained deeply imbedded society-centered assumptions, not allowing themselves to doubt that, at base, states are inherently shaped by classes or class struggles and function to preserve and expand modes of production" (Evans, Rueschemeyer, and Skocpol 1985, 5). Martin Carnoy's review (1984) of neo-Marxist theories of the state—from Gramsci to Althusser to Poulantzas and Offe—by and large supports Skocpol's (1982) summary.

Indeed, David Easton (1981) characterizes the entire "return-to-the-state" movement as arising out of a contemporary revival of Marxism. He speaks of three Marxist "comings." The first coming occurred at the time of the origins of the Marxist movement, which touched the United States only superficially in the 1880s and 1890s. The second coming occurred in the great depression of the 1930s. This impingement affected U.S. academic life and political science more deeply. The whole, now older, generation of political science was affected by this second wave and by the disillusionments of the late 1930s and 1940s. The third coming of Marxism had taken the form of this neostatist movement—more complex than the first two visitations, but presenting some of the same difficulties.

The polemic against structural functionalism is the least-developed theme in the neostatist movement. It is said to be societally reductionist, but no attention is given to the "structural" part of structural functionalism, which after all deals with state agencies and institutions. "Functionalism" developed out of the realist recognition that legal or customary norms defining the powers of the various institutions usually failed to capture their performance. Often they were quite misleading. Hence the properties of institutions were not taken as normatively given but as something to be researched empirically. And particularly if one wanted to compare political systems across cultures, one had to examine functions as well as normative and legal structures. Structural functionalism is as old in some ways as political science itself. Separation of powers—a structural-functional theory—has been a central theme of political theory at least since Montesquieu.

Neostatist Definitions of the State

I promised to analyze the statist movement on three levels of validity and utility. The first of these was the strong statist position that pluralism-structural functionalism and Marxism were societally reductionist, that they did not attribute autonomy to the state, and hence were inadequate approaches to political explanation. Our analysis of the evidence suggests that this characterization of pluralism both in theory and in empirical research is incorrect. Overwhelmingly, the pluralist literature has been shown to be one in which governmental autonomy is recognized; in which the explanatory logic goes in both directions, from society to the state and from the state to the society. Neostatism seems to be a polemic internal to Marxism, since this intellectual movement has in the past decades oper-

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ated from a social class reductionist set of assumptions.

If the critique by the neostatists of the mainstream literature of political science has not been supported by evidence, it may still be that the statist approach is significantly superior to pluralism-structural functionalism as an approach to explanation, that it specifies the elements and variables of analysis better, that its causal logic is more powerful, and the like. The writers in this genre may have been careless and mistaken in their search of the literature, but their approach to research design may be superior.

The shift from institutionalism to realism in the political science of the late nineteenth and early twentieth centuries was enormously productive. It gave birth to the work of Vilfredo Pareto, Max Weber, Emile Durkheim, Woodrow Wilson, James Bryce, Moissaye Ostrogorski, Roberto Michels, Graham Wallas, George Catlin, Charles Merriam, Harold Lasswell, and Harold Gosnell, among others; to the large empirical literature on political parties and pressure groups; and to realistic studies of governmental institutions and processes. This movement of "realism" assumed that by "unpacking" and disaggregating general and diffuse concepts it was possible to reach a firmer grasp of the reality scholars were seeking to understand, interpret, and explain. We understand liberty, justice, equality, obligation better by spelling out their varieties and their component parts. Similarly, as a part of this movement of realism, legal-institutional concepts such as the state were disaggregated. One spoke of government or of the political system and its various legal-institutional components—executives, bureaucracies, legislatures, and courts—and of agencies and institutions of a paralegal and nonlegal sort such as political parties, interest groups, media of communication, and the like.

The neostatist movement wants to reverse this trend and return to large

and relatively loosely defined concepts such as the state and society. They write about "strong and weak states" and "strong and weak societies," but it is not clear what they mean by strength and weakness. So many dimensions are conflated here that they cannot expect this approach to research to be taken seriously. Indeed, it is an irony of the statist movement that this central concept either is not clearly specified or, when it is specified, is very like the definitions of *government*, *political system*, and other terms denoting the totality of political phenomena employed in the mainstream literature.

Theda Skocpol approximates a definition of the state in her earlier work on revolutions, where she sets aside the neo-Marxist efforts of Miliband, Poulantzas, Anderson, Therborn, and Offe because they fall short of acknowledging the possibility of state autonomy. She writes,

We can make sense of social revolutionary transformations only if we take the state seriously as a macro-structure. The state properly conceived is no mere arena in which socio-economic struggles are fought out. It is rather a set of administrative, policing, and military organizations headed, and more or less coordinated by, an executive authority. Any state first and fundamentally extracts resources from society and deploys these resources to create and support coercive and administrative organizations. Of course, these basic state organizations are built up and must operate within the context of class-divided socio-economic relations as well as within the context of national and international dynamics. Moreover, coercive and administrative organizations are only parts of overall political systems. These systems also may contain institutions through which social interests are represented in state policy-making as well as institutions through which nonstate actors are mobilized to participate in policy implementation. Nevertheless, the administrative and coercive organizations are the basis of state power as such." (1979, 29)

Skocpol is struggling here to free herself from the class determinism of Marxism. She even adopts the system metaphor to accommodate important aspects of politics not easily reducible to coercive and

administrative organizations. But in her later work she comes back to a concept the limits and content of which are unspecified. Thus in her more recent formulation (Evans, Rueschemeyer, and Skocpol 1985) she skirts the question of defining the state. What she does after critiquing the Marxist and pluralist-structural functionalist approaches to the state, is to cite favorably more "state-centered" types of research, such as the work of Nettl, Stepan, and others. For more remote ancestors of the statist approach she cites Max Weber and Otto Hintze. Because her approach is through illustration rather than specification, it is difficult to distinguish the extent to which and the ways in which her state-centered approach would differ from the treatment of the state and government in the work of Herring, V. O. Key, Eckstein, Ehrmann, the text writers in U.S. and comparative government. It is also impossible to determine how it differs from the treatment in structural functionalism of executives, bureaucracies, courts, legislatures, parties, pressure groups; or how it compares with the enormous literature of the 1960s and 1970s that could not properly be subsumed under either of these approaches.

In a later chapter in the SSRC volume (Evans, Rueschemeyer, and Skocpol 1985), in collaboration with Margaret Weir, Skocpol presents a number of diagrams of the policy process. She rejects two that begin the policy process with "socially rooted demands" or "authoritative intellectual developments" in favor of the third one that presents the starting point of policy explanation as "state structures and policy legacies," and in which the activities of politicians, and officials, and demands of social groups are treated as intervening variables. She is clearly insisting here on a "state-centered" explanation of the policy process. I suggest that the important contrast here is between the state-society interaction model of mainstream political science and this state-centered one.

In chapter 2 of this volume Dietrich Rueschemeyer and Peter Evans adopt the Weberian definition of the state as the agency that seeks and maintains a monopoly of legitimate violence in a given territory. This is not substantially different from mainstream efforts to define government and the political system (e.g., Almond and Coleman 1960, 6, 7). And if one reads the fine print in the statist literature it is clear that their definitions are quite similar to mainstream definitions. Leaving aside the neo-Marxist vocabulary, the conclusions of Rueschemeyer and Evans as to the complexity and diversity of state phenomena comparatively considered are really quite eclectic and mainstream: "We recognize that across a range of historical circumstances—in ways that vary substantially—the state tends to be an expression of pacts of domination, to act coherently as a corporate unit, to become an arena of social conflict, and to present itself as the guardian of universal interests" (Evans, Rueschemeyer, and Skocpol 1985, 48).

Stephen Skowronek's *Building a New American State* (1984), often represented as the exemplar of the new statist paradigm, does not offer a definition of the state. The narrative of this important contribution to U.S. administrative and political history, is carried not by the "state" but by the "government," the "administration," or the "bureaucracy" or by particular agencies of government like Congress, courts, and political parties.

Stephen Krasner goes a great deal further in specifying the essence of the statist approach. He attributes five characteristics to the recent statist literature "that distinguish it from orientations associated with the behavioral revolution" (1984, 224). Echoing a criticism of Theda Skocpol, Krasner states that the statist approach views politics more in terms of rule and control than of allocation. Krasner does not seem to be aware here of the political science literature on regulatory policy and agencies, on the emphasis in

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the structural-functional literature on extractive and regulative, as well as the distributive, aspects of politics. The early and influential theoretical work of Lowi (1964) whose threefold classification of types of public policy—the regulative, distributive, and redistributive—has had such great influence in empirical political science research, is also unmentioned by Krasner.

A second characteristic of the literature is the argument that the state is an actor in its own right. "It cannot be understood as a reflection of societal characteristics or preferences" (p. 225). Here Krasner is simply repeating the myth of the societal reductionism of pluralism.

Krasner's third point is that statist studies place more emphasis on institutional constraints on individual behavior. Statists, according to Krasner, accord only a very small scope of discretion and initiative at the individual level: "Actors in the political system, whether individuals or groups, are bound within these structures, which limit, even determine, their conceptions of their own interest and their political resources" (p. 225). On this point Krasner is probably correct. Most mainstream political scientists would draw back from a thoroughgoing determinist position.

Krasner's fourth contrast has to do with viewing political phenomena in a longitudinal and historical perspective. He makes much of the tree model of historical choice points, the argument that a particular choice tends to preclude other options. This decision tree model is, of course, a contribution from behavioral social science. The importance of the longitudinal, historical dimension has long been recognized by behavioral political science, in public opinion and election studies, in the political psychobiographical literature, and in structural-functional analysis. More recently, behaviorist students of the Congress such as Polsby (1968), Lawrence Dodd (1986a, 1986b),

and Cooper and Brady (1981) have exploited the possibilities of the historical approach.

Krasner's fifth and final point is that behavioral political science tends to view political phenomena in equilibrium, process, and compatibility terms and fails to appreciate the importance of conflict and stress. This is difficult to square with the history of pluralism. Pluralism as a normative theory was born out of the struggle of economic, religious, and ethnic groups against the overpowering sovereign state, as well as the effort to conquer the state in order to use its coercive powers to benefit the interests of exploited groups and classes. The pressure group struggle described by Herring, Odegard, Schattschneider, Key and others, hardly took the form of a smoothly purring machine. Political culture research, consociational theory, corporatist theory, and development and modernization theory all treat conflict as central themes.

Thus with the exception of determinism in the structural sense, Krasner has listed a set of properties that do not demarcate statist studies from the main body of political science literature.

Some Concluding Reflections

Thus on two of the three grounds named, the statist movement is in error. Its attribution of societal reductionism to the pluralist-structural functionalist approaches is not supported by the preponderance of evidence. Its assertion that a statist approach is both different from prevailing views in mainstream political science research and is a distinctly improved approach to political explanation also fails to convince. Either it is diffuse or allusive in its formulation of what statism is or its definitions are indistinguishable from "behavioral" or structural-functional definitions of things political. More telling, perhaps, is the fact that in pressing their critique of older political science

approaches to political explanation, the statisticians have fallen into the trap of neglecting nonstatistical variables, such as political parties, interest groups, the media of communication, and the like. From this point of view it is a distinctly inferior model of political explanation to the models they criticize.

If statism fails in its polemic against pluralist reductionism and if its positive formulations do not convince, there still is a third ground on which to give it positive marks—the "heuristic" ground. It is surely the case in the history of scholarship that "stirring things up," even in the absence of substantive merit in a particular intellectual initiative, often proves to have been useful in the net and balance. Scholarly disciplines have a way of settling into ruts, becoming repetitive and endlessly replicatory or concerned with trifling modification and emendation of argument. A scholarly tantrum may have the effect of stirring the senses and alerting the mind to new possibilities.

Aside from generally capturing attention and provoking semantic adaptations on the part of many scholars, it may be argued that statism has drawn attention to institutional and particularly administrative history as a focus for political science research. This is all to the good. But if this, indeed, is the positive achievement of this intellectual episode, then one has to question why the literature of institutional and administrative history was not searched. Why are there no references to the four volumes of L. D. White's administrative history of the United States: *The Federalists* (1956), *The Jeffersonians* (1951), *The Jacksonians* (1954), *The Republican Era, 1868-1901* (1958)? Why no reference to White's *Trends in Public Administration* (1933)? More recently, why no reference to La Palombara's *Bureaucracy and Political Development* (1963), Ezra Suleiman's *Elites in French Society* (1978), John Armstrong's *European Administrative Elite* (1973), or

Robert Putnam's *Comparative Study of Political Elites* (1976)?

If there is something basically at fault with this literature—and I am sure that the statisticians, coming from their primarily neo-Marxist and international relationist backgrounds have something novel to contribute to the tradition of institutional history—it is still a fundamental rule of scholarship that one searches the literature before venturing to improve on it. Nevertheless, on this quite limited ground it may be argued that the statist movement has performed a constructive role.

But at what exorbitant and unnecessary cost! A generation of young scholars has been encouraged to reject much of its scholarly ancestry with little more than a paradigmatic farewell. And they have been urged to adopt ambiguous phraseology in the place of a hard-won tradition of operational rigor.

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THE RETURN TO THE STATE: CRITIQUES

This symposium features three critiques of Gabriel Almond's argument—by Eric A. Nordlinger of Brown University, Theodore J. Lowi of Cornell University, and Sergio Fabbrini of the University of Trento.

"The Return to the State" is almost entirely devoted to a negation of statist claims for distinctiveness. It is executed by highlighting the state-specific components of pluralist and other "mainstream" scholars, and by minimizing the putatively special contributions of the statist. There is something to be said on both counts; but Almond goes too far, much too far. And while Krasner's statist perspective is not fully distinctive, it is possible to delineate one that is considerably more so.

Inflating our Statist "Ancestry"

How are we to know whether pluralism and its offshoots "typically," even "overwhelmingly," accord the state "substantial" autonomy? Almond refers to several reviews of the literature and a panoply of passages within it that deal with the state's autonomy. But their meaning and meaningfulness must be accurately and fully interpreted, beginning with the identification of the literature's central questions, answers, and assumptions. There is no disagreement about the overarching question that informs the work of most "mainstream" (and many Marxist) writers: how to account for the authoritative, highly consequential actions of the state, its public policies broadly conceived.

Almond does not stand back from his citations to interpret the thrust of pluralism's answers, but at one point he inad-

vertently indicates that they are decidedly society-centered—a critical point at that, given the "classic" status of *Who Governs?* (Dahl 1961): "Dahl's study was directed against the community power model of C. Wright Mills, Floyd Hunter, and others, which *was* [societally] reductionist, attributing generalized and dominant political influence to the upper business and professional interests" (italics added). According to Dahl, the political and politically fungible resources of societal actors are by no means limited to wealth, control over poverty, and social status. Other effective resources include numbers, votes, organization, information, expertise, a positive public image, access to the mass media, and nonfinancial kinds of economic leverage. Nor do they cumulate in the same hands; their distribution takes the form of "dispersed inequalities." Dahl's primary answer to the question, *Who governs?*, is thus: Just about everyone. "Virtually no one, and certainly no group of more than a few individuals, is entirely lacking in some influence resources" (p. 228). When set against the elitist depiction of society's political contours Dahl's answer is thus no less "societally reductionist." The working assumptions are the same: public officials are dependent upon societal support and constrained by the politically best-endowed groups.

The point can be made more broadly. Pluralist, neopluralist, and elitist works derive their normative, empirical, and

analytic significance from the plausibility of the societal constraint assumptions, which concomitantly makes it all the more important to identify the private actors with the greatest policy influence. Differing interpretations of the relative effectiveness and distribution of private resources produce alternative portrayals of the effective demand groups, and thus of the extent to which political stratification is egalitarian and governments respond to majority interests. It is these issues—not any having to do with the barely discernible autonomy of the state—that are at the crux of the long-standing debate in which the divergent positions are represented by Dahl and the elitists, with the neopluralists (Lowi and McConnell) situated between them. Almond does not mention McConnell's (1967) important society-centered book or the latest major study to make the case for the exceptionally narrow distribution of effective political resources, Lindblom's *Politics and Markets* (1977). That the primary impact of this analytically and geographically wide-ranging work comes from the argument for "the privileged position of business" in polyarchies underscores the general point. And it is only once, and then ambiguously, that Lindblom speaks to the state's autonomy: "Caught in a potential crossfire between privileged controls and polyarchal controls . . . [government officials seek] to remove from politics those highly divisive issues on which businessmen would be loath to yield" (p. 205).

The societal constraint assumptions of pluralism and its offshoots are readily apparent in the issue typologies that have been most widely applauded and applied. Almond even directs us to them by criticizing Krasner for ignoring a typology (Lowi's) that "has had such great influence in empirical political science research." The same could have been said about the comparable framework developed by Wilson (1973), who also evokes Almond's

appreciation. Analytically, both of them are firmly, almost entirely, anchored in society. Wilson's fourfold classification builds upon the extent to which policy issues entail a narrow or broad incidence of costs and benefits for private actors (e.g., those involving distributed benefits and concentrated costs). Lowi's typology also derives from the issues' significance for societal actors. Are the issues generalized or narrow, aggregative or disaggregative, related or unrelated as they bear upon private interests? Distributive issues involve individuals and firms, regulatory issues bring out more inclusive groups, and redistributive ones are pursued by social classes and peak associations. The common premise, then, is clearly that public officials are regularly responsive to societal demands, pressures, and sanctions. Why else elucidate the ways in which issue differences affect different societal actors and their relative policy influence?

Policy-making studies might well be expected to highlight the import of the officials who formulate, adopt, and implement public policy. Heclo (1974) sets out the explanations that have consistently been used to account for the formation of social policies: "socioeconomic variables," "the electoral process and party competition," interest group power, and "the internal workings of government itself" (pp. 6-9). It is of some significance that only one of them deals with the state and that the "most pervasive tradition identifies [competitive electoral bidding] as central." It is of greater significance that neither the few cited studies nor Heclo's own analysis includes instances or explanations of officials acting upon their policy preferences when these diverge from those of the politically best-endowed groups. Moreover, variables that could be used in both a state- and society-centered manner are exclusively cast in terms of the latter. Changes in a country's gross national product, for example, might explicate the

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state's policy preferences; yet it is only their effect upon the private actors' demands for social policy and their willingness to bear the costs that are deemed relevant. Roughly comparable statements apply to Cameron's (1978) synthesis of the five explanations for the expansion of the industrialized countries' public economies since 1945. Only one of them—the "institutional explanation," as it accounts for the officials' preferences for greater public expenditures—is exclusively state-centered. Only part of another—the officials' opportunities for creating a "fiscal illusion," in the "fiscal explanation"—says anything about *how* the state can act upon its preferences when these diverge from society's.

Despite their society-centered thrust, pluralist and related works may yet provide evidence and hypotheses that underscore the state's autonomy. Almond points to a veritable slew of writings that appear to do so. Yet only Dahl's urban renewal case shows public officials to be acting autonomously after having generated societal support; and only Bauer, Pool, and Dexter's analysis of trade legislation constitutes an instance of autonomous action in the absence of societal support. Nearly all the other references to the state fall into one (most into more than one) of four categories.

First, there are the references that are off the mark, even inadvertently underscoring the impact of societal pressures. Several pluralists are said to conceive of the state as the bearer of the public interest. But this is purely a normative claim when left at that. We wonder whether the state succeeded in transforming these preferences into public policy. And in Laski, Cole, Herring, and Schattschneider the public interest becomes a clarion call, an appeal, just because the state could not overcome societal constraints. How else interpret such phrases as, The state "must be guarded from domination by economic groups or social classes," "The exigencies

of the age demand," There is a "need for," The "first goal" is, and "They were to argue for"—a state that can transcend private pressures so as to enact a positive welfare version of the public interest?

Second, there are the explanations for the officials' ability to turn their nominal powers into decisional power. The autonomy-enhancing conditions include the sizable portion of British gross national product that the state "disposes [of] directly," the French state's nationalized industries and "control of money and credit, exports and convertibility," the Italian bureaucracy's "concept of the public interest, [which is] one of the few meaningful weapons" for fending off interest groups, the U.S. president's "persuasive power" and position as "commander in chief of more than two million men in uniform," and the special knowledge of most bureaucrats that allows them to shape "the agenda for decisions." These capacities and opportunities may well enhance the state's autonomy, but it is not shown that they are sufficient to counteract, neutralize, or circumvent societal constraints. In fact, the larger the number of policy levers and financial resources in the hands of public officials, the greater the societal demands and pressures brought to bear upon the state and the greater its dependence upon societal cooperation for the successful implementation of public policies. Except for the Bauer, Pool, and Dexter study, there is no mention of a single autonomy-enhancing condition that applies to the legislature. And how is it possible to ignore the U.S. state's most remarked-upon feature, the division and dispersal of power and powers as this is said to explain its minimal autonomy? According to one "influential" pluralist, the U.S. state was, and continues to be, divided so that the parts "would be captured by different interests" (Polsby 1971, 140-41).

Third, there are the partial statements, intimating that officials have turned their

preferences into policy, without, however, indicating that they have actually done so. To be "responsible for the vast majority of policy initiatives," to "have the power to formulate programs of action," and to be the "chief legislator" is not necessarily to be acting on one's own preferences, as opposed to acting in order to maintain or acquire societal support. And if officials act on their own preferences, it is not shown that the policy-making efforts succeeded. Initiatives have been withdrawn and drastically compromised due to the intervention and mobilization of private actors, and they have been blocked by other officials who were (more) susceptible to societal pressures. Based upon some selective quotes, we are told that David Truman's "executives, congressmen, and judges have both authority and discretion." Leaving judges aside, where is the evidence of their autonomy? It may be expecting too much of a literature review to cite not just chapter summaries but also empirical verses. But how could Almond ignore the opportunity of a focused "polemic," given what was said (twice) in a statist study about the major work of "the leading group theorist of recent political science"? "Not one of the dozens of state units in Truman's compendious study adopted legislation or regulations that were opposed by the weightiest, or even the potentially strongest, of the interested groups" (Nordlinger 1981, 45, 151).

Lastly, there are a few studies that depict officials translating their own preferences into public policy. Yet even they are incomplete—and thus possibly less than telling—because they fail to indicate whether the state acted with or without the support of the politically best-endowed groups. Whether the latter's preferences coincide with, or diverge from, the state's is a critical issue. Acting on one's own preferences under conditions of divergence is obviously far more problematic, so much so that Krasner

(1978, 55-57) and Skocpol (1979, 24-31) define autonomy exclusively in these terms. How then does one interpret the statement quoted from Key that public officials "can frequently obtain acceptance of a policy more nearly reflecting the general interest by going over the heads of pressure group leaders to workers, to businessmen, to farmers who have ordinarily a greater sense of community responsibility than do their hired men in the offices of pressure societies"? If group members control greater resources than their leaders, if they constitute the more effective demand groups, the officials' autonomy becomes less than impressive. The studies in Wilson's edited volume establish that regulatory policies were enacted over the opposition of particular business interests (e.g., industrial polluters, shipowners). But it is not said whether their political clout was greater than that of the much larger groups who variously welcomed, demanded, and worked for the enactment of regulatory policies. If not, these are instances of what Almond refers to as "the lowest level of state autonomy."

As for conclusions, this statement now speaks for itself: "Nordlinger fails to—and indeed cannot—document his characterization of liberal scholarship as societally reductionist." When taken as a whole, Almond's diverse literatures do take state autonomy as somewhat problematic, and—absent emphases and analytic development—there are some disparate accounts of the state's occasional autonomy. But that much has not been denied by the statist. Nor do Stepan and my society-centered characterizations of earlier writings refer to all the different currents in Almond's "mainstream." If it is thought that Almond's case could be made more persuasively, it should be noted that he has seized just about every opportunity to make it. Among important pluralist and related works he only missed Lowi's (1969)

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"interest group liberalism." Here the "most interested and best organized" groups are not only the most effective private actors to constrain the state, as in McConnell 1967, but they are also the preferred groups, those whose enfranchisement and demands were purposefully promoted by public officials in the 1960s (but not in the 1930s) for pragmatic *and* normative reasons—in the belief that these groups represent all significant societal interests (p. 71).

Statism Clarified

The assessment of the statist begins with some excessive and cramped commentary, moves onto reasonably firm literal ground, and then fails insofar as it hinges upon an inflated reading of statist elements into earlier writings.

With regard to conceptualization, several strictures about the state and its "strength" being defined in a conflated, underspecified, and overly encompassing manner are warranted. But beyond that Almond engages in excess. It is one thing to note Skocpol's inadequate definition of the state. She could also have been criticized for other vague formulations and central conceptual gaps, for the largely illustrative, underdeveloped (not just exaggerated) depiction of a "paradigmatic reorientation," and for her all-too-quick resort (for a statist) to societal explanations before exhausting state-specific possibilities. But it is quite another to dismiss and to conflate inaccurately Skocpol's concern with class-based power disparities with Marxism, impugn her putative "struggle" with it, and then criticize the resulting definition of the state for not being distinctive.

This last comment is also made about definitions of the state that do pass conceptual muster. Yet no statist has claimed definitional singularity; and there is no reason for definitions—as opposed to the

questions and answers that they delimit—to be distinctive. Almond must be aware of the good "positivist" reasons (maximizing reliability, validity, replication, and cumulativeness) for not taking this path. Some conceptualization of "strong and weak states" do have their problems. But is it therefore reasonable to conclude that "this approach to research [cannot] be taken seriously"? Without mentioning its strong and weak state labels, Almond characterized my typology as having "interesting research possibilities." And not only does Krasner's (1978, 55–61) offer at least as many interesting possibilities, it is the centerpiece of an important, analytically developed, well-researched statist study that is not even mentioned.

The argument that statism is not at all distinctive then focuses upon the five characteristics that according to Krasner "distinguish it from orientations associated with the behavioral revolution." One is acknowledged as singular, after an egregiously cramped reading: a clause ("even determine")—in a sentence that delimits its scope, in a paragraph that makes its meaning fully apparent, in an article that does not refer to any works that fit with Almond's reading—is used to turn Krasner's institutionalism into a "thoroughgoing determinist position." This literal excess is also inexplicable. Almond could easily, and presumably would, have made the same point about institutionalism that he does about three other statist orientations—that it is not distinctive.

On a cramped interpretation of Krasner's tersely worded claim, Almond would be correct. Statist concerns with institutional explanations, problems of rule, control, and order, a polity's disjunctures, stresses, and struggles over allocational and procedural issues, and the past as it patterns the institutional present—these have all been analytically developed and investigated within the "mainstream," several of them in one of Almond's impor-

tant contributions (1973). Indeed, the case could have been strengthened by noting Krasner's characterization of statism "as being concerned with two central issues: the extent of state autonomy and the degree of congruity between the state and its environment" (1984, 224). For the latter dilutes the claim of distinctiveness by implicitly assigning roughly equal importance to the societal environment, as in Skowronek's analysis (hardly a "narrative") of industrialization's powerful challenges to the localized, nationally fragmented U.S. state and its consequent adaptation.

On a more generous reading that pays attention to the qualifiers (*emphasis, more, and emphasize*), Krasner is obviously not claiming *de novo* status for four statist characteristics. He is saying—and with considerable warrant—that it is only recently that they have been consistently and tenaciously focused upon the state itself. The fifth distinguishing feature is critical. Krasner says that the state's autonomy is one of the two "central issues," with Almond presumably agreeing, since half his article serves as the underpinning for this terse assertion: "Here Krasner is simply repeating the myth of the societal reductionism of pluralism." The response can be equally terse: for the most part, it is hardly a myth.

The statist perspective is thus—Shall we say discernibly, substantially, or decidedly?—distinctive. And it could be made more so by ordering, delimiting, and then expanding upon Krasner's guidelines. Focusing upon the state as an independent actor turns the most distinctive, analytically significant, and intrinsically important assertion into the perspective's centerpiece. As an independent variable, the focus is upon the state's recurring directives and institutional features as they impact upon its autonomy and society. Krasner's other three characteristics are incorporated insofar as they help ex-

plicate past and current variations in the autonomy and impact of the state.

Before delineating this perspective it is worthwhile commenting upon the relevance of the most important paradigmatic issue that the discipline is currently "working through," that of the rational actor model's explanatory power. As part and parcel of their institutional focus, Skocpol rejects and Krasner sharply downplays analyses rooted in atomistic individual actors. And for whatever reasons, not one acknowledged statist has worked with positive choice theory. Yet in principle and future practice there is no reason why a distinctive statist perspective cannot be derived from it, at least in part. Just as Krasner can claim singular status for statist studies insofar as their institutionalism is focused upon the state and its autonomy, a reliance upon positive choice theory would not necessarily detract from statism's distinctiveness. Leaving aside the costs and benefits of the deductive rigor to be derived from an exclusive concern with narrow self-interests, rational actor analysis and cultural and institutional approaches are not incompatible, inasmuch as they deal with different levels of analysis. Pluralist and statist studies rely heavily upon self-interested motivations. And rational actor analyses can illuminate the state's institutional contours as one kind of collective outcome, as in Mayhew's (1974) interpretation of the consequences that flow from Congress's "electoral connection."

There is, however, a problem. Its presence is implicitly acknowledged in Almond's article, which does not allude to a single positive choice writer (they are surely in the "mainstream") who has assumed or discovered an autonomous official. Beginning with Downs (1957), when these scholars have dealt with state-society relations, they have consistently invoked the societal constraint assumptions. It is not just U.S. congressmen but also the officials of democratic and

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authoritarian regimes in Latin America (Ames 1987) and Africa (Bates 1987), whose rational "survival" interests prompt them to act as entrepreneurs in acquiring and maintaining societal support, as when markets are replaced by marketing boards and other politicized agencies so that officials can distribute favors to the clients upon whom they depend politically. With Almond's article signaling that the autonomy of the state has become (or saying that it always was) part of the conventional wisdom, positive choice theory has good warrant to investigate more than just the officials' reelection and survival dependencies. The behavioral and institutional consequences of other interests, beginning with the officials' search for autonomy, can also be rigorously spelled out. So, too, can the outcomes that derive from the interests of officials who enjoy considerable security of tenure and autonomy vis-à-vis private actors, as with Niskanen's (1971) budget maximizing bureaucrats and their relations with legislative committees. Given the consistent claims of public choice writers that economic slowdowns and similar "irrationalities" are attributable to the interventionist policies adopted by politically dependent officials, it would be worth asking how autonomy-seeking and autonomously positioned officials affect economic performance. That is a question that would certainly fit into a statist perspective.

The State As an Independent Actor and an Independent Variable

The core of this statist perspective features (1) public officials' forming their own policy preferences, the extent to which they do so being explained by the state's internal variations, and the preferences being distinctive vis-à-vis society's; (2) the state acting on its preferences despite their divergence from those of

the most "powerful" private actors, with its internal characteristics patterning the frequency and strategy by which it does so, in the past and present; (3) the state's recurring activities and institutional contours' impacting upon society and the efforts of private actors to constrain the state; (4) and the state enjoying analytical priority with its autonomy determinants being used to identify the most important among the societal actors and variables that impinge upon it.

As an independent actor, the state's agenda of preferences is very much its own. Public officials are minimally malleable—susceptible and receptive to societal preferences—in forming and altering their own. Private interests are treated as "data" to be sifted, interpreted, and evaluated according to the officials' own lights, interests, and priorities. Socioeconomic ills are diagnosed similarly. As an independent variable, it is the state's boundedness, cohesiveness, and differentiation that minimizes its malleability.

A well-bounded state features careerists who remain within its precincts and promotions that are at least partly based upon length of service. Having a bevy of currently relevant experiences and memories to draw upon, identifying closely with their vocation, and being well socialized into its mores, the careerists' preferences are not apt to be shaped by society's. The officials of a cohesive state hold substantially similar preferences and agree upon the decision rules for resolving disputes. Their chief reference groups are thus other officials whom they turn to for information, cues, and guidance when considering options in the formulation, adoption, and implementation of public policies. In a differentiated—corpuscular and specialized—state the many separate units generate their own preferences, and the officials' narrow responsibilities make for policy expertise and subjectively experienced competence.

There is neither need for, nor a disposition toward, "private tutoring."

It is not just that the autonomous state's policy preferences are its own, they are also decidedly distinctive. In their substance and underpinnings they do not regularly coincide with those of any larger or smaller societal associations, groups, strata, ethnic segments, or regions. What public officials do, whom they interact with, where they sit, and what they see and know help generate the singular interests, values, and beliefs underlying their preferences.

Officials attach process meanings and significance—the maintenance of routines and practices, coordination imperatives, convenience, and predictability—to the consideration, formulation, adoption, and implementation of public policies. Their preferences are shaped by organizational needs and rivalries over policy turf, budgets, internal autonomy, and prestige. And, all-too-often forgotten, governing is also an intellectual exercise. It involves distinctive policy frameworks, decisional criteria, and information focused upon the policy options' ramifications, second-order consequences, implementation problems, and their fit with existing policies.

Separated from society by virtue of their directive positions, officials prefer policies that help structure and heighten their autonomy. The institutionalized mitigation of societal constraints not only maximizes the translation of the officials' preferences into public policy, concomitantly satisfying their underlying interests and values, but it also allows officials to act autonomously with less risk, cost, and effort; attend to their internal affairs; experience a sense of secure tenure, and see themselves as authoritative "state" officials.

Due to their institutional positions public officials have a several-sided interest in allowing conceptions of the public interest to shape their preferences. Despite

the analytical scalpels and sledgehammer attacks to which this concept has been subjected, it is "real" insofar as it has real consequences. Because of the officials' "high," strategically situated vantage points and of the value they place upon a judicious self-image and respected role definitions, they come to hold distinctive public interest preferences. Taking advantage of the prestigious institutions within which their offices are embedded, they have an interest in articulating public interest pronouncements so as to gain popular support and ward off private pressures (and a psychological incentive to believe these pronouncements). The long-term, broad-gauged policies that are seen to benefit either society as a whole or its parts may involve the interests of separate state units, the autonomy and legitimacy of the state, the maintenance of the political and economic order, the application of technocratic decisional criteria, responsiveness to the concerns of politically disadvantaged groups, and the acceptance of current sacrifices on behalf of future generations.

Although used interchangeably up to now, the preferences of individual officials are not the same as the state's. The latter represents an amalgamation of the former; the officials' resource-weighted preferences result in a state preference after being aggregated in a conflictual or conciliatory manner. Here institutions enjoy double-barreled explanatory power. The officials' resources (and even some of their skills) are institutionally derived, and their effective use and impact are mediated by institutional norms and decision rules.

How then does the state come to act autonomously? There are five strategies whereby the state translates its preferences into authoritative actions. Ideally, the state does this by combining autonomy-enhancing capacities and opportunities with predominant (not necessarily widespread) societal support. Such a

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(doubly) strong state—and comparably situated officials in other states—is able to realize most of the collective, organizational, and individual interests and values that shape its policy preferences. The autonomy-support combination is represented by the first two of the five strategies:

1. A state that currently enjoys societal support may capitalize upon its autonomy-enhancing capacities and opportunities to solidify it. Public officials act to crystalize the societal preferences that converge with its own, foster deferential and otherwise acquiescent beliefs and norms, and reinforce issue-specific and diffuse political indifference.
2. When the state's preferences diverge from society's, a strong state makes the latter convergent with the former. Public officials convince those with divergent preferences to alter them, persuade them that it is not in their interest to deploy their resources, offer them practically costless benefits to neutralize their opposition, and mobilize the support of currently indifferent societal actors.

An independent state acts—and some officials in other states act—autonomously despite a divergence in state-society preferences. It is unwilling or unable to enlist predominant societal support.

3. Public officials may free themselves from societal constraints by deterring opponents from exploiting their resources, mitigating the extent and effectiveness with which they are deployed, and offsetting and circumventing the resources' constraining effects.

An independent state may also rely exclusively upon its inherent powers.

4. It may simply accept the sanctions

(e.g., protests, investment strikes, voting switches) that are prompted by its authoritative actions.

5. It may use its coercive powers to threaten and repress opponents so as to obviate any challenges and sanctions.

Most states are able to act autonomously with at least some frequency despite divergent state-society preferences. Even democratically elected officials have over a dozen autonomy-enhancing options both for transforming societal preferences and for freeing themselves from societal constraints. With few of these options depending upon exceptional skills or unusual circumstances, some are regularly available and relevant to the issue at hand (Nordlinger 1981, 109–117, 128–39). The state's inherent powers are present by definition, and even a small coercive apparatus is regularly sufficient to threaten and repress opponents (Nordlinger 1987, 378–79).

Given divergent preferences, what is it, then, about the state itself that explains the frequency with which it acts autonomously and the strategy by which it does so? Interests and attitudes have already played their part in shaping the officials' preferences. That leaves their beliefs and perceptions regarding the costs, risks, and opportunities of autonomous behavior. These are captured by two subjective variables that are institutionally determined.

Resilience refers to the officials' perceived capacities for counteracting manifest and potential societal opposition. It varies in accordance with the state's differentiation and cohesiveness. A differentiated state can exploit its diverse skills, expert status, and substantive expertise either to alter societal preferences or to mitigate opposition efforts and their consequences. A cohesive state is singularly advantaged in being able to articulate a coherent and persuasive message for

changing societal preferences and—with its resources being marshaled and coordinated—for fragmenting, neutralizing, and countering the deployment of private resources.

Insulation refers to the belief that the state is minimally dependent upon societal support—support that ranges from endorsements and demonstrations, through votes and campaign contributions, to the calling and calling off of labor and financial strikes. The belief that autonomous action is affordable in the absence of societal support varies with the state's cohesiveness. Dependence upon private actors is lessened when its resources are deployed in a "unitary" fashion, as some (dependent) officials rely upon the resources of other officials and so avoid being fully exposed to societal leverage and as private actors are unable to play officials off against each other. A cohesive state is also better able to insulate itself by a resort to force. There is less of a debilitating divisiveness brought on by officials who oppose coercive measures that contradict their regime norms and downgrade and degrade their own offices.

The statist perspective can then move to another level of analysis, from heuristically prior "statics" to the "dynamics" of the emergence, alteration, and maintenance of the state's internal features. The "historical cure" is not, however, taken indiscriminately. The focus is on the features that were more or less autonomously instituted by the state and affected its capacities for autonomous action in the past and present. It is here that the polity's stresses, crises, and disjunctures take on special import. And because the state is the first and foremost entity to confront, and be constricted by, external actors militarily and economically and depends upon societal extractions to finance its foreign policies, the international environment is especially salient in explicating structural changes and poli-

cies that heighten the state's autonomy (Nordlinger 1987, 385–86).

Last, but by no means least, a distinctive statist perspective provides several paths by which to "return" to society. As an independent variable, there is the impact of the state's consistently salient policies and institutional weight, past and present. Its recurring directives and activities, internal processes, structural arrangements, and symbolic accoutrements help shape political beliefs and norms; levels and strategies of participation and opposition; the political crystallization and alignments of ethnic segments, classes, and interest groups; and the latter's organizational contours, inclusiveness, and specialization. As an institution, the state also helps determine who gets what, when, and how. As an intervening variable between societal demands and policy decisions, it patterns the relative effectiveness of private resources and how they are deployed.

A statist perspective need not—and this one certainly does not—suggest that the state regularly has a greater impact upon society than society upon the state. But without in any way minimizing the importance of societal actors and variables, the state can advantageously be accorded analytical priority. Over and above the state's impact upon society, the "mainstream" is almost exclusively concerned with the aspects of society that directly, indirectly, or tangentially effect the state—its structures, performance, and policies. And a statist framework can readily help identify those with the most frequent and decisive impact. In terms of the present perspective, which societal actors and variables best explain variations in the state's autonomy? Given the identification of certain subjective variables (malleability, resilience, and insulation) as the immediate explanation of its autonomy and their dependence upon certain institutional variables (boundedness, differentiation, and cohesiveness), to what

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extent, in what ways, and on whose part, are these societally determined? When accorded analytical priority, a statist perspective can serve as a beachhead from which to reconnoiter and then move into the societal interior.

At that point not just the putative "polemic," but the entire question of statism's distinctiveness will have been left behind. And deservedly so, as research and theorizing bring state and society together in meaningful, mutually illuminating ways that parallel their policy interactions and patterned relations. Indeed, judging by the writings of Callaghy (1984), Hall (1986), Haggard (1988), and Goldstein (1988), which focus upon institutionally patterned state-society relations, Almond's "generation of young scholars" is already here.

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Gabriel Almond has struck again, and political science will be better for it. The state has returned. But return of the state to political science was in the air long before the SSRC-supported "return-to-the-state movement." As usual for foundations, the SSRC acted like the amateur investor who bought in after the stock was already high.

In other words, the state was never really out. It was only overshadowed. Taking myself as a case in point, although behavioralism was still dominant in the early sixties, I felt no sense of isolation as I shifted my own vocabulary from policy-making to policy, from decisions and issues to "areas of governmental activity," and from categories of policy to "functions of the state." At Chicago in the late sixties we coined *post-behavioral era* to suit our own strengthening inclination to stress institutional phenomena. David Easton published it first, in his 1968

APSA presidential address, and gave it a somewhat different definition; but even so, it had an institutionalist spin to it. In the early 1970s the state was already central to what was becoming a "Cornell school" of political science (in Cornell's "Department of Government"). That university's orientation can be seen even in Almond's citations, reaching from Sabine to more recent members and products: (alphabetically) Aberbach, Krasner, Lowi, Nordlinger, and Skowronek. Another Cornell colleague, Sidney Tarrow, joined me as cochair of the APSA program during Charles E. Lindblom's presidency (1981) to try to hasten the trend by adopting as the program theme, "Restoring the State to Political Science." Still another, Peter Katzenstein, was on the SSRC Committee. Vivian Shue is a China statist.

However, as "restoring the state" became something of a movement, I began to ask myself, "With friends like these, who needs enemies?" Leave aside my annoyance with the fact that the new and more self-conscious statist rarely cite those of us who immediately preceded them. In fact, as Almond argues, they tend to disregard almost as much as do the behaviorists the rich statist or institutionalist tradition that is part of the mainstream political science literature. The political scientists of the thirties, forties, and fifties were strong in their appreciation of the state apparatus. An important part of their training was in traditional political theory; and most of them also studied public administration. For example, most of the "household names" of the fifties and sixties either did their dissertations in public administration or did some early, formative research in that area. Examples drawn directly from Almond's notes include Beer, Burns, Dahl, Herring, Key, Latham, and Truman.

If a poll were run for the number one political scientist of the past 50 years, most of the candidates would be from the

above list, and my own pick would be V. O. Key. Nevertheless, he was typical of his generation: They were all strong on empiricism and *they avoided theorizing*. I never heard any of them say this, but the pattern is too consistent to be accidental. Political science meant realism, empiricism, loyalty to what was seen now. The highest art was description, good narrative. Quantification was important, but it simply meant description taken to a higher and more sophisticated level. Explanation was important too, but that was nothing more than describing two or more things simultaneously, each in relation to the others. Things were quantified wherever they could be; Key and his predecessors—like Gosnell—were past masters at it. Until well into the 1940s, quantification meant aggregate data drawn from county voting statistics, census data, and individual data drawn from legislative roll calls and the few available public opinion studies. Much more data and much better mechanisms for analysis are around today, but frankly, I think there has been little improvement since the 1940s in the uses of politically relevant data. When individual data from sample surveys became available, there was no hesitation or resistance in embracing them. Key and Truman were in many respects the first behaviorists in political science, as can readily be seen in their articles, their SSRC papers, and opportunities to proselytize. But neither they nor the others can be called reductionists, either to the individual or to the society. They were empiricists, in the business of describing political reality and making as much sense of it as possible. And for them political reality was institutional reality—legislatures, executives, parties, policies, elections, and organized groups. Almond is more than merely correct when he insists that these political scientists never forgot that it was a state in one form or another they were dealing with.

However, Almond misses an important point (maybe because he was too close to it), which is that this great generation of political scientists was an atheoretical generation. I am not alone wondering how much greater a work *Southern Politics* (1984) would have been if Key had let himself be inspired by Karl Marx or any other theorizing political scientist—for instance, his colleague Barrington Moore—and had pursued to the fullest the theoretical implications of one-party systems, the suppression of blacks, class voting, and state government control ("state" control) under these circumstances. But theory takes practice, one practices only to reach a desired goal, and theory was not a goal or vocation of this generation of empirical political scientists. In fact, Key's most self-consciously theoretical book, *Public Opinion and American Democracy* (1961), was also his least lasting. Even Lasswell, the most theoretically inclined of that generation—and probably the most erudite—did not sit down and write a theoretical work. His publications were empirical (e.g., *Psychopathology and Politics* [1960]), encyclopedic (*Power and Society* [1950]), or methodological (inventing research topics or methods for others to pursue, such as content analysis, political psychology, policy science, political recruitment). My work for Sayre and Kaufman in New York in the mid-fifties led me at that time to the conclusion that if the wisdom, the insights, the hunches, and the operating generalizations of this generation of empirical political scientists had been collected in a vast oral history project, we might now be on the way to political theory rich enough to pull us out of the shadow of the Great Books. But no member of that generation would step forward and claim to be either the creator or the adherent of theory properly so called. Theory was not a vocation; to speak of oneself, indeed to think of one-

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self, as a theorist or as someone doing theory would have been an unnecessary arrogance.

There were already some signs of a change in self-conception in the late 1950s. Reference was more frequently being made to a distinction between normative theory (the sort of thing actually done by the great thinkers and studies by political scientists who call themselves political theorists) and empirical theory (an oxymoron intended to locate a place some safe distance from Aristotle for twentieth-century mortals to try to make their research findings outlast the next issue of the *Review*).

Among the many probable causes—or provocations—of the new tendency toward theory, the three I find most interesting are the behavioral critique, the power structure critique, and the critique implicit in the experience with research outside the Western tradition. The emerging urge for the new vocation of theory was, in my opinion, the best thing that could have happened to political science. But since it was new and since it was to a large extent a reaction to outside provocations, the results were not going to be complete or pretty.

Behavioral Provocation

Psychology came out of World War II as the hot field. It had made a direct contribution to the war effort, whereas political science as a discipline had made none. It was a more relevant science for the postwar period because it offered a more promising approach to understanding fascism. And psychology had made the most outstanding breakthroughs in systematic studies of masses. The ultimate affront came when the National Science Foundation was established, including psychology at the very core of science while leaving political science on the outside looking in. Since many behavioral psychologists were already looking at

voting because of its firmness as a behavioral datum, it was quite easy for political scientists to jump on the psychology bandwagon and to look upon individual units of opinion and of choice as superior both to aggregate data and to impressionistic data used by empirical political scientists. For many purposes it was superior and still is. But every solution is its own problem, and the problem of emulating a sister discipline is the likelihood of picking up its theoretical perspective as well as its data and methods. From the perspective of any definition of the political, behavioral psychology is reductionist.

My own opinion is that this—not pluralism as such—is the main source of reductionism in political science. The most ardent behavioralists were the most ardently committed to the individual as a unit of analysis and the most impatient with institutions and history. Almond observes quite accurately that Bentley, the prime reductionist, lay virtually unappreciated between 1908 and his rediscovery by David Truman in 1951. Almond is also correct in his observation that although Bentley was sometimes cited, his reductionism to the individual or to the group was not really adopted. However, the behavioralists—though unaware of Bentley as such—would have made him proud. Bentley was crying out for a unit equivalent to the atom. That was the only way to eliminate "soul stuff" like state and sovereignty. Behavioralists found it. As "empirical theory" came into fashion, the behavioralists were ludicrous in their efforts to build from individual data to the structure within which the behavior was taking place, without taking into account any information independent of their survey data.

The most important theoretical universe for the behavioralists was "the political system," especially as elaborated in Easton's book by that name (1960). But note the great distance between the individual units (more attitudinal than be-

havioral) and the system, the concreteness of the individual data and the total abstraction of the political system, and the substantive emptiness of the political system. This, to those of us who were emerging institutionalists or statist, was the most frustrating part. Nobody, including Easton himself, looked inside the "little black box" on which inputs, outputs, and feedbacks operated. Easton tried such accommodations as "with-inputs"; and I was flattered by his interest in my arenas-of-power scheme as a possible means of filling in the contents of the little black box. But that was one of the problems of the political system. Its very abstractness and generality made it too accommodating. It was everything, therefore it was nothing. Note finally that when Easton and Eastonites turned empirical within the systems context, they literally stepped outside the political system altogether and studied political socialization. Now the interesting thing about political socialization is precisely that it takes place at a point outside, where people are not yet in but are ready to enter the political system. No wonder we would soon think of political socialization as something akin to what some critics said (with less justification) of the music of Debussy—it opened up a new street that turned out to be a blind alley.

Power Structure Provocation

First was a critique of empirical political science by the political sociologists. Community studies had always been prominent in sociology, and many a young political scientist was inspired by reading *Yankee City* and *Middletown*. But with Floyd Hunter's *Community Power Structure* (1953) something new was added. First, it was plainly more systematic than its predecessors. Better yet, its methods made community studies replicable and its findings testable. And

testing they needed: the greatest contribution of Hunter's study was not its strength but its flaws, which provoked responses from a great number of political scientists. There is no need to replay them here, but they galvanized what eventually became a movement within political science, not yet a movement to embrace self-consciously something called theory but at least a movement to strive toward something in the long run that might be more important, to lift the discourse. I stress that: to lift the discourse. The reaction in political science was not to reject the individual or the group as units or to reject social forces or to reject any existing data or methods. The reaction was to lift the discourse toward (or back toward) the institutions and controls that make politics politics.

If I were required to identify one person as a link between the atheoretical empiricism and behavioralism on the one side and the vocation of theory on the other, I would choose Robert Dahl. Dahl had already been up to something that might eventually have been called theory in his collaboration with Charles E. Lindblom in *Politics, Economics, and Welfare* (1953). And Dahl's graduate seminar at Yale was entitled, *The Policy Making Process*. During the year I took the seminar, 1954/55, Dahl tried out some of the ideas that were to become central to *A Preface to Democratic Theory* (1956); but there was no self-conscious pluralist theory that I can recall, and the readings for the year were from a mainstream literature deeply preoccupied with the institutions of government. There was nothing socially reductionist about anything we did. But something else had happened a few months before that academic year had begun, namely, the publication of Floyd Hunter's book. In what turned out to be a quite direct response to Hunter, Dahl proposed that we do a group project rather than individual research projects for our year-long assignment and that the

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group project be a "power structure" study of the nearby community of Guilford, Connecticut. Guilford was more manageable than the larger city of New Haven, and recent events involving local education policies made Guilford a more interesting place to study. This was a first exposure of Dahl as well as the graduate students to the study of the distribution of power.

However, before these ideas would culminate in *Who Governs?* (1961), Dahl was to write the theoretical foundations for it in *A Preface to Democratic Theory* (1956). I really doubt Dahl would have chosen to do it in that sequence; but as so often happens, our agendas are set by others, in this case the invitation by the University of Chicago to deliver the Walgreen lectures in 1955. The *Preface* was, to my knowledge, the first self-conscious effort to piece a consistent and sustained "democratic theory" together from what is known now (but was not known then) as the pluralist literature. As Almond reports, Truman, Latham and others had made some sage reflections; and Sir Ernest Barker, Francis Coker, and two men unfortunately neglected by Almond—Robert MacIver and Merle Fainsod—had made important observations about the independence and lack of independence of individuals and groups from the state. Dahl had pulled it all together:

It is significant that C. Wright Mills' *Power Elite* appeared the same year (1956). Just as Dahl's book was not a reaction to Mills, Mills' book was a reaction neither to Dahl nor, interestingly, to Hunter. Hunter was only twice cited by Mills, and only once substantively, to confirm Mills' view that "power does not reside in . . . middle-level organizations; key decisions are not made by their membership" (1956, 37). However, the pure coincidence of the publication dates of those two important books only serves to confirm the point, which is that theory as a vocation was now in the air. What we

called political theory suffered atrociously in comparison to Marx, Mill, or Madison. But the vocation of theory was emerging, out of the struggle between elitism and pluralism.

Non-Western Countries Provocation

Most of the best field research on every country in the world was done by U.S. political scientists—and in many countries outside Europe U.S. scholars were doing the only empirical research. Almond (1960) himself was a significant factor especially in his cooperation with James Coleman on an approach to theory clumsily called structural functionalism, which tried to build on the irreducible (not at all reductionist) idea that every identifiable institution in society had to meet some basic social need (or "functional prerequisite") or else it—and probably the society itself—would decline or collapse. Structural functionalism is a very powerful notion; it is a fulcrum that can help lever empirical work toward a theoretically interesting statement. Far from being reductionist, this approach to a more sustained discourse takes the view that the organized and repeated patterns described empirically become an index to the society itself and, ultimately, to all societies.

Both pluralism and structural functionalism came under attack in the late 1960s not for being reductionist but for being biased in favor of the status quo. This was an accurate observation, coming from another theory source, the critics of the new democratic Left. This might qualify as a fourth provocation except that (1) it was, in political science, a direct offshoot of the Mills-type critique and (2) by the time these critics arrived on the scene, political science was already committed to theory as a meaningful vocation. The leftist critique strengthened all would-be

theorists by making them more conscious that all theory has political implications; all theory is a political act. Self-consciousness, like everything else in the vocation of theory, takes practice.

My own work in the late sixties was very much a part of the critical reaction (Lowi 1969), and I continue to embrace not only the criticisms I made but many of the criticisms made by people to the left of me. But criticism does not require rejection. Pluralism and structural functionalism were vitally important in the history of political science. For political science to have a vocation of theory at all, we had to get started, and we had to make our mistakes. The beauty of pluralism and structural functionalism was that they were both political and were not merely a mimicking of the other social sciences.

The most serious problem of these early political science theories has, however, not yet been fully confronted. Although Almond recognized it, he was being too defensive to give it its due. This is what Almond referred to as society-centeredness. Almond quotes Skocpol as claiming that the pluralist and functionalist approaches viewed government "primarily as an arena within which economic interest groups or normative social movements contended or allied with one another to shape the making of public policy decisions. . . . Government itself was not taken very seriously as an independent actor" and Krasner as confirming this criticism: "Pure interest group versions of pluralism virtually ignore public actors and institutions. . . . Government may thus be seen as an arena within which societal actors struggle to insure the success of their own particular preferences, . . . [leaving] no room for anything that could be designated as a state as actor with autonomous preferences capable of manipulating and even restructuring its own society."

Skocpol and Krasner seem to me to be absolutely correct in this. If I am disap-

pointed it is only because they did not cite others who made the same criticism a decade and more before them. Take the following example from my work of 1969:

Competition and its variant, bargaining, are types of conflict distinguishable by the existence of rules. . . . But rules and their applications imply the existence of a framework of controls and institutions separate from the competition itself. Whether we call this a public or not, *there is a political context that is not itself competition within which political competition takes place.* . . . [And] rules and their enforcement do not merely exist. They must be applied with regularity and some degree of consistency if pluralist competition is to exist at all. This is administration. Administration is necessary to construct and to change the system within which pluralism is to operate, yet pluralism presupposes the existence of that favorable structure. . . . [An] administrative component must be there. . . . [or] neither the reality nor the theory of pluralism has any meaning. (Lowi 1969, 37-39, emphasis original).

And although the statist critics are correct that there is something socially centered about the notion of arenas "within which societal actors struggle to insure the success of their own particular preferences" (in Krasner's words, quoted by Almond) arenas were not always treated merely as "a cash register that totals up and then averages the preferences and political power of societal actors." Again with Almond, people did appreciate the state and state institutions before the bring-in-the-state movement came along. The concept of arenas is a good concluding point because it ties back into a basic flaw in the new statist approach.

In the first place, the concept of arenas is very much a part of state-centered, institutionalist political science. It has encouraged many political scientists to study public policy, and it has influenced many already policy-oriented political scientists to look more seriously at politics within a state-control context. One of its particular strengths is that there is more than one arena, reflecting the two statist realities, that the state (1) is not neutral

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and (2) is more than one kind of force in the society. That points directly to my understanding of the most serious flaw in the statist approach. If this is a restatement of one of Almond's criticisms, it deserves repetition: the statist hit on a good idea—all the better because it is an old one. But they went too far with it. They reified the concept. They somehow came to the conclusion that "the state" is a unitary, solitary reality that can be brought into theory as some kind of a measurable force. When the statist speak of "state structures," for example, they are getting into empirical realities and are, in my opinion, quite off base. They speak of relative degrees of autonomy (Nordlinger) and relative degrees of strength or weakness (Skocpol). And they tend to use policy case studies as a way of measuring state autonomy and strength even as they treat the state as a macrostructure within which the struggle takes place. States can in fact be strong or weak, and states can be more or less autonomous. But there is no single dimension of strength, and there is no single dimension of autonomy. In fact there are several dimensions of each, and any given state could be placed high or low on any of the dimensions of strength or autonomy. This is precisely why embrace of "the state" as a unitary concept will inevitably be poorly defined. There are too many characteristics of states, and too many of these characteristics are really continua, with every state placing at a different point on each continuum.

Experience with the new statist movement, enriched by Almond's insightful review, has led me to some conclusions about the state and its role in political science. State—like power—is a term of art. It demands a particular focus. It is a residual category, applied to the explanation when all known measurable and controllable factors have been exhausted. It is not a phenomenon that can be studied directly. Although studying power as

such came to naught, political science would be virtually helpless without it. Nevertheless, power alone, even as an organizing concept in political science, was insufficient to drive the discipline, mainly because it was too situational, too likely to lead the observer away from control and from history—in a word, away from institutions. State is a worthy addition, not as a new variable but as a change of focus in political theory. State is really not needed so much in everyday empirical political analysis. But it does become significant when empiricists seek to lift their discourse toward a line of argument that permits them to join with, or engage, others who do not care for their empirical situation but care a great deal about how people are governed. The older generation of political scientists appreciated the state but can be criticized for two kinds of neglect: neglect of theory and neglect of responsibility. Theory means subordinating empirical experience to the long line of argument. Responsibility means subordinating experience *and* theory to a good constitution. I am not yet satisfied that the new statist movement has moved political science much further toward meeting either of those two needs. The realism of the older generation left them open to the criticism that they were ahistorical, nontheoretical, nonnormative, and, as a consequence, apologetic. The new statist may be bringing back the state, but for the wrong reasons. The point is not to make the state a variable but to make political science, through a new and higher level of discourse, a discipline worthy of constitutional democracy—scientific, theoretic, historical, and critical.

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Almond's essay, "The Return to the State," provides us with a useful oppor-

tunity to take stock of the debate on the state that has by now been in progress among scholars of politics for a decade.

With his habitual elegance and with the authority that derives from his acknowledged mastery of the vast field of postwar scientific thought in the political field, Almond examines the logic that has and does provide the basis for the statist critique of the mainstream of political science; that is to say, he does not limit himself to a confutation of one or more of the constitutive arguments of such a critique but rather directs his attack at its very foundations, its conceptual structure and analytic method. Almond has set himself an important and difficult task. If he achieves a degree of success, the terms of the theoretical debate between "mainstream" and "statist" political scientists will have to be substantially revised.

Almond's attack has three objectives: to demonstrate (1) that an interpretation in society-centered terms of pluralist and structural functionalist postwar literature, an interpretation that has undoubtedly served as a "rallying point" for the various studies that have come to constitute statist literature, is baseless; (2) that the claim by statist literature that it has formulated a new theoretical paradigm better suited than those it criticizes to interpretation of political processes is baseless—which is further demonstrated by the fact that in practice such a "new" paradigm is a retrograde step for analysis, inasmuch as it displays the limits of reductionism and determinism that it erroneously attributes to the mainstream of political science; and (3) that the benefits that may (and do) accrue to political research as a result of this "return to the state" have been far outweighed by its cost to the scientific community. Whereas Lindblom (1982), in his discussion of radical political theory, reaches the conclusion that mainstream political science can be enriched by the critique that the theory had directed against it, Almond,

although motivated by the same spirit, reaches a different conclusion: Arguments that have for some time now belonged to mainstream political science are advanced by the statist critique couched in much more confused and contradictory terms. In short, *Il faut tourner la page*.

Has Almond Achieved his objectives? As far as the first is concerned, it seems reasonable to conclude that he has been unequivocally successful. With patience and with a meticulous use of quotations (thus employing a debating style that deserves praise), Almond demonstrates that the statist critique can be partially justified only when it applies to classical pluralism, which certainly underestimated the amount of autonomy available to the state in the policy-making process. In Almond's view, however, this underestimation has by no means been a feature of postwar literature—when, of course, by *state* is meant the government, or, better, that group of public officials and elective personnel operating in agencies endowed with decision-making resources (Easton 1960, chap. 4). The (particularly effective) argument that Almond adopts in reaching this conclusion consists of the consideration and discussion of mainstream literature as a whole (or at least its constitutive components—political theory, comparative government, American studies, and European studies). Despite claims to the contrary, Almond argues, this literature does not conceive of the state (the government) as a "cash register"; it has in a more concrete fashion assigned to empirical investigation the task of identifying the nature of the specific relations among (both private and public) actors in the various contexts of policy examined (e.g., most notably, Lowi 1964, which is extensively and positively quoted by several authors of a statist orientation).

The mainstream of political science has therefore worked with a paradigm that treats the relationship between state and

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society in terms of an interactive system of reciprocal influences. On the basis of this paradigm categories of analysis have been derived (among which *political process, policy area, and political structure* have been of especial importance) that have enabled researchers to reconstruct the precise relations of influence between the two entities, although they have recognized that the relationship is an asymmetrical one ("The policeman can stop traffic; drivers cannot stop the policeman"). Unlike previous legalist and formalist tradition, which conceived of the state as an abstract set of norms and institutions, postwar literature, with its roots in the behavioral revolution, has "denuded the term of most of its implication" (Easton 1968, 283). The state, "reduced simply to a neutral and empty conceptual shell for identifying the actors in the international sphere" (p. 283), has therefore been turned into a methodologically operative concept, an individual unit of analysis that can be subjected to empirical investigation. While, "as a result of this revolution in methods and data, political science," for Easton, "has begun to break away in earnest from an institutional orientation" (p. 291) and "processes, rather than structures and institutions, have begun to appear as the major guidelines for political research" (p. 291), Almond argues that the mainstream of postwar political science has continued to address those key questions that have traditionally inspired political thought (including questions relative to political authority and the exercise of rule), and in fact it has provided more sophisticated and more satisfying answers to them. Easton wrote 20 years ago, "The new conceptualization of the key problems with which political science deals does not in itself preclude the continuation of past concerns in political research" (1968, 288).

Almond's reply undoubtedly rescues pluralist-structuralist functionalist

literature from the criticism of determinism that had been leveled at it and obliges the proponents of such criticism either to concede the point or to reformulate it with much better documentation and with references to that general body of scientific knowledge that the literature presently incorporates. However, it should be added that Almond's treatment of Marxism can hardly be regarded as exhaustive: an author from that field of study needs to make a direct contribution to the debate. In fact, the complexity of Marxist debate over "the capitalist nature of the state" and "the relative autonomy of the state" precludes an excessively simplified reading (cf. Alford and Friedland 1985, pt. 3; Carnoy 1984; Kasinitz 1983; and Zeitlin 1980), not only because different positions have emerged during the debate (e.g., Block 1977a, 1977b, 1981 and Offe 1975, 1984), but also because, as Easton points out, some "Marxists have effectively given up the concept [of the state] insofar as they use it simply as a rough substitute for governmental apparatus, or what I would prefer to label the political authorities" (1981, 320). If this is true, then Marxist analysis, too (at least as in some respects), should now be ready to employ the instruments and methods of an empirical theory, which by its nature is hard to reconcile with a determinist framework. However, I would argue that Almond's assessment of the society-centered nature of Marxism could also be subjected to more exhaustive debate.

Can we say that Almond has achieved his other two objectives with equal success?

State and Political Institutions

Almond's reply has lent implausibility to any claim that the mainstream of political science does not possess a concept of the state (of the government) as an actor. Much more plausible, however, appears the criticism that mainstream politi-

cal science has undervalued the importance of the institutional components of the political system. Easton (1968, 292) has stated that among the political scientists whose literary output since the war now constitutes mainstream political thought the conviction has grown "that the specific institutional setting is of less importance than are the generic forms of behavior that appear in the various institutions, at least for better understanding the way in which the institutions themselves function." Thus the study of executive and legislative, of judicial and administrative, institutions becomes chiefly the study of their specific forms of behavior. And these, in turn, reflect the dynamics of the behavioral interaction among individuals and subunits as they perform the roles, also specific, that constitute such institutions (cf., esp., Eulau 1968). The political structure, by necessity, therefore comes to be regarded principally as a relation among roles (rarely as a relation among institutions and never as a relation among organizational principles). This, it seems to me, is essentially Almond and Powell's (1978, chap. 3) position, although they provide a much more refined treatment.

Almond is probably correct to criticize the vagueness with which certain concepts ("state" and "society," "weak state" and "strong state") have been introduced into the scientific debate by some scholars of a statist orientation. However, he does not appear willing to draw any analytical conclusions as to the reasons for the revival of concepts that seemed to have been definitively superseded in the scientific development of political science.

In particular, two phenomena—the enormous expansion of public intervention in the postwar period among advanced democracies in general (with their well-known national differences) and the relative coherence with which this intervention was conducted (to the point of giving shape, for example, to specific na-

tional models of welfare state)—seem to have altered the nature of the relations between public and private. (An enormous amount of thinking on the subject has given rise to a genuine literature of renewed political economy centering on analysis of the "state" and the "market." See, e.g., Flora and Heidenheimer 1981; Goldthorpe 1984; Lane 1985.) Moreover, the development of the welfare state was accompanied by an increased complexity of the (chiefly state) institutional arrangements (Rose 1984), which made manifest the degree of control—in terms of conditioning and not necessarily of determination; the distinction is methodologically important and empirically verifiable and greatly facilitates interpretation of the various components of statist literature—exercised by these arrangements over both social dynamics and the political processes themselves. It seems reasonable to suppose that the reemergence of the problem of the state has been due not to the efforts of scholars of a statist orientation but to these real processes and the questions they have raised.

Elkin has written (1985, 2), "But several features of twentieth-century democratic political economies have made this [mainstream] view of the connection between society and the state increasingly problematic." He therefore asks (p. 4), "Has not the rise of America as a world power in the atomic age prepared us to think in statist terms?" Perhaps it is no coincidence that the reintroduction of the state into debate in political science has been due to scholars of political economy (leaving aside authors on international politics, who have in fact never relinquished the term). For example, Dunleavy (1987, 470) has pointed out, "The ageing of the conflicts between institutional and behavioral methods, and between the apostles of a 'positive' political science and defenders of normative political thought, has coincided with the strong revival of two political economy approaches closely linked

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with different theories of the state. The first is public choice theory. . . . The second political economy approach is neo-Marxist." Elkin (1985, 6) notes that dissatisfaction with the latter approach has led to: "an eclectic political economy whose starting point is typically the state officials themselves or whatever institutional arrangements are labeled the state."

The clearly central role that state institutions have come to play within the various national political systems seems also to have encouraged a revival of interest among scholars in the historical role played by these institutions in the shaping of the identity of political actors. (See Katznelson 1981; Katznelson and Zolberg 1984; Shefter 1978; Salisbury 1979 and Wilson 1982 address the labor unions' organizational characteristics, which, together with those of state institutions, have made a neocorporatist policy impracticable.) This awareness of the increasingly institutional nature of contemporary politics has not translated, and does not necessarily translate, into a state-centered paradigm, even if this awareness has in many cases attributed a causal position to political institutions—though part of the literature did move toward a (deterministic) state-centered paradigm. March and Olsen (1984, 738) have recently summarized the new analytical approach as follows:

"Without denying the importance of both the social context of politics and the motives of individual actors, the new institutionalism insists on a more autonomous role for political institutions. . . . Political democracy depends not only on economic and social condition but also on the design of political institutions. The bureaucratic agency, the legislative committee, and the appellate court are arenas for contending social forces, but they are also collections of standard operating procedure and structure that *define* and defend interest. They are political actors in their own right." (my emphasis)

One gains the impression that Almond not only underestimates this reawakening of interest in political institutions but also

the effort that has been made—which is certainly to be found in a part of statist literature—to specify the reasons for the relative coherence displayed by specifically state political institutions in the course of their evolution.

The point has been made in the following manner by Benjamin and Duvall (1985, 55), who have a somewhat restrictive view of the state as a set of administrative agencies, or at least of what they call "State I": "It makes little sense to conceive of the state simply as the set of disparate administrative agencies, each doing its own separate thing. The state is that set of agencies as a coherent whole, or totality. Rather than being concerned with what the separate agencies do, the theorist of the state must be attentive to the organizing principles that give coherence to the disparate set. It is a matter of seeing ordering principles in apparent chaos." Such an analytic approach sees political institutions not only as actors and structures that define interests but also (in the case of state institutions) as being generated by principles of organization that allow their identification as parts of a relatively coherent whole, or better, as components of a specific subsystem.

These are certainly not extraordinarily innovative conclusions for social science. For example, 20 years ago Eisenstadt (1968, 410) stated that "institutions . . . can be defined . . . as regulative principles which organize most of the activities in a society into definite organizational patterns" and took pains to point out that "institutions are very close to, but not identical with, groups or roles that are organized around special societal goals or functions. Thus, not only are the principles of political regulation effective with regard to those groups whose major function is some kind of political activity—be it administration or mobilization of power—but they also regulate various aspects of groups whose predomi-

nant goal or function is economic, cultural or educational." Therefore the revival of interest in political institutions seems to derive from a need to specify what we might call the "institutional structure" of the political system, an important component of which is undoubtedly the structure of the state institutions. Moreover, a preference for an institutional (specifically, state) approach to the interpretation of political processes is not alien to the mainstream literature of political science: consider, for instance, Rokkan (1970) and Tilly (1975) and the role played by institutional factors in their interpretations of political development and, especially, Huntington (1968) and his fundamental analysis of the processes of institutionalization.

It would be interesting to examine the emphasis—to be found in the best statist literature—on the institutional structures that organize the political process, policy-making in particular. However, I shall mention only the research conducted by Katzenstein (1978) into international economic policies (published more than 10 years ago), which is still one of the most important accounts in the literature. In presenting his research, Katzenstein justifies his use of the statist approach by citing the arguments advanced by the Weberian sociologist Bendix and his colleagues (1973, 11), according to whom "social life is structured—not exclusively of course, but structured nonetheless—by just those formal institutional mechanisms. To disregard such structures at least implies the belief that social reality is essentially amorphous. This does not mean that institutions work as they are intended to work; it *does mean that they have an effect*" (my emphasis). The problem that now arises, I believe, concerns the specification of the characteristics and limits of such a structure, especially of its state component, which—for objective reasons—is the most important. Blondel (1987, 483–84) writes on the importance

of state institutions: "It may be true that political activities within the state are more important than other political activities—it may also be true that the state may have the power to regulate the political activities which take place in other organizations: for instance, the state may specify that only certain types of associations are allowed or that associations may engage in certain types of activities. This may result in political activity within the state being dominant; it does not result in the end of political activity in other bodies." We might argue over the use of the term *bodies*, but the problem of the relationships among subsystems seems to me to have been well put in conceptual terms.

In dealing with the problem of specifying the characteristics of the state, authors of a statist orientation have put forward analytical solutions that appear neither empirically convincing nor theoretically coherent. Nevertheless, they are solutions that might be developed in an interesting way. Generally speaking, many of these authors use, in combination, two approaches to the state: the first treating it as a set of administrative agencies, the second as an institutional and legal order structured on precise, and historically determined, organizing principles. These latter are visible principles in the sense that they institutionalize themselves in norms, regulations, procedures, resources, and relations; and public action must therefore be conducted within the *constraints* and *opportunities* defined by the characteristics of that process of institutionalization. In institutionalizing themselves, these principles tend to become relatively permanent through time, even if environmental or political changes within the system may lead to their—albeit slow and partial—transformation. Understood in this sense, state institutions perform not only regulative but also selective functions; not only do they have functions that define interests but

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also functions that distinguish between interests that are legitimate and those that are not. To argue, as Benjamin and Duvall do (1985, 36), that "those organizing principles, in effect, constitute the structure of the state" does not therefore entail a return to an abstract theory of the state—provided that an accurate specification is made of the institutional structures embodying the principles. It is worth reminding ourselves that the usefulness of the approach has been demonstrated by a number of comparative studies of both the development of welfare states (Esping-Andersen and Korpi 1986) and of developing countries (Duvall 1983).

I would maintain, therefore, that Almond has plausible reasons for concluding that statist literature has not formulated a paradigm that is sufficiently coherent or sufficiently alternative to that of mainstream political science (although March and Olsen [1984, 747] have pointed out that "The institutionalism we have considered is neither a theory nor a coherent critique of one. It is simply an argument that the organization of political life makes a difference"). More controversial, however, in the light of the preceding brief discussion, is his criticism, via Krasner, of the whole of statist literature for having taken up a "thoroughgoing determinist position."

State and Political Science

We can draw two basic conclusions in order to discuss the third of the objectives that Almond set himself. The first, statist literature also comprises a variety of points of view, the methodological and conceptual differences among which need examining. Together with approaches that probably presume they are able to transform the state into the analytical premise for a new interpretative model of

politics, we find other approaches that, more soberly, seek to encourage a reconsideration of the importance of political institutions (and of state institutions in particular) within political systems—past as well as contemporary. March and Olsen (1984, 747) write, "It would probably be more accurate to describe recent [statist] thinking as blending elements of an old institutionalism into the non-institutionalist view of recent theories of politics." Second, if, in examination of statist literature, we use a criterion that is able to highlight the distinction between contributions that see the state as an (indeterminate) "concept" and those that see it as a (determinate) "institution," it is possible to lay a different basis for comparison between mainstream and statist literature.

Certainly, comparison on these terms would not be a linear or painless undertaking. After all, the mainstream of political science has its theoretical and methodological roots in a long process of scientific reassessment, one consequence of which has been that the analytic framework has rejected the state understood as a concrete institution. Twenty years ago Fried (1968, 144) summarized the problem as follows: "The revolution in thought, which, at least temporarily, undermined the analysis of the state as a *complex of concrete institutions*, began towards the end of the nineteenth century with the emergence of functionalism. It was in the twentieth century that new ways of looking at the state, stimulated by the parallel development of behavioral and operational approaches, began to show competitive degrees of refinement" (my emphasis). The state was replaced by the political system; and interactions and processes internal, and above all external, to formal structures took over from the old predilection for laws, rules, commands, and order. Thus the student of politics was provided with tools and methods that then invested his or her in-

quiries with scientific integrity. The study of politics thus attained the (recognized) status of a specific social science. It is in nobody's interest to relinquish these achievements.

Nevertheless, it seems legitimate to ask whether political research has not, in the course of its scientific evolution, denied itself fruitful areas of investigation by abstaining from analysis of the state as a complex of concrete institutions. In other words, does a "return to the state" embody a return to the study of the legalist forms of political power—a return to the elaboration of abstract concepts—or does it represent a greater sensitivity among students of politics towards institutional arrangements and their importance for, if not centrality in, the evolution of political systems? Pasquino (1968, 121) summarizes the problem thus: "Contemporary political science, even in its best formulations, has not given enough attention to structural-institutional problems and *their impact on the dynamics of the political system*" (my emphasis). Such attention, however, characterizes the two volumes (Castles and Widenmann 1986 and Katz 1987) on "The Future of Party Government" sponsored by the European Institute of Florence. Almond himself (1987, 246) has recently written in a discussion of functionalism that "from the methodological side it is viewed by some as reductionist, as obscuring the importance of the state and *political institutions*"—as opposed, that is, to the politicoadministrative personnel who operate within them.

There do not seem to be methodological obstacles to a translation of the definition of the state as a "legal-institutional order" into a definition that sees it as a specific subsystem, one that is part (but a particularly important part) of the more complex political system. Many features distinguish this subsystem from others.

Of course, this specification is necessary only if one recognizes that such a

subsystem enjoys a special stature with respect to the other subsystems within the political system; a stature that it derives, *inter alia*, from both the specific features of its internal political structure and from the specific tasks performed by its institutional structure in relation to the overall political system. In its turn, this raises the problem of the specification and the definition of such concepts as "political structure" and "institutional structure" and the relation between them—a problem that does not lend itself to any easy solution. Nevertheless, the mere act of pointing out these distinctions enables us to reconstruct the reasons why the state subsystem *de facto* performs tasks that the other subsystems do not. In this way (and in a non-determinist manner) it is possible to re-introduce into the analytical framework those functions of conditioning and legitimation that the institutions of this subsystem (in its most general activity) perform *vis-à-vis* the processes and actors of the political system.

Some recent remarks by Easton may help to make the point clearer: "[The political system is] those social interactions and *institutions* through which a society makes decisions considered binding by most members of society most of the time" (1987, 478; my emphasis). Now this approach to the political system does not entail an analytical use of institutions in the exclusively behavioral sense. In other words, this approach does not appear to require inhibitory mechanisms for the formulation of a dynamic model able to establish the relations (and empirically verify them) between (individual) choices and (institutional) constraints. If certain inflexibilities in the formulation are relaxed, there seems to be no logical irreconcilability between approaches that see the political process chiefly as interaction among individual actors who make choices and those approaches that relate the choices made by actors to the fulfillment of duties and

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obligations. In reality, actors behave in structured contexts (contexts that do not determine the outcome of an action but influence it), the characteristics of which can be traced back principally to the nature of the political institutions that are, precisely, "most closely related to the making and implementing of political decisions." It seems difficult to deny that in the history of modern societies state institutions have enjoyed—and enjoy even more in today's welfare society—a particularly privileged position in the "making and implementing of political decisions."

It is probable that—more than just a debate between contrasting methodological positions—the controversy over the state will continue to take the much weightier form of an irreconcilable divergence between two opposed theories of democracy—one empirical (Anglo-American) and the other rationalist (Continental European)—a divergence that has recently been summarized by Sartori (1987, 53): "Rational democracies are constructed deductively . . . whereas the construction of empirical democracies largely results from . . . inductive elements." For rationalist theory the state is the "fixed structure," the "depersonalized, impersonal, juridical form" that can give organic shape to the evolution of the democratic regime. For empirical theory the state is an entity behind which there stand "concrete persons" who "govern" the process of trial and error that makes the constant improvement of the democratic regime possible. However, it seems reasonable to claim that today the debate on the state manifests itself in consistently new terms. If this new development is not accepted, then statist and mainstream political scientists will continue to speak different languages, with no shared vocabulary with which to communicate; and also, as Dunleavy (1987, 471) has written, "the recent emphasis upon the theory of the state does not look as if it

will disappear quickly," then the outcome threatens to be an ever-increasing rigidity in the various positions within the discipline.

Almond's essay "The Return to the State" might encourage reflection on the subject that would help to avoid this situation. It clears the ground of controversies that until very recently appeared important and helps to delimit the extent of the divergences among the various positions. If it is followed by further contributions able to shed light on both the present deployment of positions in statist literature and the significance for the discipline as a whole of a revival in institutional analysis (and of course in administrative and institutional history as well)—contributions not couched in exclusively individual terms—then the basis could be laid for a new and interesting phase of theoretical debate—a debate that could lead to the overcoming of several important limitations present in both approaches. I submit that Almond has only partly achieved his objectives (successfully the first, less satisfactorily the other two). Nevertheless, his contribution, if taken up, will certainly come to be regarded in the future as an important step forward in the debate between mainstream and statist political scientists.

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RESEARCH
NOTES

MEASURING POLICY CHANGE IN THE U.S. SUPREME COURT

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Masuring the U.S. Supreme Court's policy changes is complicated by change in the content of the cases that come before the Court. I adapt from earlier scholarship a method to correct for changes in case content and use this method to measure change in the Court's support for civil liberties in the 1946-85 terms. Analysis based on this method indicates that because of changes in case content, the average difficulty of reaching a pro-civil liberties result varied during that period. With corrections for case difficulty, the Warren Court of the 1950s appears to have been more conservative, and the Burger Court more liberal, than patterns of case outcomes themselves suggest. This method, while imperfect, has utility for the measurement of policy change in the Supreme Court and other institutions and thus can serve as a building block in analyses of the processes and determinants of change.

Over the past two decades, as Republican presidents have appointed a series of Supreme Court justices, interest in the Court has focused on policy change. Particular attention has been paid to the Court's growing conservatism in civil liberties. But scholars and other commentators disagree on the extent of the Court's movement to the right.

This disagreement results in part from the difficulty of measuring policy change in institutions such as the Supreme Court. The content of the cases that the Court decides is constantly changing. Thus it is not easy to compare the ideological positions that underlie the Court's decisions in two different periods.

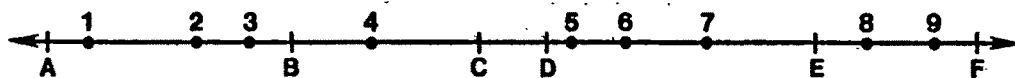
There may be no entirely satisfactory means to overcome this difficulty in a policy area as broad as civil liberties. But even imperfect methods for measurement of policy change can facilitate analysis of the processes and determinants of change. I present one method to measure policy change and use that method to examine changes in the Supreme Court's support for civil liberties in the 1946-1985 terms.

Conceptualizing Policy Change

At the outset, the concept of policy change should be clarified. We might measure change simply in terms of changes in the pattern of outcomes. If the Supreme Court ruled in favor of civil liberties claims 60% of the time in one period and 40% of the time in a later period, we could say that the Court's policies had become more conservative by that 20% difference. But such a calculation fails to take into account changes in the content of civil liberties cases.

The difference between change in outcomes and policy change can be described in terms of positions on an issue dimension, depicted in Figure 1. Assume that cases in an issue area fall along a single continuum for all justices. Each justice has an *i*-point representing the justice's ideal policy, and each potential case outcome has a *j*-point. In each case, the vote of each justice is determined by the relationship between that justice's *i*-point and the *j*-points representing alternative outcomes. If only two alternative outcomes

Figure 1. Representation of Ideal (*i*) Points for Justices and Midpoints between *j* Points for Cases on a Single Dimension



Note. Numbers represent *i*-points on the dimension for each justice, letters represent the midpoints between the (unshown) *j*-points for the two alternative outcomes in each case. If this is a liberal-conservative dimension (e.g., high support for civil liberties vs. low support for civil liberties), each justice will cast a liberal vote in each case in which the midpoint between *j*-points is to the right of the justice's *i*-point. Thus, the vote in case A would be 0-9 against a civil liberties claimant, while the vote in case F would be 9-0 in favor of a claimant.

exist (e.g., affirming or reversing a criminal conviction), a justice will vote for the more liberal alternative if the justice's *i*-point is to the left—the liberal side—of the midpoint between the two alternative policies. The Court's decision will be liberal if a majority of justices have *i*-points to the left of that midpoint.

From this perspective, changes over time in patterns of outcomes could occur in three ways (see Asher and Weisberg 1978; Baum and Weisberg 1980; Glenn 1977).

1. *Personnel change.* The pattern of outcomes changes because some justices are replaced by others who have different *i*-points on the dimension.
2. *Member change.* The pattern of outcomes changes because the *i*-points for some continuing justices shift.
3. *Issue change.* The pattern of outcomes changes because a new set of cases has a different mix of *j*-points from the earlier set. If the *j*-points generally move to the left, it becomes more "difficult" to cast a liberal vote and the proportion of liberal outcomes declines.

Changes in outcomes that result from issue change should not be interpreted as policy change, because the Court would

still reach the same outcomes if it were confronted with the same cases. But outcome changes that result from personnel and member change qualify as policy change, because the Court has shifted rather than the cases. The analytic task is to separate out issue change from these other two sources of change in case outcomes, in order to ascertain the extent of actual policy change.

A Method for Estimating Issue and Policy Change

Within the body of work on legislative and judicial policy change, some scholars have taken into account the effects of issue change. One approach, used primarily in studies of the Supreme Court, is to control for variables related to issue content (e.g., Atkins and Sloope 1986; Ulmer and Thomson 1981). The most comprehensive of these efforts is Segal's study of search-and-seizure cases (1984, 1985), which was highly successful in separating the impact of a variety of case facts that influenced outcomes from the impact of personnel change and member change. For areas as broad as civil liberties, however, it is far more difficult to measure and control for case content in this way. For such areas, another ap-

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proach is needed to estimate issue change.

An alternative approach is to infer the content of issues from votes themselves (Burstein and Freudenburg 1978). Two studies in quite different areas developed similar methods of this type. One was a study of congressional voting change on economic and welfare policy by Clausen and Cheney (1970, 148-51). The other was an effort to compare the skills of batters in baseball who played during different eras (Cramer 1980). The general method used in these two studies is applicable to institutional policy change in broad issue areas such as civil liberties, and the method used here is adapted from those studies.

Three assumptions underlie the estimation method to be used:

1. Every civil liberties case falls along the same single policy dimension for every justice, and every justice places the alternatives in each case at the same j -points on the dimension.
2. Each justice's ideal point on the civil liberties dimension remains constant throughout the justice's career.
3. Justices' votes are based solely on the relationship between their ideal, or i -, points and the j -points for alternatives in the case.

Although these assumptions are common in research on judicial decision making, their imperfections are obvious. A method can be based on these assumptions, however, because their deviations from the reality of Supreme Court behavior appear to be limited and generally unsystematic. For instance, all civil liberties cases do not fall perfectly along the same attitudinal dimension; but dimensions within civil liberties correlate highly, and a single dimension describes the voting pattern in these cases fairly well (Schubert 1963, 1974). Justices undoubtedly differ in their placement of some cases on a policy dimension, but the high scalability of votes in specific issue

areas suggests that these differences are limited (Rohde and Spaeth 1976).¹ Similarly, it appears that some justices' policy views shift over time ("Changing Social Vision" 1983), but past studies (Schubert 1974, 159) and the data used for this study suggest a high degree of stability for most justices.² Thus, a method resting on these assumptions is appropriate if imperfect.

The assumptions rule out the possibility of member change, leaving only personnel and issue change. Thus, changes in case outcomes can result only from issue change and personnel change; changes in individual voting can result only from issue change. Because the issue change component does not represent true policy change, the personnel change component represents the full extent of policy change. Since changes in individual voting result only from issue change, we can obtain a measure of true policy change by subtracting changes in individual voting (issue change) from changes in case outcomes (personnel change plus issue change).

To use this approach, several steps were taken. First, cases from the 1946-85 terms with civil liberties issues were identified and the votes of each participating justice coded.³ Cases were eligible for inclusion if they contained a civil liberties issue and at least one page of opinions was issued. To approximate the unidimensionality more closely, cases were excluded if civil liberties claims were in conflict or (with a few exceptions) if civil liberties claims were made by businesses.⁴

Both unanimous and nonunanimous decisions were included. This decision was appropriate to the purposes of the study, because both kinds of cases define the positions of the Court and of individual justices. An analysis from which unanimous decisions were excluded produced results similar to those of the main analysis that includes those decisions.

Justices' votes were coded as pro- or

Table 1. An Example of the Procedure for Calculation of Corrections

Natural Court	Support Scores								
	Black	Douglas	Clark	Warren	Harlan	Brennan	Stewart	White	Court
9	81.7	93.4	51.7	85.1	40.4	83.1	59.7	61.7	79.1
10	64.0	90.9	49.1	76.8	36.1	74.3	47.8	55.6	65.2
Change	-17.7	-2.5	-2.6	-8.3	-4.3	-8.8	-11.9	-6.1	-13.9

Note: Median change for continuing justices = -7.2; correction = +7.2; policy change for Court = -13.9 - (-7.2) = -6.7.

anti-civil liberties or as mixed. The coding was based on the potential outcome of the case for the party making a civil liberties claim—whether that party would win the case if that justice's vote prevailed. A justice's vote was coded in relation to the range of possible outcomes for the parties, not in relation to the positions of other justices, because our concern is absolute levels of support for civil liberties.

The votes of each justice and the decisions of the Court as a whole were summed for each "natural court" to produce a civil liberties support score, the proportion of individual votes or of decisions favorable to litigants with civil liberties claims. A natural court is a period that begins with one personnel change and ends with another. In three instances, two or three natural courts were combined to create a period in which the Court decided at least 75 civil liberties cases.⁵

To measure issue change, support scores for justices were compared across natural courts. For each adjacent pair of natural courts, a continuing justice's support score for the first natural court was subtracted from the justice's support score for the second court to produce an individual difference score. The median of the difference scores for the seven or eight continuing justices then was calculated. In line with the logic described earlier, that median then was subtracted from the change in the Court's proportion of

pro-civil liberties outcomes to produce a measure of policy change between the two natural courts.

This procedure is illustrated in Table 1. In the example shown, the median of the justices' difference scores between the ninth and tenth natural courts was -7.2%.⁶ Based on the assumptions, this figure indicates that it had become slightly more difficult to support civil liberties because the average *j*-point for cases had moved to the left. This -7.2% figure then can be subtracted from the change in the proportion of pro-civil liberties outcomes between the two periods: -13.9 - (-7.2) = -6.7. This result indicates that the actual policy change was only about one-half as extensive—though in the same direction—as the change in outcomes. The -7.2% figure, with the negative sign made positive, also can be used as a correction for case difficulty in the Court's support score; and the corrections can be cumulated across periods to allow comparisons of the Court's "true" support for civil liberties among all the natural courts in the 1946-85 terms.

Findings

We can begin consideration of the findings by examining the actual pattern of outcomes in civil liberties cases during the 1946-85 terms. The data are shown in Table 2.

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The pattern of outcomes generally coincides with what we would expect. With the departures of the most liberal members of the Vinson Court in 1949, the Court's support for civil liberties dropped sharply. The early Warren Court of the 1950s supported civil liberties claimants more often, and the support level increased again with the 1962 appointments that created the late Warren Court. The successive Republican appointments beginning in 1969 were followed by successive declines in support.

There are, however, some anomalies. In the late Warren Court, a major drop in civil liberties support followed the replacement of Arthur Goldberg by the like-minded Abe Fortas. The Court's collective policy views were probably most favorable to civil liberties after Thurgood Marshall's appointment in 1967, yet this was not the period of the greatest civil liberties support.

The Burger Court's record might be considered the greatest anomaly. The 38% support level from 1975 on ranks well below the level in the early Warren Court and at about the level of the late Vinson Court. Yet even in its last decade, the Burger Court's doctrinal positions were often quite favorable to civil liberties. To equate that Court with the late Vinson Court, which appeared to be much less friendly to civil liberties doctrinally, seems inaccurate.

Table 3 shows the changes in support levels that result from the correction procedure. The corrections for each pair of natural courts, shown in the "correction from previous" column and cumulated in the "cumulative correction" column, are of interest in themselves as measures of issue change. The figures indicate that case difficulty declined considerably in the late Vinson Court and a bit more through the 1957 term. It then increased in several increments, until the cases decided in the 1969-70 terms were nearly 20% more difficult, by the metric used

Table 2. Proportion of Supreme Court Decisions Supporting Civil Liberties Claims, 1946-85 Terms

Natural Court	Terms	Proportion
1	1946-48	49.5
2	1949-52	38.7
3-5	1953-56	57.5
6	1956-57	64.2
7-8	1958-61	57.8
9	1962-64	79.1
10	1965-66	65.2
11	1967-68	75.8
12-13	1969-70	50.3
14	1971-73	48.2
15	1975-80	39.1
16	1981-85	37.2

Note: "Natural Court" refers to the number of the natural court or courts included, counting in sequence from the beginning of the 1946 term. Where a term is included in two natural courts, the dividing line occurred during the term; substantial portions of the 1956 term lie in each natural court, while the 1975 term lies primarily in the fifteenth natural court. Mixed decisions are counted as one-half favorable.

here, than those decided in the early Warren Court. Case difficulty declined in the early 1970s, but most of that decline was canceled out by later increases.

The most important pattern is the considerable growth in case difficulty between the late 1950s and late 1960s. This growth might be expected. As it moved into several civil liberties fields, the Warren Court tended to deal with the easier cases—those with relatively clear civil liberties violations—first. Gradually, the Court took on the more arguable cases, the ones on which even liberal justices might disagree. As a result, in cases decided by the Burger Court, pro-civil liberties decisions were more difficult to reach than in the early Warren Court. The general consistency of the corrections with expectations and their fairly substantial magnitude suggest that they represent real measures of change in case difficulty rather than quasi-random fluctuations.

Table 3. Adjustments in Civil Liberties Support Based on Corrections for Pairs of Natural Courts, 1946-85 Terms

Natural Court	Terms	Support Score	Correction from Previous	Cumulative Correction	Adjusted Support
1	1946-48	49.5	.0	.0	49.5
2	1949-52	38.7	-7.3	-7.3	31.4
3-5	1953-56	57.5	-2.0	-9.3	48.2
6	1956-57	64.2	-.5	-9.8	54.4
7-8	1958-61	57.8	+3.5	-6.3	51.5
9	1962-64	79.1	.0	-6.3	72.8
10	1965-66	65.2	+7.2	+.9	66.1
11	1967-68	75.8	+.4	+1.3	77.1
12-13	1969-70	50.3	+8.3	+9.6	59.9
14	1971-75	48.2	-7.3	+2.3	50.5
15	1975-80	39.1	+4.1	+6.4	45.5
16	1981-85	37.2	+.5	+6.9	44.1

Note: Scores are in percentages. "Correction from Previous" is the adjustment from the previous natural court based on the median difference in civil liberties support by justices who served in both natural courts. "Cumulative Correction" is the total adjustments from the first natural court to the court on that line.

The calculated corrections are applied to the Court's support scores in the final column of Table 3. The corrections did not produce any fundamental changes in policy trends (the correlation between old and new support scores is .88), but some important modifications occurred. The conservatism of the late Vinson Court becomes even more pronounced, and the Warren Court of the 1950s appears somewhat less liberal than it had appeared before. The Court of 1967-68, not the Court of 1962-64, now appears the most liberal.

The Burger Court shows the most striking results from the corrections. The gap between the Burger Court and the late Warren Court narrows a bit. More important, the Burger Court now appears to be almost as civil libertarian as the early Warren Court and the early Vinson Court and far more civil libertarian than the late Vinson Court.

These adjustments seem intuitively reasonable. The sharper distinction between the early and late Warren Courts is more consistent with the Court's personnel changes and its dramatic innovations in civil liberties in the 1960s. The Burger

Court's standing now matches its mixed doctrinal positions on civil liberties issues. The peak in civil liberties support after Marshall's appointment also makes sense. While these results hardly establish the accuracy of the correction procedure used in the study, they show that it produces realistic results.⁷

Conclusions

I have laid out and applied one method with which to control for issue change in comparing the ideological position of the Supreme Court or of other institutions over time. This method is neither perfect nor definitive, because it incorporates assumptions that oversimplify the Court's reality. But it provides one means to compare policy positions over time, and the results from its application to civil liberties policy are intuitively reasonable.

Quite aside from the value that this specific method may have, the utility of taking issue change into account in analyzing policy change should be clear. Some students of Congress and the Supreme Court have worked to measure

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and control for the effects of issue change. We need to continue thinking about means to measure issue content that will be applicable to a variety of situations. In this way, we can ease the difficult task of understanding policy change and ultimately of comprehending individual and institutional policymaking behavior.

Notes

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1. Arguably, at any given time two justices might differ in their perceptions of the difficulty of cases before them because one has experienced the evolution of issues in an area longer than the other. The high scalability of cases with varying difficulty in any given period suggests that such differences would be minor. Further, even relatively new justices are likely to come to the Court with sufficient experience in observing and assessing the issues that the Court addresses to perceive evolution that has occurred.

2. The basic stability of the justices' positions is suggested by the high correlations between individual levels of support for civil liberties in adjoining natural courts for justices who served in both periods. Of the 11 natural court-to-natural court correlations, the median is .96.

3. A full list of the coding rules summarized here is available from the author.

4. Freedom of expression claims by the mass media generally were included.

A second analysis was performed in which cases for each period were subjected to Guttman scaling, and those with two or more errors were excluded. This procedure was used to eliminate cases with anomalous voting patterns that may result from multiple dimensions of attitudes. The results were quite similar to those for the main analysis presented in Table 3.

5. Where the breakpoint between natural courts was ambiguous, it was placed between terms if possible. In the combined natural courts, justices were included only if they served during a natural court that constituted a majority of the combined period.

6. The variation in the scores for individual justices that appears in Table 1 exists for other periods as well. The mean deviation in each period ranged from 2.7% to 9.2% (the second highest was 6.6%). Commonly, a single justice was an outlier,

increasing the mean deviation substantially. To limit the effects of outliers—who may be interpreted as justices departing the most from the assumptions underlying this method—the median of the scores was used rather than the mean. The variation among individual difference scores underlines the imperfections of the assumptions on which this method is based. The moderate size of the mean deviations is reassuring because it suggests that there is a real central tendency in the difference scores, one that reflects the primacy of issue change as a source of changes in individual voting behavior.

In 4 of the 11 periods, only positive individual difference scores or only negative scores appeared. If we interpret the deviations among scores as reflecting forces other than issue change, such periods are no more consistent with the assumptions underlying this method than are other periods. Such periods differed from the others primarily in the clustering of individual scores at high positive or negative levels rather than near zero, and it is reasonable to assume that the magnitude of issue change will differ considerably from period to period.

7. The pattern of results should allay one reasonable concern about the correction method. It could be argued that external forces move the Court in a liberal or conservative direction during particular periods and that the correction method would simply treat such movement as a change in case content and suppress it. Some of the corrections do reduce the size of the calculated policy change, but others increase it. Thus the correction method appears to be quite independent of the direction of change in the pattern of case outcomes.

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DEFENSE BURDEN AND ECONOMIC GROWTH: UNRAVELING THE TAIWANESE ENIGMA

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Taiwan has displayed a comparatively heavy defense burden and rapid economic growth. Time series data from this case are used to analyze three models of the effects of defense burden on economic growth: the modernization model, the capital formation model, and the export-led growth model. The results indicate that all these models capture parts of the empirical reality, but none can account for all the complexity of this reality. Indeed, in some crucial respects each of the models—based, as they have been primarily, on intercountry comparisons—is contradicted by the Taiwanese time series. I conclude that Taiwan has not so much been able to avoid entirely the trade-offs between defense and growth as to relax these trade-offs. The reasons contributing to this relaxation tend to set the Taiwanese experience apart from the experiences of most other countries.

Compared with most developed, as well as developing, countries, Taiwan has, historically, borne a rather heavy defense burden. Yet it has achieved one of the highest and most sustained growth rates in gross national product (GNP) (at an average annual rate of 8.8% during 1952–85) and in exports (at an average annual rate of 24.0% during 1953–85) in the world. Its savings rate, its inflation rate, and its unemployment rate again rank it among the top performers globally. These accomplishments make Taiwan a rather exceptional case in regard to the generally supposed negative impact of defense burden on economic performances.

I first present three alternative perspectives on the economic effects of a heavy defense burden. The results of the time series analyses on Taiwan are discussed next. The conclusion tries to put Taiwan's

experience in a broader cross-national context.

Alternative Expectations

Ever since Russett's (1970) pioneering work, there has been a burgeoning literature on the growth effects of defense spending. In the interest of brevity, I will not attempt a comprehensive review of this literature. This review has been undertaken elsewhere (Chan 1985). Instead, I will extract three strands of arguments (perspectives, if you will) from this literature and seek to determine their respective correspondence with the Taiwanese experience.

The first perspective features the military as a modernizing agent in developing countries. According to Benoit (1973), the negative consequences

of defense spending on economic growth can be neutralized, and even reversed, by three processes. He contends that the military instills in its members modern skills and attitudes; that capital investment by the military improves a country's basic infrastructure in communication and transportation; and that government spending in general creates mild inflationary pressure, which in turn encourages fuller utilization of existing production capacity.

The second perspective highlights the dampening effects of military expenditures on capital formation, which is in turn considered a key determinant of long-term economic growth (Deger and Smith 1983). A larger percentage of GNP allocated to the defense sector is apt to be financed through a heavier tax burden or bigger government budget deficits or both. Both tend to dampen investment, with taxation playing a more direct role (people have less income to save and invest) and budgetary deficits playing a less direct role (deficit spending abets inflation, which in turn encourages consumption and discourages saving). In the absence of offsetting transfers of foreign resources into a country, the consequent decline in capital formation slows economic growth.

The third perspective stresses the adverse impact of defense expenditures on a country's export competitiveness (Rothschild 1973). It argues that such spending is particularly likely to divert resources (human and capital) from the most dynamic industrial sectors of the economy. These sectors tend to be export-oriented, and export competitiveness is a major determinant of national economic growth. By hampering the innovation and production efficiency of civilian industries that are necessary for successful export drives, a heavy defense burden is seen to bring about deteriorating trade balance, weakening currency, and structural unemployment.

Analysis Results

Although the pertinent times series for Taiwan extend in some cases to the early 1950s, I restrict my analyses to the 1961-85 period. During the years before 1961 (especially the early 1950s), the Kuomintang was struggling quite literally for survival in its last territorial remnant. By 1961, however, political stability and economic recovery had been achieved, so that the subsequent years represent a period of "normalcy."

I rely on the Arms Control and Disarmament Agency's annual *World Military Expenditures and Arms Transfers* for Taiwan's defense figures. All my other data are based on Council for Economic Planning and Development 1986. Generalized least square regression with the Yule-Walker estimation procedure is used for adjusting for autocorrelation in the data with a lag of one year. This procedure takes into account the effects of secular trends, and is appropriate and efficient when the number of observations and the autoregressive parameters are small. The generalized least square results are reported in Table 1.

I discuss first the evidence pertaining to the various causal paths suggested by the modernization model. Equation 1 shows that military spending as a percentage of GNP (MEGNP) has been negatively, rather than positively, associated with the development of Taiwan's industrial infrastructure (TRANSCOM, an index of transportation and communication facilities). This association casts doubts on the contention that capital investment by the military promotes infrastructure development.

Equation 2, however, is more supportive of the modernization model. It indicates that MEGNP has tended to stimulate fuller capacity utilization, which is indexed by the rate of energy production (ENERGYOP, the rate at which energy-generating plants operated as a percentage

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Table 1. Generalized Least Square Model Estimates, 1961-85

Equation	Dependent Variable	Independent Variable	B Coefficient	T significance	R square (%)
Modernization model					
1	TRANSCOM	MEGNP	-173.76	.0242	21.9
2	ENERGYOP	MEGNP	1.2708	.0894	13.1
3	LITERACY	ARMPFORCE	-.5997	.0001	80.6
4	BIRTHRATE	ARMPFORCE	.0582	.0001	74.6
5	GNPRATE	(FORGNRES - MEGNP)	-.1970	.0001	
		TRANSCOM	-.0024	.0823	
		ENERGYOP	.4936	.0002	
		LITERACY	.4753	.0364	
		BIRTHRATE	9.3072	.0006	
		MEGNP	-.7215	.0134	
		ARMPFORCE	-.3140	.0633	91.8
Capital formation model					
6	TAXBURD	MEGNP	-.3441	.0777	14.1
7	BUDBAL	MEGNP	-3,205.5	.0084	28.7
8	PRICE	BUDBAL	.0007	.0024	34.9
9	SAVEGNP	TAXBURD	2.4716	.0002	
		PRICE	-.0088	.8916	50.0
10	CAPFORM	SAVEGNP	.4060	.0339	18.9
11	SAVEGNP	MEGNP	-1.1998	.0945	12.8
12	CAPFORM	MEGNP	-1.6495	.0161	24.6
13	GNPRATE	(FORGNRES + CAPFORM)	-.0023	.1089	11.3
14	GNPRATE	TAXBURD	-.4880	.3442	
		BUDBAL	.0001	.1597	
		PRICE	-.1581	.0701	
		(FORGNRES + CAPFORM)	-.0031	.0819	
		MEGNP	.3321	.5527	49.3
Export-led growth model					
15	EXPRATE	MEGNP	2.1679	.2752	5.6
16	INDEXP	MEGNP	-1.5159	.3166	4.8
17	TRADEBAL	EXPRATE	-449.81	.5958	1.3
18	CURRENCY	TRADEBAL	.000002	.5490	1.7
19	GNPRATE	EXPRATE	.1131	.0125	
		INDEXP	.0045	.9224	
		MEGNP	.2628	.6271	32.1
20	UNEMPLOY	EXPRATE	-.0040	.2735	
		INDEXP	-.0271	.0004	
		GNPRATE	-.0008	.9675	49.2

of their installed capacity). The time series for ENERGYOP has fluctuated within a range between 64.3% (in 1967) and 42.5% (in 1985), indicating the presence of substantial industrial slack or underutilized capacity—a situation that perhaps distinguishes Taiwan from other developing countries. It is this slack that enables MEGNP to stimulate faster GNP growth. In its absence, MEGNP would only overheat the economy without expanding output.

The predicted effects of a large military establishment on the spread of modern skills and attitudes are refuted in Equations 3 and 4. A shrinking ARMFORCE (the number of military personnel per one thousand people) has actually been accompanied by rising literacy and falling birth rate. Both of these overtime relationships are highly significant at the .0001 level.

The multiple regression presented by Equation 5 controls for FORGNRES, or the net transfer of resources (e.g., aid, loan, investment) to or from Taiwan as a percentage of its current GNP. It is hypothesized that the difference between FORGNRES and MEGNP will be positively correlated with GNPRATE. If the former exceeds the latter, growth is stimulated. If the latter exceeds the former, growth is retarded. Incidentally, until 1962 Taiwan received between one-third and one-half of its domestic capital formation from foreign (mainly U.S.) sources. However, more recently it has become a net capital exporter.

In Equation 5, the international flow of resources (FORGNRES), after adjusting for MEGNP, features a significant negative overtime relationship with GNPRATE, indicating that Taiwan's fastest economic growth occurred after the cessation of massive U.S. aid and while it was "graduating" to the status of a capital exporter. Also, both MEGNP (T significance = .01) and ARMFORCE (T significance = .06) have been negatively

associated with the rate of GNP growth. Therefore, the causal path linking MEGNP to GNPRATE through the mediating influence of ENERGYOP is the only proposition advanced by the modernization model that receives empirical support. Whatever positive effect defense burden has had on GNP growth indirectly through fuller capacity utilization seems to have been more than offset by its other direct or indirect negative effects.

It may appear enigmatic that TRANSCOM features a negative overtime relationship and BIRTHRATE a positive one with GNPRATE. Taiwan's economy grew faster during the earlier part of the time series when its industrial infrastructure was less developed and its birthrate was higher than now. Reflecting a process of industrial maturation, the very fast rates of growth of the earlier years have shown a tendency to level off somewhat during the 1980s. This deceleration occurred in the context of continued infrastructural development and mass adoption of modern attitudes (for which BIRTHRATE is used as a surrogate measure). Regarding the impact of the latter two variables on economic growth, there is therefore a tendency for diminishing returns to set in. It is important to reckon with such longitudinal patterns that may be missed and may even be treated as counterintuitive in cross-national analyses.

How well does the capital formation model fare? MEGNP features an unexpected negative association with tax burden (TAXBURD, Equation 6), although it has had the predicted effect on budget balance (BUDBAL, Equation 7). The overtime relationship between budget balance and inflation (PRICE) shows a positive sign (Equation 8) and thus is again contrary to the expectation of the capital formation model. Inflationary pressure in Taiwan appears instead to have been driven primarily by external economic forces (e.g., global recessions,

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energy crises). In Equation 9, the hypothesized negative impact of TAXBURD on the savings rate (SAVEGNP) is again contradicted, and significantly so, by a positive B coefficient, whereas that of PRICE turns out to be insignificant.

But could MEGNP have affected savings and investment directly rather than indirectly through the mediation of TAXBURD, BUDBAL, and PRICE? Equations 11 and 12 indeed show evidence of a direct trade-off; higher MEGNP has been associated with lower SAVEGNP (T significance = .09) and CAPFORM (domestic capital formation as a percentage of GDP, T significance = .02). Do CAPFORM and FORGNRES jointly contribute to faster GNPRATE? This expectation, unfortunately for the capital formation model, is not borne out by the data. Equation 13 shows that their overtime relationship carries a negative sign.

Incorporating the aforementioned variables in a single regression produces Equation 14. This equation shows that CAPFORM, after adjusting for FORGNRES, bears a slight negative relationship with GNPRATE at a moderately significant level. MEGNP does not appear to influence economic growth directly, whereas PRICE does tend to affect it adversely. To the extent that Taiwan's inflation has been primarily "imported" from abroad, PRICE serves as a proxy measure of the state of the world economy to which this trading nation is highly sensitive.

In summary, MEGNP does appear to have a direct dampening effect on savings and investment, as suggested by the capital formation model. However, bivariate analyses fail to show any significant effects of either CAPFORM or FORGNRES on the rate of economic growth. In the multivariate analysis, the combined effect of CAPFORM and FORGNRES on GNPRATE turns out to be actually negative. Therefore, only one part of the capital formation model, ad-

ressing the adverse impact of military spending on investment, receives support from the Taiwanese case. The other part of this model, addressing the linkage between capital formation and economic growth, is not borne out. This is perhaps largely due to Taiwan's recent possession of rather large surplus capital. It has the highest savings rate and the largest foreign reserve (on a per capita basis) in the world.

Contrary to the export-led growth model, Equations 15 and 16 indicate that MEGNP does not provide a good predictor of Taiwan's rate of export expansion (EXPRATE) or of the industrial component of its exports (INDEXP). The evidence is also not strong for the hypothesized linkage between EXPRATE and TRADEBAL (trade balance controlled for GNP) or that between TRADEBAL and CURRENCY (the number of Taiwanese dollars necessary to exchange for one U.S. dollar) (Equations 17 and 18). The latter finding is perhaps not surprising in view of the previously tight governmental regulation of foreign exchanges.

The other part of the export-led growth model—addressing the causal link between export performance on the one hand and economic growth and unemployment on the other hand—does receive some support from the Taiwanese time series. In Equation 19, EXPRATE (although not INDEXP or MEGNP) is a highly significant determinant of GNPRATE. At the same time, INDEXP (but not EXPRATE or GNPRATE) shows up as a key factor influencing the unemployment rate (Equation 20). Consequently, as in the case of the capital formation model, only one part of the causal path suggested by the export-led model is substantiated by the data. Taiwan's economy has been very sensitive to export performance, but military burden has not appeared to be a major determinant of export performance.

Conclusion

Let us review briefly the highlights of the longitudinal evidence presented earlier. Regarding the modernization model, military expenditure has not promoted infrastructural development, although it has apparently stimulated fuller capacity utilization. Contrary to its hypothesis, a large armed force has not contributed to the spread of modern skills and attitudes—or at least not in the way that these concepts have been operationalized in this analysis. The declining size of the Taiwanese armed forces has been accompanied by rising literacy and falling birth rate, thus hardly supporting the view of the military as a key modernization agent.

As suggested by the capital formation model, military spending has curtailed savings and investment rates. However, the latter variables are not strong predictors of the overtime changes in Taiwan's GNP growth. On the other hand, both Taiwan's GNP growth and its unemployment level seem to have been strongly influenced by its export performance. The export-led growth model is deficient, however, because military spending has not had the hypothesized adverse impact on export competitiveness.

Some of the data relationships discovered are in fact less puzzling than they may first appear. Inter-country patterns often disappear and are even reversed when the same variables are examined longitudinally for each country. There may very well be a threshold in the overtime relationships between economic growth on the one hand and processes such as investment in human capital, development of basic infrastructure, accumulation of capital, and promotion of industrial exports on the other hand. Beyond this threshold, the marginal contribution of each additional increment of the latter variables to economic growth may well diminish, thus accounting for the negative and statistically significant

coefficients featured by TRANSCOM and (FORGNRES + CAPFORM) in explaining GNP RATE.

Furthermore, for some of the variables examined, there are ceiling effects for either statistical or substantive reasons. With its industrial exports already constituting 93.8% of all exports, there remains less room for Taiwan to improve this percentage further. As another example, with a savings rate that never fell below 25% of gross domestic product since 1970—and was as high as 34.9% at one time—domestic investment opportunities begin to be exhausted and investment attention tends to be shifted to foreign targets. And of course it is far more difficult to duplicate the feat of a 13.9% annual increase in real GNP or a 53.8% annual increase in exports during the more recent years than during the earlier years when the base figures for both were smaller.

Is there any evidence of defense-versus-growth trade-offs? The Taiwanese experience shows some statistical evidence of such trade-offs. As mentioned earlier, military expenditure tends to dampen savings, investment, and infrastructural development. It also tends to stimulate the economy, thereby possibly crowding out civilian production and fueling inflation in a country without industrial slack.

How can we explain these statistical patterns? It seems to me that Taiwan's accomplishments in GNP growth, export expansion, rising physical welfare, and improving income equality are products of a general socioeconomic process in which defense burden plays only a minor and indirect role. Fluctuations in the level of military expenditures and manpower happen to be in most cases the concomitants rather than the determinants of this process.

A search for possible determinants of Taiwan's GNP performance reveals price stability and export rate to be especially important factors. These variables imply

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that foreign economic environment plays a critical role in affecting the health of Taiwan's economy. This economy has been overwhelmingly externally oriented, with foreign trade amounting to about 85% of its GNP in 1985. Thus the export-led model is indeed correct in assigning paramount importance to trade competitiveness as an explanation of Taiwan's economic performance. However, defense burden seems not to have been a major factor in determining Taiwan's trade competitiveness. Instead, the latter has more to do with improving human capital, product upgrading, commercial adaptability, negotiation skills (e.g., Chan 1987; Yoffie 1983), and a booming world economy during the 1960s.

But even this account is not entirely satisfactory because it begs the question of why the Taiwanese have been able to attain these achievements and to relax—if not entirely avoid—the defense-versus-growth trade-offs. Other countries, with seemingly better domestic endowments and a more conducive foreign environment, have fared less well, as indicated by Taiwan's status as a frequent deviant "overperformer" in cross-national patterns. In order to unravel this puzzle, it seems that one must go beyond the narrow confines of the statistical evidence provided earlier and probe more deeply into the historical context of Taiwan's political economy.

Various historical legacies and timing assume major importance. Japanese colonialism provided Taiwan with a comparatively developed human and industrial infrastructure (e.g., Cumings 1984). It and the Chinese civil war paved the way for a strong state on the island (Amsden 1985; Barrett and Whyte 1982; Chan n.d.; Clark 1987). The massive U.S. aid (coming on the heels of the Korean War, McCarthyism, and Washington's "containment" policy) as well as a booming world economy during the 1960s (when the field of the newly industrializ-

ing countries was not so crowded) arrived at very opportune times as the Kuomintang tried first to institute political control and economic recovery and then to launch its export drives. In addition to the U.S. aid (Jacoby 1966), a substantial surplus industrial capacity and a large pool of relatively trained labor helped to lessen the adverse impact of defense burden on the economy. And of course the regime has so far eschewed huge capital outlays to finance indigenous armament industries in any way comparable to the Israelis or even the South Koreans. It has instead chosen to emphasize Taiwan's industrial comparative advantages in indigenous armament production selectively, most notably in the shipbuilding area. These considerations should not be dismissed as ad hoc explanations, but must be included in any valid attempt to unravel the Taiwanese "exception."

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CONTROVERSIES

CONTENDING MODELS OF CABINET STABILITY

A spirited debate has arisen over the best approach to the analysis of the durability of governing coalitions in parliamentary democracies. In this controversy, Kaare Strom enters a number of criticisms of the stochastic modeling approach offered by Eric C. Browne, John P. Frendreis, and Dennis W. Gleiber, and by Claudio Cioffi-Revilla. In turn, Browne and his colleagues join the issue.

Since Riker's (1962) seminal work, game-theoretic (specifically coalition-theoretic) approaches have dominated the study of government coalitions in parliamentary democracies. Riker's *size principle* and competing *policy distance* theories (Axelrod 1970; Swaan 1973) have engendered cross-national studies of cabinet formation, intracoalitional payoff distributions, and government stability (durability). Predicted coalitional properties such as minimum winning status and ideological connectedness figure prominently as explanations of durability (Axelrod 1970; Dodd 1974, 1976; Laver 1974; Mitra 1980). Government stability has also interested institutionalists, whose explanatory models include such variables as party system fragmentation, the number of parties in government and opposition, and ideological conflict levels (Brass 1977; Powell 1982; Sanders and Herman 1977; Strom 1985; Taylor and Herman 1971; Warwick 1979). Frequently, game-theoretic propositions have also been incorporated in such regression models.

In recent years, this game-theoretic and institutionalist literature has come under criticism. Government durability, it is argued, can more fruitfully be understood through stochastic models (Browne, Frendreis, and Gleiber 1984, 1986a, 1986b; Cioffi-Revilla 1984; Midlarsky 1984;

Robertson 1983a). Cabinet dissolutions result from generally unpredictable "critical events" that arise during a government's "lifetime." Accordingly, cabinet duration is best explained by models derived from survival or reliability analysis, such as the Poisson model. In this paper I shall critically evaluate this new approach, focusing on three prominent works (Browne, Frendreis, and Gleiber 1984, 1986b and Cioffi-Revilla 1984). Although innovative and methodologically fairly sophisticated, these analyses are misconceived, poorly executed, and ultimately unlikely to advance our understanding of government coalitions. Specifically, the stochastic modelers have (1) misrepresented the game-theoretic tradition and its empirical success, (2) paid insufficient attention to key terms and assumptions in their own models, (3) overinterpreted empirical support for these models, (4) misplaced the focus of the analysis through substantive unfamiliarity or faulty metaphor, and (5) provided a poor alternative to the game-theoretic approach in the development of a cumulative and rigorous science of politics. After addressing these propositions in order, I offer some suggestions for further research on government stability. The critical events perspective may yet prove its value, but only within a much stronger micropolitical theory.

The Shortcomings of Coalition Theory

The stochastic modelers have leveled three main criticisms against coalition-theoretic explanations of cabinet duration, namely, that they (1) incorporate unrealistic premises and are therefore unpromising (Browne, Frendreis, and Gleiber 1984, 173); (2) are deterministic (Browne, Frendreis, and Gleiber 1984, 1986b; Cioffi-Revilla 1984); and (3) have been empirically unsuccessful (Browne, Frendreis, and Gleiber 1986b, 630-31). On this basis, the authors argue for a "very different research program" (Browne, Frendreis, and Gleiber 1986b, 634). However, these criticisms are not particularly telling. Even if they were, they would not justify abandonment of the game-theoretic approach.

The first criticism is the most interesting. Browne, Frendreis, and Gleiber accuse coalition theory of simplistic assumptions about party preferences, such as positing a stable payoff function for an entire legislative term (1984, 173). Similarly inappropriate is the assumption that successive government formations are mutually independent events (p. 174). While these criticisms are valid and reasonable as far as they go, they are neither novel nor fatal to coalition theory. Coalition theorists have in fact relaxed such assumptions as the mutual independence of cabinet formations (Bueno de Mesquita 1975; Laver 1974). And while most empirical models have been static in the sense of predicting government formation and maintenance from a conception of fixed equilibria, such stipulations need not be made (see Bueno de Mesquita and Niemi 1984; Laver and Underhill 1982). Thus, the critics have demonstrated that coalition theory is frequently simplistic, but not that it is incorrigible. Similar criticisms have in fact been made within the game-theoretic tradition (Laver 1986; Winden 1984).

The charge of determinism is much less warranted. Regression models employed in empirical coalition-theoretic studies are deterministic in the sense of yielding point predictions of cabinet duration, but capture stochastic elements of the process through error terms. However, game-theoretic durability studies are not deterministic in the strict *theoretical* sense of specifying the sufficient conditions for government dissolution. While government *formation* studies are typically deterministic in predicting specific (and occasionally unique) solutions, this is not true of similar studies of cabinet *durability*. There is no deductive theoretical argument that cabinet duration is precisely determined by the structure of the formation game. Rather, the argument tends to be cast in probabilistic terms (e.g., Dodd 1976). Cioffi-Revilla misrepresents the competition when he presents as the only alternative to his analysis a deterministic model in which cabinet duration is "entirely determined and exactly predictable" from the set of independent variables (1984, 334). Obviously, this is a hopeless task on which no sensible coalition theorist would embark. However, theoretically interesting explanations need not meet such heroic requirements.

The third and final criticism of the extant literature is directed against its empirical success. Cioffi-Revilla's critique on this point is unsatisfactory, since he presents such a caricature of the "deterministic" approach.¹ Browne, Frendreis, and Gleiber more soberly contend that existing models of cabinet durability generally explain only 20-30% of the variance. This, we are told, is disappointing, given more than 10 years of empirical research. Moreover, traditional studies have allegedly reached an upper limit of predictability, partly because of the misguided assumption that each cabinet embodies a certain predetermined durability (1986a, 94). This limitation is allegedly also due to the specification of the regres-

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sion models, in which the number of independent variables approaches the number of cases (1986b, 630).

This critique can be countered in several ways. First, these critics place excessive emphasis on the coefficient of determination (R^2), which may be affected by a variety of factors of little theoretical interest (see King 1986). Second, several empirical studies have in fact exceeded the 20–30% barrier in predictive accuracy. Powell (1982, 149) reports that two variables alone explain 50% of the variance in cabinet durability (see also Strom 1985; Warwick 1979).² The more powerful of these two predictors is minimum winning status. So the bottle that is half empty to Browne, Frendreis, and Gleiber, may be half full to many other social scientists, who would happily accept such explanatory power. Indeed, this is precisely the assessment made by the otherwise critical Laver (1986, 32–33). Finally, it is premature to conclude that an upper limit of explanatory power has been reached, particularly since the number of cases in the typical study runs in the hundreds and the number of independent variables is fewer than five. Thus, a degrees-of-freedom problem is not evident.³

Events and Cabinets in the Stochastic Models

Let us now consider the stochastic models presented by these critics of the game-theoretic tradition. Their main flaw is a failure to illuminate the political decisions that cause a change of government. The explication of how and why critical events effect changes in political alignments is inadequate. Simply linking government dissolutions to extraneous events does not *explain* the former any more than precipitation explains the investment decisions of farmers. While there may be an empirical (and even

causal) relationship, explanation requires a linkage to our understanding of human behavior.

Cioffi-Revilla does not explicate the micropolitical connections between critical events and government dissolutions. After citing a few examples, he simply asserts that a multitude of causes can cause the loss of pivotal votes in parliament (1984, 321). He also claims, unobjectionably, that political shocks and crises "probably come from the characteristics of the Italian political system as a whole" (p. 334). Nowhere is there a micropolitical interpretation, much less a theory, of the relationship between critical events and government resignations. While Browne, Frendreis, and Gleiber recognize its importance, their micropolitical argument is neither consistent nor persuasive. In a recent article, they portray cabinet stability as resulting from decision processes (1986b, 637). A critical event represents uncertainty in a government's environment, a "demand" to which it must "respond." But this conception is contradicted in their earlier and more extensive discussion, where events sometimes lead directly to cabinet dissolution, affecting neither decision-making structure nor actor preferences (1984, 184). The authors have in mind events such as constitutionally mandated elections or the death of a prime minister. But such events render their entire conception of government resignations unilluminating. Dissolution is described as "the response to a demand that actors will not or cannot satisfy" (1984, 181). If a dissolution were in fact caused by the premier's death, then this description would be meaningless.

Browne, Frendreis, and Gleiber suffer from attempts to generalize across very heterogeneous government dissolutions. Some of these can fruitfully be analyzed in terms of decisions, demands, and satisfactions; others cannot. The authors face this predicament because they neither

develop their micropolitical argument well enough nor define their terms adequately. Curiously, there is no serious attempt anywhere in this literature to define either *critical event* or *cabinet dissolution*. These are, after all, the key terms in the stochastic models. Cioffi-Revilla is content to equate critical events with random political shocks and crises, "having numerous manifestations, such as scandals, parliamentary defeats, pivotal defections, and other occurrences" (1984, 334). Browne, Frendreis, and Gleiber admit to having "at present little systematic knowledge of such events" (1984, 185). Like Cioffi-Revilla, they are confined to unsystematic exemplification (1986b, 633). Note also that critical events are not empirically observed but simply inferred from the model. The basic causal factor in the stochastic models, then, is *undefined* and *unobserved*.

Browne, Frendreis, and Gleiber (1986b) test their Poisson model in separate analyses of 12 parliamentary democracies between 1945 and 1980. Their hypotheses are supported for four countries, but must be rejected for the remaining eight. This result becomes even less impressive when one considers the low degrees of freedom, which dilute the power of their chi-square test. Furthermore, the authors are certainly wrong in presenting their empirical model as a test of alternative hypotheses (p. 641). First, the fit of the Poisson model is never compared to that of any other. Second, it is theoretically possible to specify an infinite number of other models (some presumably substantively meaningful) with equal or better fits. The proof of the pudding clearly cannot rest here.

A further problem is that these Poisson models are based on the unrealistic assumptions of *homogeneity* and the absence of *contagion* (see Davis, Duncan, and Siverson 1978). Let us examine these problems with reference to Cioffi-Revilla's analysis. The author interprets his results as evidence of a common

Poisson process operating on the durability of all Italian governments. However, the aggregate results could just as well mask a pattern of very heterogeneous durabilities across cabinets. Heterogeneities may occur over types of cabinets or over time. Thus the most short-lived governments may also be *monocolori* rather than majority governments, ideologically "open" rather than connected, or those formed during the crisis of a particular coalition formula (e.g., the Center-Left). Indeed, there appears to be empirical support for each of these possibilities (see, e.g., Marradi 1982). Also, durability may vary cyclically, with governments formed very late in a parliamentary term less durable than those formed just after elections (see Sanders and Herman 1977). A Poisson model may or may not be applicable for each subclass of governments, but the aggregate model will tell us little in case of significant heterogeneity. Moreover, *contagion* would be present if the duration of one government depended on that of its predecessor. Again, this possibility should hardly be overlooked. Yet Cioffi-Revilla provides no control for any of these potential problems.⁴

Governments Misconceived

The definition of a government is neither obvious nor inconsequential (see, e.g., Dodd 1976, 121-23; Harmel and Robertson 1986; Hurwitz 1971; Lijphart 1984; Sanders and Herman 1977). After careful review of five definitions, Lijphart (1984) concludes in favor of using changes in party composition as the sole criterion of a cabinet change. Dodd (1976) reaches the same conclusion. Other scholars advance other criteria (Robertson 1983a, 1983b; Sanders and Herman 1977). Cioffi-Revilla and Browne, Frendreis, and Gleiber neither specify nor justify theirs. One may excuse Cioffi-Revilla's lack of

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explicit criteria since he does not face the problem of cross-national equivalence. Browne, Frendreis, and Gleiber's inattention is more glaring, especially as they actually cite (1986b, 646) Lijphart's instructive article.

Operationally, Cioffi-Revilla seems to rely on political conventions in counting Italian governments; whereas Browne, Frendreis, and Gleiber appear to count a new government with every change in prime minister, party composition, or electoral period. These definitions seem to yield identical results, with more government resignations and far lower cabinet durabilities than the Dodd-Lijphart criteria. In practice, the operational criteria adopted by the stochastic modelers skew their results by introducing substantial amounts of "noise." Recall that a change of government is recorded when the prime minister dies, or when a snap election returns the incumbents to power. This operational definition should in itself favor models of dissolutions as caused by random events and disfavor explanations based on rational party behavior. In fact Lijphart (1984) demonstrates that cabinet durability is significantly more strongly related to such factors as minimum winning status, the number of parties in government, and party system fractionalization under his definition than under the alternative chosen by the stochastic modelers.

Political or academic conventions specifying when a government begins or ends are arbitrary, and operational definitions must be justified by the research problem at hand. The failure of the stochastic modelers to define their dependent variable accordingly has serious ramifications for the underlying theoretical purpose. Government transitions are portrayed as objectively defined phenomena of intrinsic interest to political scientists. But this is not necessarily so. The death of a prime minister need not interest political scientists, unless this event affects the

operations or output of political institutions. Both Cioffi-Revilla (1984, 318-19) and Browne, Frendreis, and Gleiber (1984, 169) stress the significance of cabinet durability for political stability. But if systemic stability is the fundamental concern, it appears incongruous to treat, for example, resignations due to the ill health of a prime minister as equivalent to the downfall in 1960 of Ferdinando Tambroni's Italian government in the face of impending civil war. Transitions that are purely technical or involve only minor personnel changes have very different implications for stability from those that represent changes in the set of governing parties (see Dodd 1976, 122; Strom 1985). The same distinction is central if we seek to understand the strategic behavior of political parties. The former is an explicit concern of the stochastic modelers; the latter is common to much of the literature in this field.

The stochastic modelers thus define their cases inappropriately for their theoretical concerns. They also occasionally infuse parliamentary governments with too much life. Indeed, biological metaphors are pervasive. Browne, Frendreis, and Gleiber describe governments as existing in a world that occasionally produces "life-threatening events" (1984, 182), and attributes of the coalitional payoff structure are likened to "protective armor" (1984, 178). In the words of Cioffi-Revilla, "Italian cabinet governments do not age, and they do not suffer political wear; so long as they are in power they are as good as new" (1984, 336). These descriptions suggest that governments can be understood as living organisms or mechanical objects, which, of course, they are not. The metaphors are understandable, since social scientists have largely borrowed stochastic models from biology and engineering. However, uncritical application of such models is not without its hazards. The distinctive requirement of social science is to under-

stand human intention (Elster 1983), which calls for theoretical care in the adaptation of models borrowed from disciplines where intentional explanation is less appropriate.

Scientific Progress?

The fundamental question concerning the stochastic models of government dissolution is to what degree they contribute to a political science better capable of explaining cabinet stability and related phenomena. Progress in political science depends on the development of research programs in the discipline (Ball 1987; Lakatos 1970). In the Lakatosian view, research programs consist of a "hard core" of fundamental propositions or axioms and a "protective belt" of auxiliary hypotheses (see also Laudan 1977). Criticism within a research program should be directed against its protective belt, and scientific progress results if modifications in the protective belt lead to greater corroborated empirical content (Lakatos 1970, 118). Falsification is no simple matter. According to Lakatos, "there is no falsification before the emergence of a better theory" (Lakatos 1970, 119). Since legitimate scientific criticism is directed only against auxiliary hypotheses, it is and should be very difficult to reject a promising research program.

The game-theoretic study of government durability is part of one of the most ambitious research programs in the social sciences. Although this research program has been most successfully developed in economics, it has increasingly stimulated scholarship throughout political science and particularly in the study of voters, parties, and their coalitional behavior (Riker 1983). The stochastic modelers essentially abandon this research program in their study of government stability. Cioffi-Revilla makes no attempt to build on the game-theoretic tradition. To their

credit, Browne, Frendreis, and Gleiber do present such an effort, but their discussion is scattered and theoretically underdeveloped. It also contrasts with their eagerness to depict their work as a radical departure from the game-theoretic tradition (e.g., 1986b, 634) and their general indictment of the existing literature.

Abandonment of the rational choice tradition in studies of government durability should be exceedingly well justified. The stochastic modelers clearly cannot justify such a radical departure. Their critique of the existing literature is directed primarily against auxiliary hypotheses and methods. While this is entirely legitimate, it does not add up to a case against the entire research tradition, unless (1) the latter is incapable of incorporating progressive modifications and (2) a superior alternative exists.

Clearly, a superior alternative has *not* been presented. The stochastic models reviewed are both ill-conceived and unilluminating. There is no convincing micropolitical connection between critical events and cabinet dissolutions. No theoretical definition of the dependent variable is provided, and the operationalization is ill suited to the purported theoretical concerns. The independent variable is neither defined nor observed. The empirical models are built on dubious assumptions and yield inconsistent results. Finally, the *predictive* power of the stochastic models is poor. For example, Cioffi-Revilla's model gives us no basis for predicting differences in durability between any two Italian cabinets.

Also, there is promise of a multitude of progressive modifications in the protective belt of the game-theoretic tradition. Game-theoretic analyses of government durability have tended to rely on exceedingly simple assumption about actors, payoff functions, and institutional environments. Despite these restrictions and the limitations of the dominant solution concept (Hardin 1976; Ordeshook 1986,

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408-14), empirical tests have actually produced promising results. There is good reason to try to improve these results by relaxing auxiliary assumptions. Most importantly, game-theoretic models of cabinet durability need to become less static. Dynamic elements can most easily be incorporated by allowing utility functions to vary over time. For example, payoffs to political parties may perfectly well change over a parliamentary term. As elections draw close, the value of short-term office holding declines, and electoral considerations become paramount. Also, different parties pursue different kinds and mixes of benefits. Some parties are more office-motivated, others more policy-motivated, and variation may occur within as well as between countries (Budge and Laver 1986). Hence, the constant-sum assumption, on which the size principle is based, is not always applicable. Coalition theory must also incorporate temporal or hierarchical interdependence between governments. Such interdependencies can be formulated as supergames or nested games (see Tsebelis 1987). It is unduly restrictive to view each government formation or dissolution as independent from preceding or successive ones (see Bueno de Mesquita 1975; Franklin and Mackie 1983). Coalition studies may also be based on noncooperative rather than cooperative game theory. New and richer models require greater institutional sensitivity than has often been exhibited by coalition theorists.

In modifying coalition theory, there is something to be learned from the critical events perspective. Coalition theorists have generally paid little attention to the circumstances under which governments resign. However, it does make sense to think of events as keys to cabinet resignations. After all, if nothing happened, there would be no reason to dissolve the government.⁵ But an adequate critical events theory of government duration has to meet the following criteria: events must

(1) enter the utility calculations of the players in the governmental game; (2) be defined and observed independently of government resignations; and (3) be allowed to vary in their (a) exogeneity and (b) sufficiency as causes of government resignations. The sufficiency problem is recognized by the stochastic modelers, but their assumption of exogeneity is more serious. The events that cause governments to fall are assumed to be uncontrolled and random. In reality, however, such events (e.g., embarrassing publicity) are frequently engineered, or at least affected, by players in the game (i.e., parties inside or outside the government). But if critical events are sometimes endogenous, the Poisson model may be fundamentally inappropriate.

Conclusions

The new stochastic models of government durability are disappointing and inadequate. They are too method-driven and constitute exercises in model-fitting rather than explanation. The most serious problem with the stochastic models lies in their relationship to the game-theoretic research tradition. Stochastic modelers—especially Cioffi-Revilla—present no credible way to integrate their work within this research program. This is not to say that stochastic models have nothing to offer the student of government stability, or that coalition theory should rest on its laurels. Game-theoretic analyses commonly rely on simplistic auxiliary assumptions, which Browne, Frendreis, and Gleiber deserve credit for challenging. Yet their rush to falsification is premature at best. As Lakatos has argued, we should be tolerant in criticism and patient in falsification. Flaws in auxiliary hypotheses do not justify abandonment of one of the most rigorous and

comprehensive research programs in political science.

KAARE STROM

University of Minnesota

For some years we have shared with Kaare Strom a concern for explaining various processes associated with cabinet government. Indeed, our work is well known to him since we participated jointly on an APSA panel in 1982, where our stochastic model was first introduced. We also both published papers on the topic in a symposium issue of *Comparative Political Studies* in 1984, and, most recently, our work in the area appeared together with his in a special issue of *Legislative Studies Quarterly* (Frendreis, Gleiber, and Browne 1986; Strom 1986). His current reading and criticism of our approach is surprising in light of this shared history and our own efforts at cumulative theory development, as the following quotation shows:

The immediate theoretical task is to construct a working model of governmental life-cycle which incorporates the findings of both the structural attributes and random events models. We have noted above the complementary quality of these research findings. They suggest that the next theoretical advance in this area should be the formulation of a model which identifies the ways in which political events trigger changes in the ability of governments to continue in office. As a predictive tool, such a model would describe the conditions under which governments fall, even where the appearance of these conditions is not predictable very long in advance. The two phases of research on governmental duration have respectively stressed the static and dynamic elements of the process. The next phase will incorporate and integrate these elements. (Frendreis, Gleiber, and Browne 1986, 626)

As is apparent, we do not argue that the work that preceded ours is invalid or that a decision-theoretic approach should be rejected.⁶ Rather, we see our work and that of Cioffi-Revilla (1984) as presenting

an augmented conceptual and methodological specification of the stability problem. We are pleased to have this opportunity to discuss some recent developments and unresolved questions in this field. Although we disagree with much that Strom has said, we do agree that this is an area where the research agenda is full and the certainties are few.

In his rush to discredit this recent work and rehabilitate the research agenda that preceded ours, Strom finds himself defending standard conceptualizations of the problems of cabinet government that he elsewhere criticizes (1986, 583). We see emerging in his critique a new intellectual history that both misrepresents the relationship of past work to game theory and misunderstands the new research agenda that our work suggests. In both instances the problem seems to emanate from interpretative differences regarding investigative procedures that support the normal science of coalition theory. Since we feel that the most important questions facing this field are still ones of theory development, however, we spend the bulk of our space discussing the theoretical issues highlighted by the recent studies. Inasmuch as an appreciation of this recent work requires its fair representation, we begin by briefly identifying some areas in which we feel Strom has presented distorted or erroneous versions of our findings and conclusions.

Strom's Misrepresentations

Strom criticizes our attention to uncertainty and our conceptualization of the appropriate universe of analysis and responds to our critique of the limited predictive power of the structural attributes approach. While Strom is correct in identifying game theory as an important approach to the study of political coalitions, his comments should not be taken to mean that it is a necessary, or the

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only, approach that has proven useful to researchers.⁷ More than one theory and numerous empirical testing methods have been used in explaining the many aspects of cabinet government and coalition processes. His and ours are just two of these.

Strom's presentation is a substantially distorted description of our criticisms of one of the existing research traditions investigating the problem of cabinet stability, which we have variously termed the "deterministic" (Browne, Frendreis, and Gleiber 1984, 1986b) or "structural attributes" approach (Frendreis, Gleiber, and Browne 1986). Central to the structural attributes approach is an implicit assumption that governments more likely to form are also likely to endure longer. Our position has been that this understanding should be expanded to incorporate the idea that a government is brought down by some "event," the *timing* of which is not necessarily related to those factors (attributes)—such as fractionalization or minimal winning status—that other work has identified as being relevant to the process of government *formation*. Throughout our work we have argued the inappropriateness of the traditional assumption that the structural and strategic conditions existing at the time of governmental formation are directly related to the length of time the government is expected to survive. (In other words, these structural attributes do not determine the timing of the event that marks the government's dissolution.) Our labeling of this as deterministic, in the sense of searching for the determinants of governmental duration in the characteristics of the government, has not ever before been confused with deterministic (versus probabilistic) predictive ability. Our replacement of this term with "structural attributes approach" (Frendreis, Gleiber, and Browne 1986) removes any cause for misunderstanding.

Our basic argument remains that while structural attributes may establish the

baseline of inherent cohesion of a government (its *durability*, in our usage), they should be unrelated to the length of its historical tenure (its duration) unless there is a causal linkage between these structural attributes and the timing of the events that bring about governmental dissolution. The linkage we suggest depends on the government's dynamic response to events in its environment, the changing of the characteristics of the government over time, or the change in the perception of actors as to the meaning of those characteristics to the problem of continuation in power. Given this complexity, a theory of government stability must specify a model, the statistical testing of which depends critically upon a proper evaluation of the error term, for instance, partitioning the error term of regression-based empirical tests into systematic (though-stochastic) effects and the purely random component. We consider reports of the multiple coefficient of determination in the extant literature on cabinet stability to be measures of the information about duration that is carried by various combinations of posited structural attributes. While partial regression coefficients may be useful in estimating the magnitude of relational components in such models, the coefficient of determination is the only measure that describes the whole of the relationship. The persistence of large error terms in these analyses, however, encourages further exploration of the nature of error variance in these models.

The theoretical argument we have advanced in our work has been that the duration of cabinets is appropriately specified by an accounting of the process that results in the dissolution of cabinets. In our treatment, the major theoretical element (admittedly an underspecified, hypothetical construct) has been the postulated occurrence of some "event" (a terminal event) considered sufficient to cause the downfall of a government.⁸ We

hypothesize that the appearance of such an event over the tenure of a cabinet is random as it is oriented in time and, by extension, that the distribution of such events over some set of cabinets (the set of cabinet durations) is also random with respect to timing. This conceptualization of the downfall process runs counter to the specification of current structural attributes models in which cabinet duration is assumed to be causally determined by an attribute set (e.g., cabinet fractionalization, majority-minority status, etc.)—institutional qualities of governments that either preexist or come into existence with the formation of a cabinet. In structural attributes models, then, these qualities (collectively, cabinet durability) are expected to determine cabinet duration. But if we may conceive duration as determined by downfall, and if downfall is determined by the production of terminal events, then for structural attributes to provide a causal explanation of cabinet duration they must also determine the schedule upon which these events occur. Our specification of an events model (1986b) seeks, indirectly, to examine this issue by testing the null hypothesis that the distribution of cabinet downfalls (durations) is stochastic.⁹

In surveying the reported findings of attributes research on cabinet duration, we have concluded that structural attributes models have likely approached an upper limit of predictability at about 30% of the variance (in duration). Strom seeks to refute this conclusion by citing the results of three studies (Powell 1982; Strom 1985; and Warwick 1979). We regard Strom's report of *R*-squared at .305 and .319 in his own analysis to be unilluminating on the issue. It is the other authors who report substantially higher coefficients. While their methodological decisions are thoroughly consistent with their research objectives (e.g., see Browne, Frendreis, and Gleiber 1984, 1986b for our discussion of Warwick), those same decisions

render their empirical results noncomparable with respect to the remainder of duration studies that, like our own work, utilize the universe of governments and weight each government equally. Warwick's and Powell's procedures differ from the other similar studies in that they select cases nonrandomly from the universe of all forming cabinets. Their case selection procedure (and Powell's weighting scheme for governments) serves implicitly to specify additional explanatory attributes (or expunge cases explained by unincluded variables). Such decisions are better made explicitly in a fully specified model. This kind of specification error creates the possibility of significant estimation bias. We consider the attenuation of the relevant universe as the likely explanation of the increase in duration variance explained in these studies, and, therefore, as dubious evidence for optimistic claims regarding the ability of the attributes of cabinet governments to explain (eventually) cabinet duration adequately for the relevant universe. Indeed, an examination of Warwick's alternative models in his Tables 1 and 2 (1979) indicates that even the variables identified as carrying information about duration are redundant to that task. Nor does traditional game-theoretic research offer additional attributes for inclusion.

A final distortion involves Strom's allegation that we have misconceived the basic unit of analysis—specifically, the criteria that determine a change of government (a downfall). The basic charge is that (1) we offer *no definition* of cabinet change (downfall), and (2) *our definition* of cabinet change (downfall) is unfruitful. This contradiction notwithstanding, we believe that neither of these observations is warranted. In a work not cited by him, two of us have discussed the definitional issue in detail (Browne, Gleiber, and Mashoba 1984); and in the articles he does cite, we believe our deci-

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sion rule is clear. We have considered a downfall to occur whenever there is a change of prime ministers, an augmentation (or diminution) of the number of coalition partners, an accepted cabinet resignation, or the holding of an election (necessitating a new investiture vote). We have also published a complete listing of the 93 Scandinavian governments in our data set, along with their dates of formation and dissolution (Browne, Frendreis, and Gleiber 1986a).¹⁰

Respecting our decision to include in our analysis all cabinets (save "caretakers") that formed in a country during a particular time period, we believe that the research question requires an accounting of the stability of cabinet governments, and not only those governments that some researchers believe are "interesting" (e.g., those whose tenure is characterized by interparty conflict among coalition partners). Restricting analysis to some such subset of cases is not worthless, for it does allow examination of a particular kind of instability. However, to assert that this is the only kind of (in)stability that is worthy of investigation is to depreciate (to zero) an understanding of other systemic sources of instability and the impact that such processes may have on the decisions of relevant actors. In short, consideration of the entire universe of cabinet formations and dissolutions allows the modeling of the system that political actors confront and characterizes the uncertainty under which they operate. This implies that cabinets have greater or lesser vulnerability to disruptive events during their tenure, increasing or decreasing the probability of dissolution. Restrictive criteria in the selection of cases may be an appropriate tool where the research intent is to demonstrate the operation of certain processes in a "controlled test"; it is less useful—and in fact systematically misleading—if the goal is to explain dissolu-

tion and identify the factors that precipitate it in the observed population.¹¹

The Research Issues

We shall take up what is perhaps the most crucial of Strom's points, namely, the evaluation of the merit of an events-based approach for understanding stability processes in cabinet governments. We begin by addressing two issues raised by Strom: the relationship of our work to the game-theoretic research tradition on the problem and our supposed failure to provide an adequate "micropolitical" theory to support the stochastic treatment we have given to cabinet stability. After this, we turn to a direct discussion of the potential value of an events-based approach.

We are frankly amazed by Strom's notion that our work has sought to discredit and banish game theory from the arena of coalition research. To the contrary, we have repeatedly argued that cabinet coalition behavior should be ultimately approached as a problem of rational decision making.¹² We assume that our critical survey of research on the problem of cabinet stability (especially in our 1984 article) has unwittingly contributed to Strom's confusion. There we were attempting to uncover the implications of existing coalition models (especially those of Riker [1962] and Axelrod [1970]) for the question of the continuation of a coalition through time. Our conclusion was that these models are essentially time-independent and that this simplification, while allowing for more tractable analyses of the coalition formation problem, presently impedes the application of this work to studies of the persistence of cabinets beyond the point of formation. Later in the same article we observed that uncertainties regarding the persistence of cabinets through time may have implica-

tions for modeling the process of coalition formation, and we proposed a non-cooperative bargaining model (a game of fair division) as promising (precisely the class of models, Strom notes with approval, currently under investigation by others).

The more serious question, however, concerns how the several related processes associated with coalition behavior have been developed in the literature. Analyses of cabinet coalition formation and payoff allocation have been developed, in the main, from models of n -person cooperative games. The problem of cabinet stability, however, has developed from an entirely separate research tradition, beginning with the institutional observations of A. Lawrence Lowell (1896) and extending through studies of whole societies (e.g., Almond, Flanagan, and Mundt 1973). Thus, while empirical work on formation and payoff allocations has sought to test propositions and implications deduced from formal models, contemporary work on cabinet stability has sought to relate favored systemic attributes with a surrogate stability measure (typically, cabinet duration). The recent structural attributes research that begins with Taylor and Herman (1971) and extends through Warwick (1979) relies on variables identified by game and coalition theory (e.g., minimal winning status) but is not itself characterized by the formal or deductive methodology associated with game theory. Strom's characterization of the recent stability research as being part of the game-theoretic tradition of coalition studies, then, is simply wrong. More than that, it is mischievous insofar as we are asked to imbue this research with the considerable explanatory power that is characteristic of deductive analytic structures (e.g., game-theoretic models).

Paradoxically, one of the main faults we have found with the structural attributes approach has been its failure to locate the phenomenon of interest (extent

of cabinet duration) in a "micropolitical" theory. Rather than specify models of a decision process involving rational behavior by human or party actors, this approach instead proposes that cabinet duration is determined by several systemic attributes that (mostly) are descriptors of institutional parameters within which cabinets function (e.g., majority-minority coalition status, number of party actors, and ideological cleavage in a cabinet). Since the unit of analysis in these studies is the government, we see the model investigating relationships at a level far removed from the decisional behavior of individuals or parties. In fact, no claims regarding individual or party behavior emanate from these studies. In contrast, we have clearly specified a preliminary (and very simple) model of cabinet decision making that is relevant to the problem.¹³ Since in our model the downfall of cabinets is attributed to the occurrence of terminal events, the production of such events occupies a central place in any events model of cabinet stability. This does not mean that there is no mediation by political actors; rather, events drive the process. Still unresolved, however, is any theoretical development of the idea of "events processes." Lacking such understanding, we have nevertheless posited their existence and assumed the timing of their occurrences to be uncertain. By extension, the duration of cabinets is uncertain, and thus may be treated stochastically. In our model we have represented the uncertain timing of terminal events by specifying that the duration of cabinets is a random variable with a Poisson-type (negative exponential) distribution.¹⁴

We have not proposed a model of cabinet stability that is either claimed or intended to be complete in the sense of providing a fully adequate explanation of the processes that govern the downfall of cabinets. The simplicity of our model is for us a necessary first step in developing

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a new approach to our research problem. Its main departure from the earlier work is to conceive the tenure of a cabinet as a continuous decision problem confronting cabinet actors. At present our concept of events (demands from the environment) is confounded by its inclusion of objective, perceived, and anticipated environmental phenomena. Thus, "event" is at best a hypothetical construct and at worst a positive heuristic not unlike rationality, utility, or perfect information concepts in the core propositions of the touted game theory. Specifically, the tenure of cabinets is conceived as a series of actor decisions regarding the continuing existence of their cabinet, extending through time from formation. The model assumes that over this period, actors are confronted with a series of "events," one of which (a terminal event) will impose a demand that one or more actors will not or cannot satisfy. When this occurs, there is cabinet dissolution.

The test of our stochastic model (Browne, Frendreis, and Gleiber 1986b) was intended solely to establish the plausibility of our conception of the dissolution process, not to provide a predictive model capable of explaining this process, as Strom seems to believe. Indeed, it should have been evident that our model of the dissolution process is perfectly null; it establishes (counter to structural attributes models) an expectation that the continuing existence of cabinets is not systematically influenced by factors in their environment. In other words, the model assumes that if a cabinet exists at any point in time (e.g., on a particular day), the probability that it will exist at the immediately succeeding point in time is the same regardless of whatever point in time is chosen. Thus the particular timing of a cabinet dissolution is uncertain, in fact perfectly uncertain, leaving the expectation that a given cabinet may fall at any time at all.

Our model was tested by a comparison

of observed cabinet durations and the hypothesized negative exponential distribution. Here the expectations of the model are "confirmed" if we fail to reject the null hypothesis that cabinet duration is a random variable with a negative exponential distribution.¹⁵ Drawing on duration data from 12 postwar parliamentary democracies, the results supported an inference based on the failure to reject the null in four countries (Belgium, Finland, Israel, Italy); while for the remaining eight, the probability estimates were of such magnitude as to prevent any confident inference with respect to the hypothesis.¹⁶

Among other things, Strom contends that the empirical test we conducted is merely an exercise in curve fitting and a premature and misleading "rush to falsification." We reject his apparent view that the presentation of anomalous evidence somehow lies outside valid scientific procedure. Strom's appeal to the authority of Lakatos notwithstanding, scientific progress proceeds from raising questions and generating new hypotheses, even when the first steps in that direction are less fully supported by a long-standing theoretical tradition.¹⁷

The fundamental concern, however, should not be whether we are sufficiently respectful of some theoretical icon but rather whether or not our depiction of the coalition dissolution process is accurate. As the opening quotation indicates, we believe our work complements the earlier tradition, in effect providing a necessary redirection for researchers interested in this problem. We do not question the validity of the findings reported from the analyses relating structural attributes with duration. We tentatively accept, furthermore, the interpretation that systemic attributes may furnish some cabinets with a quality of inherent stability (or durability). It is rather the effectiveness of this quality in preventing (or delaying) the downfall of cabinets that is at issue. The fact that there is correlational evidence to

suggest some (rather weak) effect has led us to conclude that "cabinet duration may be expected to be distributed around a durability-determined central point, but with a large average deviation" (Browne, Frendreis, and Gleiber 1986b, 634). Thus, if we are correct in assuming that events are the causative agents of cabinet downfall, the inherent stability of cabinets, furnished by particular attributes, should induce a systematic effect in the timing of cabinet dissolutions. Indeed, it appears that some such effect may have been observed in those eight countries where our test results did not support an inference of failure to reject the null. In the remaining four countries, however, the pattern of cabinet dissolutions supports an inference that the durability attributes of their cabinets were ineffective in impeding the impact of randomly timed terminal events. Indeed, as we have reported, the goodness of fit of the structural attributes and random events models are inversely related across systems (Frendreis, Gleiber, and Browne 1986; Browne, Frendreis, and Gleiber 1986b).

In summary, existing structural attributes models typically report the existence of a positive linear relationship between systemic attributes and cabinet duration, but with a substantial amount of residual error. Attributes researchers have tended to deal with the problem presented by a large error term in two ways. First, they have introduced some new attribute variables or sought new measurements of existing ones in hopes of explaining additional variance. Second, they have decreased the size of the error term through decisions relating to the selection of cases. (Indeed, Strom has criticized us for failure to do this by including in our data set cabinets that do not terminate for what he considers reasons related to political conflict.) Our events approach to the problem takes issue with the presumption that the iden-

tification of new attributes (or measurements) is likely to result in a substantial reduction of the remaining error variance in regression models of the process. Rather, we believe that an essential component of the process that causes the particular timing of cabinet downfalls is intrinsically stochastic. For some countries systemic attributes appear to have no appreciable impact on the timing of cabinet dissolutions. In the remaining countries, attributes may act in such a way as to prolong (or foreshorten) the duration of cabinets from what would be expected of a perfectly random downfall pattern. Even here, however, variability over the range of probability estimates, coupled with the large error terms of regression models, suggests the likelihood that the pattern of downfalls is heavily influenced (although variably) by stochastic elements.

The Research Agenda

It should by now be clear that what separates our work from that of structural attributes researchers is a basic difference in the conception of the idea of stability as it is attached to cabinet governments. For us, cabinet stability is importantly dependent upon stochastic elements in the decisional environment (disruptive events), the timing of which are (largely) unpredictable in advance. Thus stability should be considered a continuous (rather than a discrete) quality of cabinets, where the continued existence of a cabinet is an interactive function of the impact of disruptive events (or events patterns) on decision making and some quality of resistance to such events that *inter alia* might be associated with structural attributes. Thus cabinets should be viewed as being more or less vulnerable to the continuous occurrence of randomly timed events over their respective tenures, with downfall representing, potentially, a transition from one state of dynamic equilibrium to

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another. Since we propose creation of a dynamic model of cabinet stability, we have been hopeful of discovering evidence supportive of an inference that the timing of cabinet dissolutions is influenced by stochastic elements. We feel that the evidence of our tests is sufficient to warrant further development of an events approach to the problem.¹⁸

While such a research agenda as we propose could be developed quite independently of a reliance upon game-theoretic representations of related governmental processes (such as government formation or the distribution of benefits), we wish to close by suggesting some implications an events approach to cabinet stability has for the development of coalition theory in general. As noted, we see the major contribution of an events approach to be the identification of a major source of uncertainty in the rational decision-making calculations of coalition actors. For if payoffs to actors are assessed in terms of benefits that are expected over some period of time in office, uncertainty regarding expected utility and the length of tenure might reasonably alter their decisions—both with respect to choice of partners and preferences for particular benefits—from what they would be if cabinet longevity were either irrelevant or fully predictable. Further, uncertainty as to longevity encourages us to view coalition processes dynamically, since actors may be expected to base their decision-making behavior on probability assessments about changing conditions (such as the reliability of a flow of benefits). This suggests that the articulation of game-theoretic explanations might profitably draw on recent developments that attend to metagames (super-games), relaxation of restrictive information assumptions, and reevaluation of the conflictual-cooperative nature of joint decision-making processes.

As is readily seen, our work on an events approach is in a very early stage of

development and is not, as Strom has asserted, an attempt to displace the game-theoretic foundations of coalition research. The development of an events approach calls for a redirection of scholarly attention from the question of how long cabinets may be expected to endure toward the questions of when and why cabinets will fall. Our reconstruction of the central research question focusing on cabinet dissolution rather than duration accomplishes a great deal, for it alters the structure of the problem from one that seeks identification of new and improved structural attributes believed to promote (inhibit) intracoalition consensus to one that locates elements of stress and support encountered by decision makers over the period of their tenure in office. Strom seems to concede this point in the two paragraphs that precede his concluding section. We are encouraged that our work has apparently succeeded in promoting this reconceptualization of the problem.

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Notes

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1. Cioffi-Revilla never addresses the theoretical foundations of the "deterministic" tradition. Its applicability to Italy is dismissed simply by the assertion that the causes of Italian government downfalls have been "numerous, varied, and essentially unpredictable" (1984, 334). This statement is followed by a selective catalog of Italian postwar cabinets, with a brief ad hoc description of the causes of their respective dissolutions. There is no semblance even of a

common conceptual framework that could permit analysis of these alleged causes.

2. Warwick, like Powell, reportedly is able to explain about half the variance in cabinet durability. However, Browne, Frendreis, and Gleiber (1986b, 632) fail to obtain equally impressive results in their attempt to replicate Warwick's analysis. The discrepancy appears to be caused by ambiguities in Warwick's case selection procedure.

3. Empirical studies often implicitly assume the legitimacy of treating individual governments as mutually independent events and of cross-national generalization. If such assumptions are not made, sample sizes may diminish substantially. However, even in single-nation studies, the number of cases is often comfortably large (e.g., 39 in Cioffi-Revilla's analysis).

4. Browne, Frendreis, and Gleiber implicitly recognize the problem of heterogeneity by estimating separate models for each country. However, this obviously still leaves them open to all the problems of intrasystem heterogeneity and contagion.

5. However, if the argument about payoffs as a function of time is taken to an extreme, it may eliminate the role of critical events in explaining government dissolutions. In other words, the payoff changes responsible for all government resignations could conceivably be accounted for by the passage of time (e.g., toward the next election).

6. The relationship between games and decisions is addressed in some detail by Luce and Raiffa (1957). They discuss the role of uncertainty in decision making and the theory of games. It should be clear that assigning the appropriate conceptual significance to uncertainty and adequately modeling it is critical to our formulation of the cabinet stability-duration problem.

7. Indeed there are voluminous literatures on coalition processes that have been developed in social psychology and sociology. For reviews, see Murnighan 1978 and Hinckley 1981.

8. Strom is critical of our failure (except by illustration) to imbue the theoretical element, "terminal events," with substantive content. Not only is his observation here correct but we consider this to be a major impediment to progress in theory development. Aware of this deficiency, we have begun investigating the problem and have reported some preliminary results of these efforts (Browne, Frendreis, and Gleiber 1986a; Browne and Gleiber 1986).

9. Although attributes researchers have never investigated the events environment of cabinets, an indirect assessment of the ability of structural attributes models to "predict" the events processes that result in cabinet dissolution inheres in the coefficient of alienation they produce. If we are correct in presuming that events (or events processes) are the proximate causes of cabinet downfall, then for structural attributes models to predict cabinet duration

successfully, the appropriate regression model should specify events as intervening between attributes and duration. Since in existing structural attributes models no intervening events variables are specified, the implicit presumption is either that events are unrelated to duration or that their ability to bring down a cabinet (the timing of "terminal events") is completely determined by structural attributes. In either case, events are not seen to have any independent effect on duration. Thus, in structural attributes models, the coefficient of determination constitutes a measure of the degree to which events processes are statistically irrelevant to an explanation of cabinet duration.

10. Compare our enumeration of Scandinavian governments to Strom's (1986) appendixes.

11. Similarly, Lijphart's (1984) recommendation of a particular definition of durability is based in part upon the strength of the correlations between a set of systemic independent variables and the five alternative measures of durability he examines. However, contrary to Strom's assertion that Lijphart designates one definition as uniquely superior, Lijphart actually concluded, "All five measures are basically acceptable" (1984, 278).

12. Indeed, we have been quite explicit about this. See Browne 1982, 352-56; Browne and Franklin 1986, 479-80; Browne, Frendreis, and Gleiber 1984, 184-92; Browne, Frendreis, and Gleiber 1986b, 621; Frendreis, Gleiber, and Browne 1986, 621.

13. See Browne, Frendreis, and Gleiber 1986b, 636-40.

14. In our model, we use the theoretical concept "terminal event" as a hypothetical construct to provide the linkage between the observed distribution of interarrival times of dissolutions and the relatively large stochastic (error) terms in structural attributes models of cabinet duration. The "curve fitting" of which we are accused is the comparison of a derived expected distribution to an observed distribution. Regression models produce the same kind of comparison, but their basis is the inductive estimation of a BLUE curve to represent the theoretical, or expected, distribution. This leads us to conclude that our work is more squarely in the deductive tradition Strom has been advocating. Furthermore, empirical tests of game-theoretic models (preferred by Strom) depend on the same sort of comparison of expectation and observation as ours do. The resulting analysis is an uncontrolled (no inclusion of the structural attributes explanation) model of what we believe to be the primary component of an explanation of government dissolution.

15. This is where Strom is confused by our use of the term *alternative hypothesis*, by which we mean research hypothesis. The usual statistical test is to reject the null hypothesis while contingently accepting an alternative, the empirical estimate, as a research hypothesis. By implication, failure to reject the null hypothesis warrants rejection of the alternative

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research hypothesis.

16. We are unfamiliar with any work supporting Strom's assertion that fewer degrees of freedom reduce the power of the chi-square test. We assume Strom is actually referring to small expected cell frequencies, a point we specifically addressed (Browne, Frendreis, and Gleiber 1986b, 642, n. 11). For additional consideration of this question, see Chapman and Schauffle 1970, especially chapter 11. Strom also makes inappropriate use of the technical term *power* (1-beta) which refers to the probability of rejecting the null hypothesis. Although *power* is related to confidence, sample size, and effect size, *power* is not an issue in our research, because we do fail to reject the null hypothesis. The inconclusiveness of our empirical test across countries may result from the contamination of the simple univariate model by factors other than events (especially those determinants of duration identified by the traditional literature) that are uncontrolled in our test. Despite this limitation we obtain strong positive empirical results.

17. While our empirical work is not derived from a fully articulated theory, the earlier structural attributes work is equally vulnerable to such criticism. This work, for example, typically employs statistical models that are not the derived implication of any theory; the properties of the regression models employed in this research (e.g., linearity) have not been justified, either verbally or mathematically, in discussions of the hypothesized relationship between attributes and duration.

18. Our basic conception of stability anticipates the potential fruitfulness of nonlinear dynamic modeling. Relevant literature supportive of this view is rapidly developing in mathematics, the physical and biological sciences, and information theory. For a reasonably comprehensive survey of these developments, see Gleick 1987 and Campbell 1982. Unsurprisingly, this methodology has been slow to be disseminated into the social sciences, but relevant applications have been undertaken in economics with reference to the long-term fluctuation-stability of prices in both the cotton market and the stock market and in political science in an examination of regime stability in pre- and postrevolutionary Nicaragua (James 1988). Additionally, dynamic modeling of international conflict processes and alliance behavior has recently gained a secure position in the international relations literature. Here, prominent examples include work by Michael D. Ward (1982a, 1982b), Manus Midlarsky (1981), Robert T. Holt, B. L. Job, and L. Markus (1978), and several selections in a compendium on breakdown in the international system, edited by Dina Zinnes (1983).

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IS OPPOSITION TO NUCLEAR ENERGY AN IDEOLOGICAL CRITIQUE?

In the June 1987 issue of this Review, Stanley Rothman and S. Robert Lichter offered evidence to support their argument that "the new environmental movement in the United States is partly a symbolic issue," that elites in the news media and in public interest groups misrepresent the dangers of nuclear energy as a surrogate for more direct criticism of liberal capitalism in the United States. In this controversy, Charles J. Helm expresses skepticism about the Rothman-Lichter line of argument; and they respond.

Rothman and Lichter (1987) argue that within the last 20 years there has been a significant shift in public attitudes over the risks attendant with nuclear energy. Drawing on a content analysis of the media's coverage of nuclear energy and a survey of attitudes in the media elite, they find evidence of biased stories authored by an elite skeptical of nuclear energy and critical of U.S. society. They conclude that the media elite gravitated to environmental issues (consciously or not) as a "surrogate" for an underlying ideological critique of U.S. society that was necessitated by the demise of movement politics in the mid-seventies and the absence of a leftist political party as a more traditional channel of political critique. The main thrust of my remarks will center on two points central to their argument: first, the posited link between public attitudes and a biased media elite and second, the Left's search for a "surrogate" avenue—environmental issues—when other outlets were foreclosed.

Causal Link

The time sequence for the posited link between media bias and public skepticism

over nuclear energy, as in any causal argument, is controlling. Is there any evidence, in Rothman and Lichter's account, that media coverage of nuclear energy underwent a shift to a more critical mode prior to the public's growing doubt over nuclear safety? They seem to feel that if media and public attitudes have changed on the issue of nuclear safety then the argument for causal sequence is implicit in the relation between the media as catalyst and the public as receptacle. As evidence of the causal sequence they offer the argument that since media coverage and public attitudes in the fifties and early sixties were sympathetic to nuclear energy and now a "substantial majority" is opposed to any new nuclear plants, the major source of that change is, most likely, fears generated by the media during the seventies. (The confidence of the scientific community on nuclear energy has remained constant throughout.) The unstated assumption here is that the public is poorly informed and heavily dependent on media cues for any shift in public attitudes. At some point in the late sixties, according to Rothman and Lichter, the media apparently grew increasingly out of sync with public attitudes on safety in nuclear plants. But for all the care and sophistication they have shown in the development of their

surveys and supporting analysis, they offer no evidence and little argument to support any shift in media coverage preceding a shift in public attitudes. Even if the media consensus now is not in accord with the present scientific consensus on the safety of nuclear energy, that is no evidence of the time sequence or of a causal link.

While we lack any direct evidence—surveys of attitudes in the media elite or content analysis of the media's coverage of nuclear energy during the sixties—of the transition in media coverage, there is indirect evidence that suggests the need for caution in trying to isolate the causes of a shift in public attitudes. Are the media, as Rothman and Lichter imply, the major arbiters of public attitudes? The recent controversy surrounding the disease AIDS might prove instructive here. The scientific community has almost uniformly contended that AIDS cannot be spread through casual contact. More specifically, unless one either is sharing needles or engaged in intimate sexual relations with a suspected AIDS carrier or is the unfortunate victim of a tainted blood sample, the danger is minimal. The media, in contrast to its account of the risks attendant on nuclear energy, has generally been quite "responsible" in reporting the scientific consensus on the medium of AIDS exchange. Apparently these careful admonitions have fallen on deaf ears. In some school districts, AIDS victims have been forced to withdraw from the public schools; and in California a referendum that would have required the quarantining of suspected AIDS carriers, while handily defeated, still drew considerable support. What is the source of such hysteria, fear, and prejudice? Why isn't the public listening to its "leaders" as reported by the media?

The obvious rejoinder here is that nuclear energy and AIDS really are not comparable. AIDS is a disease that has only received public scrutiny for the past

few years and is confined in large measure to a population outside the mainstream. There is no cure in sight and the relentless figures on deaths from the disease are rising at a geometrical rate. Despite the accidents at Chernobyl and Three Mile Island, there have been far fewer deaths directly tied to nuclear energy. Nuclear energy has been on the public agenda since World War II and the links between illness or deaths and nuclear energy are considerably more tenuous. These contrasts, though well taken, seem to me insensitive to public concern, mistaken or no, on issues like nuclear energy and a disease like AIDS. Well before the supposed shift in media coverage, public fascination and fear of the atom was much in evidence. During the fifties there were countless "mutant" movies (Boyer 1985; Oakley 1986) clearly tapping public fears and suggesting the untoward effects of radiation exposure on humans and animals (ants, tarantulas, bees). The atom has been a powerful metaphor suggestive of both life and death, beauty and destruction, the beginning and the end. The public has not been willing to allow the scientific community the last word in shaping their perceptions of this mysterious and awe-inspiring phenomenon. In the work of Lippman (1922), Edelman (1967), Bennett (1987), and Nimmo and Savage (1976) on images, symbols, and stereotypes, we have some sense of the possible disjunction between image and reality and the types of issues that are more prone to these sorts of distortions. The public hysteria (within some communities) over AIDS or the atom or both and the disparity between elite and mass attitudes should offer a cautionary tale for those tracing lines of influence between the media and its audience.

Surrogate Thesis

Rothman and Lichter have rather nicely tapped a disjunction between the scien-

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tists' confidence in nuclear energy and public skepticism. They are sufficiently surprised and disturbed by their findings to feel that it warrants talk of a leftist surrogate manipulating (consciously or no) public attitudes through the media. But perhaps the explanation they seek is far less oblique: less psychological and more materialist. Lipset and Schneider, in *The Confidence Gap* (1987), after reviewing hundreds of public opinion surveys over the past 20 years, have shown a steady decline in public confidence in institutions at all levels in U.S. society. While they second the Rothman and Lichter account that the press has tended to give a rather bleak picture of business and government on environmental issues, they reject "the assumption that this emphasis is largely a reflection of a liberal bias" in the media, suggesting instead that it is "the desire of journalists to find exposés, to get the story behind the official report. There is a story in reporting the 'failure of . . . institutions'; the version that reports successes is viewed as public relations" (Lipset and Schneider 1987: 438). As to the source of this loss of public confidence in our institutions, rather than a leftist media in search of a surrogate avenue, they suggest a more mundane answer: "Vietnam, Watergate, the energy crisis, recession and hyper-inflation demonstrated . . . that our institutions generally, and the federal government in particular, had failed to perform" (p. 436).

The evidence for the "surrogate" thesis is of course indirect. Much of the force of that thesis rests on a contrast Rothman and Lichter draw between the Left in France and in the United States. In France, "the political left does not require surrogate issues. It can still mount a direct attack upon the existing social and political order" (p. 397). Lacking the need for surrogates, "the French Communist party is strongly supportive of nuclear energy, and the French Socialist party at least moderately so" (p. 402). The absence of

an institutionalized Left in the United States has apparently created both an alternate outlet (the environmental movement) and a pseudo-need (concern about nuclear safety). The French Left, less frustrated than their U.S. counterpart, have no need for surrogates and remain largely indifferent to environmental issues. (I take it that it would follow from their argument that if—apparently a very big *if*—the French Left ever makes a "frontal" attack on environmental issues they will see them "clearly," that is, bereft of any ideological tainting). Environmental beliefs in the United States are perhaps "sincerely held" on Rothman and Lichter's reading, but they are also evidently the dyspeptic substitute for the failures of leftist politics. Whether purposely or not, one effect of their argument is to delegitimize claims of the environmental movement—both within the media and without—with respect to safety and nuclear energy. They have moved from a critique of the environmentalists' positions to the structural dislocations—both institutional and psychological—of leftist politics.

In an article so "sensitive" to the Left's ideological tainting of the issue of nuclear safety, it is disquieting to see a comparative scholar of Stanley Rothman's stature select France as a relevant counterpoint to U.S. environmental politics. As they note, only to dismiss it just as quickly, France is something of an aberration in Europe, combining an aggressive nuclear energy program and a weak environmental movement. West Germany—or Sweden or Belgium—would provide a more challenging counterfactual test of their "surrogate thesis" and the posited link between the media, nuclear energy, and the Left. Presumably, West Germany would also fit their model of a political system with an organized Left—the Social Democratic Party (SPD). Even though the SPD was the party in power during much of the early history of the "citizen initia-

tive" movement in the late sixties and early seventies, these groups began outside the traditional channels of neo-corporatist politics in West Germany (Conradt 1982). It was their sense of the closure of German politics, even within the organized Left, that necessitated the need for *alternativen*. The Greens only organized nationally in 1979 to consolidate a massive citizen protest against any further building of nuclear energy plants and only entered the Bundestag as a recognized party in 1983. While the SPD and the Greens have formed coalitions in some *länder*, there is still a quite tenuous truce between the traditional Left and the Greens. (During party conferences the Fundies—a radical wing of the Greens—have repeatedly voted to withdraw from parliamentary politics.) Why did one segment of the West German Left, with the collusion of some traditional agrarian conservatives like Herbert Gruhl, attach themselves to a surrogate environmental movement even while the SPD—though weakened—remained quite powerful? What is the source of this frustration and denial of traditional leftist politics in West Germany? Have scientists in the Federal Republic reached a more critical consensus on nuclear safety?

The plot thickens even further if we bring in the other missing variable—the West German news media. In the United States, Rothman and Lichter argue, media bias has been, at least in part, responsible for the public shift in attitudes. As any introductory text on German politics makes clear (Conradt 1982; Edinger 1977; Rothman 1970), German television and radio is administered by state-based independent boards. While the networks possess considerable autonomy, the establishment political parties make sure their voice is heard on these boards. Within both German television and the independent press, there is also no developed tradition of muckraking or investigative journalism. (There are several exceptions,

the two most obvious being the weekly magazines *Stern* and *Der Spiegel*.) The Axel Springer conglomerate controls a large part of the German press (its papers draw over 60% of the readership) and is staunchly antileftist with the Greens a favorite target (Conradt 1982). Many newspapers are also frequently identified with a particular party and tend to follow party policy quite closely. (During much of the seventies, the SPD and Christian Democratic Union [CDU] party line was quite supportive of a nuclear energy program.) Whatever the evidence for media bias toward the nuclear industry in the United States, the picture is considerably more mixed in West Germany. Why—given the absence of systematic media bias and the presence of an organized Left—has the antinuclear movement (critical of both nuclear weapons and plants) been so strong in West Germany amongst such a wide segment of the German population?

The source of public attitudes on issues like nuclear energy are really quite difficult to trace; and for all the care that Rothman and Lichter have shown in their surveys and analysis, their linkage of media bias and public attitudes is still not proven. Their suggestion that the environmental movement has served as a "surrogate" for an underlying ideological critique is not supported by the West German example. And finally, the "surrogate thesis" implies both that the Left was forced to seek out alternate avenues of expression and that they found their voice and target as a result of the failures of leftist politics. Such a displacement thesis demands quite compelling evidence, particularly since less circumspect explanations are readily available (e.g., Lipset and Schneider 1987).

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Opposition to Nuclear Energy

Charles Helm raises two issues: (1) he denies that we have demonstrated either that the U.S. national media have contributed substantially to nuclear energy's fall from grace in this country or that ideology has influenced their coverage; (2) he questions whether we have demonstrated that opposition to nuclear energy is partly a surrogate for more direct attacks on the political system. He rejects our comparison with France and suggests that if we had turned to other countries, our argument would be less persuasive. After all, a party of the Left (the Social Democrats) exists in Germany, and the Left has mounted an attack on nuclear energy.

Before we turn to the main body of our reply, we should make two points. First, we agree that nuclear energy has several strikes against it and that public anxieties about it are easily aroused (see p. 401 of our article). We explicitly state that such public concerns about nuclear energy and other environmental issues are not without foundation. In dealing with one factor that has played a role in the ongoing public debate, we do not deny the relevance of other concerns. Our aim is not to discredit antinuclear activists by examining their motives but rather to understand the process of social change.¹

Second, we do not believe that we have proven our hypotheses in any absolute sense, only that we have established a *prima facie* case. Most of the issues we raise require a far more in-depth treatment than we could provide in the article. We plan to do so in Rothman, Lichter, and Lichter 1988. Even then, our evidence will necessarily lack the compelling quality it is possible to achieve in some areas of the natural sciences.

Let us turn first to the issue of media influence. We do not maintain, as Helm suggests we do, that the disjunction between public and scientists' views establishes that the media are responsible for changing public attitudes. Our argument

is more complex. Specifically we demonstrate that (1) scientists overwhelmingly support the development of nuclear energy and believe that nuclear plants are safe;² (2) media reporting suggests that the scientific community is sharply divided over nuclear safety, with perhaps a majority opposing the development of nuclear energy; (3) elite and public perceptions of the scientific community's views more closely resemble media reports than reality (as measured by our surveys of scientists); (4) elite views of nuclear safety (a key determinant of policy preferences) correlate with the perspectives presented on media outlets that they consider reliable. People trusting antinuclear news outlets are less likely to believe that nuclear plants are safe than are those persons who trust pro-nuclear media outlets. So we do not *assume*, as Helm argues we do, that the public is badly informed. We demonstrate that both the public and various leadership groups are misinformed and that their misinformation seems to be related to media coverage of the issues involved.

It is also logically possible that prior ideological convictions drive *both* nuclear attitudes and media consumption. Therefore we agree with Helm that our case would be strengthened by demonstrating that changes in media coverage of nuclear energy *preceded* changes in public views of nuclear energy. In fact, we have developed this evidence on television reporting and public attitudes for the 1970s. As Table 1 demonstrates, the decline in positive public attitudes toward nuclear energy was preceded by both heavier and increasingly negative television coverage, even before the accident at Three Mile Island. Note also that as coverage grew more negative, it also increased in absolute terms and became more focused on reactor safety.

Helm attempts to demonstrate that the media's effect on public opinion is not very great by citing media coverage of

Table 1. TV News Coverage and Public Opinion on Nuclear Safety

Time Period ^a	Stories per Year ^b	Reactor Safety Stories (%) ^c	TV Slant (%) ^d	Public Opinion (%) ^e
1971				32 (57 - 25)
1970-75	26	8	-14 (25 - 39)	
1975				23 (55 - 32)
1976-78	50	17	-24 (12 - 36)	
1978				2 (43 - 41)
1979	518	41	-30 (17 - 47)	
1980				-35 (28 - 63)

^aSome of the data is time-discrete and some is time-aggregate. Figures were unavailable for 1969.

^bAverage yearly number of nuclear energy stories.

^cPercentage of all nuclear-related stories dealing with safety of nuclear reactor operation. (Source: Lichter, Rothman, and Lichter 1986, 185-86, 203, and recalculation of our data.)

^dPercentage of positive stories minus percentage of negative stories. (Source: Lichter, Rothman, and Lichter 1986, 204 and recalculation of some of our data.)

^ePercentage in favor minus percentage opposed. (Source: national Harris and Gallup surveys cited in Rankin et al. 1981, 30-31.)

AIDS. He asserts that while such coverage has been very responsible, the public has still responded in a highly panic-stricken manner. Although systematic content analysis is lacking,³ nevertheless it is our impression that Helm is probably correct in his assessment of media coverage. We disagree, however, with his characterization of public reaction: despite his anecdotal evidence, the panic has thus far been relatively minimal (*New York Times*, 17 February 1988).

The key comparison is this: imagine how the public would react if the media featured stories that gave the impression that a significant portion of the scientific community believes AIDS *can* be contracted through casual contact with infected individuals. Recall the recent wave of media-borne criticisms that descended upon Masters and Johnson for suggesting

this and on *Newsweek* for publicizing their study.

We never suggested that the media are all-powerful in shaping public views. Our argument is merely that they play an important role in the formation of such opinion. Helm cites Lipset and Schneider's *The Confidence Gap* (1977) to demonstrate that journalists' reportage is shaped by the desire to expose (rather than by political views) and that the media have little influence on public opinion. We never argue that journalistic coverage is determined entirely by the political views of journalists, only that their views do influence coverage. We offer considerable evidence for that proposition in our article, as well as our book, *The Media Elite* (Lichter, Rothman, and Lichter 1986).

Further, Lipset and Schneider's own evidence does not support the assertion

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quoted by Helm. For one thing, they do not examine journalists' attitudes or coverage of issues. Their bad news hypotheses is unable to explain media coverage of AIDS, nor can it explain positive media coverage of busing, as we document in *The Media Elite*. Second, as Lipset and Schneider point out, the public's trust in business in the abstract was declining at the very same time that their personal experience with business continued to receive high ratings. In addition, Lipset and Schneider found an inverse correlation between trust in government and trust in the media. Finally, Democrats and liberals trust the press more than do Republicans and conservatives (Lipset and Schneider 1987, 99, 122-25). It is unlikely that these correlations are accidental.⁴

Helm implies that U.S. citizens came to reject nuclear energy because they did not trust scientists sufficiently to give them the last word on the issue. He may be right. We shall never know because, as our article demonstrates, the public in the early 1980s still trusted "independent" scientists' views on nuclear energy but were misinformed about such views (p. 384).

The surrogate issue, Helm's second concern, is a very complex one, which we plan to develop in more detail in our forthcoming volume, *Elites in Conflict* (Rothman, Lichter, and Lichter 1988). We will limit ourselves here primarily (and briefly) to Helm's criticism of our choice of France as the country with which to compare the United States and his use of Germany as a counterexample. We do have some data on the issues that he discusses. We should, however, note that one characteristic of all European countries is the considerable influence of the U.S. media. The United States' declining economic power has yet to reduce its cultural hegemony (Richard Grenier, "Around the World in American Ways," *Public Opinion* Feb./Mar. 1986, pp. 3-5,

58-60; Jack Valenti, "And the Winner Is . . . American Movies, Television, and Videos," *Public Opinion* Feb./Mar. 1986, pp. 13-15). There is little question but that the U.S. media helped bring the student movement to full flood in Europe in the 1960s (Rothman and Lichter 1982), just as they helped bring the nuclear energy issue to a world wide public. Further, as our article points out, a perspective, once institutionalized, may draw upon many other motives in addition to that which fueled the initial movement. One must be careful not to extrapolate backward from the motives and arguments used in a movement at full flood (or in decline) to its sources.

Having said all this, the German case better supports our argument than Helm's. First, radicalism in West Germany has lacked a political outlet since World War II, although for somewhat different reasons than in the United States. The Communist party was effectively destroyed by the war and the creation of East Germany; the SPD jettisoned its Marxist program in 1959 and joined the Christian Democrats in a coalition government in 1966. The emergence of the student movement of the late 1960s was partly a function of the lack of an outlet in the parties for feelings of radical alienation from the system. Helm makes this point himself.

Despite its slight shift to the Left during the 1970s, the SPD remains, in the eyes of alienated students and other intellectuals, a bastion of the system that lacks a genuine commitment to radical change. We submit that the Greens provided an outlet for themes of alienation that could not be expressed through the SPD or other conventional structures. The growth of the Greens has pushed the SPD to the Left, but it remains a party of the establishment.

Helm's position on the German media's treatment of nuclear energy is refuted by systematic survey and content analysis data.⁵ First, as we point out in our essay

(p. 387), the German scientific community is as supportive of nuclear energy as is its U.S. counterpart. The study we cited there (Institut für Demoskopie 1985) also found that journalists were skeptical about nuclear safety and that the public was not aware of the extent of the support of the scientific community for it. In short, their findings replicate ours.

More recently, H. M. Kepplinger and Rainer Mathes of the Institut für Publizistik at the University of Mainz studied nuclear energy coverage in seven major West German print outlets over two decades (1965-86) and found the same rise in negative coverage that we did. They also studied coverage of several other environmental issues such as air and water pollution. They conclude, "When we look at reality, we can ascertain that the environmental situation has improved, but when we look at the media's version of reality, the environmental situation continues to deteriorate." They analyze this coverage in terms of a new ideological perspective which "took hold of journalism in the mid-1970s." As they note, "it was not the reality which changed, but the perspective. This new portrayal of reality by the media led to a fundamental change in the public's views" (Kepplinger and Mathes 1987, 15-16).⁶

Any attempt to explain attitudes toward particular issues as unconscious surrogates for attitudes toward other issues is impossible to establish definitively, certainly in a short essay. This, of course, is true of most propositions of any interest in the social sciences. Nor do we claim that the surrogate argument fully explains what has been happening in Germany and the United States. Real issues are involved and massive shifts are taking place in European and U.S. social and political life. We hope to deal with many of these in *Elites in Conflict*.⁷

Nevertheless, environmentalism was and is a surrogate issue for many on the Left. Our correlational data support this

argument, as do the statements by such people as Dellinger and others quoted in our essay.⁸ As the environmentalist scholar Langdon Winner points out in *The Whale and the Reactor* (1986, 142),

In a society strongly committed to capitalism as a way of life it is difficult to address the social ills of capitalist practices head on. A more subtle tack is to begin discussing urgent issues that do not appear to have any ideological valence at all. In pointing to this strategy I do not wish to dismiss any of the claims made by these political activists or writers. But it is fair to say that this approach . . . is another example of a conceptual Trojan horse. If it were effective to tackle social injustice and the concentration of economic power directly, I suppose we would do it more often.

Anyone familiar with Winner's work knows that he does not write as he does to undermine environmentalist arguments. Neither do we, necessarily, although we are certainly less sympathetic to certain aspects of the new environmental movement than he is.

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Notes

1. We do not say that environmental beliefs are "perhaps" sincerely held. We say that they are sincerely held. Here, as elsewhere, Helm does not accurately describe our argument.

2. They still do, even after Chernobyl (*Media Monitor* [Washington: Center for Media and Public Affairs] May 1987, pp. 1-2).

3. The Lichters' Center for Media and Public Affairs is presently conducting an in-depth analysis of the issue.

4. For an extensive survey of the literature on the influence of the media, including the Lipset and Schneider 1987 study, see Lerner and Rothman n.d. We do not characterize journalists as being on the "Left." We do offer evidence that they are liberal and more likely to trust radical scientists than to trust the scientific establishment.

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5. Helm erroneously cites Rothman's earlier work (1970) to support his case, which it does not. (See Rothman, Scarrow, and Schain 1976, 121; see also Noelle-Neumann 1984.)

6. The outlets studied were *Welt*, *Frankfurter Allgemeine Zeitung*, *Süddeutsche Zeitung*, *Frankfurter Rundschau*, *Spiegel*, and *Stern*.

7. See also Rothman 1984.

8. Helm quotes us as saying that the nuclear energy issue became a surrogate for the failures of left-wing politics. Our point is rather that given the hegemony of liberal capitalism in the United States, there has never been a successful radical movement. The radical movement didn't fail, it never got off the ground.

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Forthcoming in December

The following articles and research notes have been tentatively scheduled for publication in the December 1988 issue:

Charles J. Barrilleaux and Mark E. Miller. The Political Economy of State Medicaid Policy.

Courtney Brown. Mass Dynamics of U.S. Presidential Competitions, 1928-1936.

Gregory A. Caldeira and John R.

Wright. Organized Interests and Agenda Setting in the U.S. Supreme Court.

John S. Dryzek and Stephen T. Leonard. History and Discipline in Political Science.

William A. Galston. Liberal Virtues.

Ronald Inglehart. The Renaissance of Political Culture.

Mark S. Kamlet, David C. Mowery, and Tsai-Tsu Su. Upsetting National Priorities: The Reagan Administration's

Budgetary Strategy. A Research Note.

Patrick J. Kenny and Tom W. Rice. Presidential Prenomination Preferences and Candidate Evaluations. A Research Note.

Robert C. Luskin and Carol Cassel. Simple Explanations of Turnout Decline. A Research Note.

Terry Moe and John E. Chubb. Politics, Markets, and the Organization of Schools.

John D. Robertson and Marick F. Masters. Class Compromise in Industrial Democracies.

Lawrence S. Rothenberg. Organizational Maintenance and the Retention Decision.

Arlene W. Saxonhouse. The Tyranny in the World of the Polis.

H. P. Young. Condorcet's Theory of Voting.

REVIEW ESSAY

SCHOLARSHIP ON PRESIDENTIAL SELECTION IN THE UNITED STATES

BYRON E. SHAFER
Oxford University

Media and Momentum: The New Hampshire Primary and Nomination Politics. Edited by Gary R. Orren and Nelson W. Polsby (Chatham, NJ: Chatham House, 1987. 200p. \$20.00, cloth; \$11.95, paper).

A Horse of a Different Color: Television's Treatment of Jesse Jackson's 1984 Presidential Campaign. By C. Anthony Broh (Washington, DC: Joint Center for Political Studies, 1987. xiv, 93p. \$7.95, paper).

Selecting the President: The Nominating Process in Transition. By Howard L. Reiter (Philadelphia: University of Pennsylvania Press, 1985. xviii, 192p. \$22.50, cloth; \$8.95, paper).

Presidential Selection. Edited by Alexander Heard and Michael Nelson (Durham, NC: Duke University Press, 1987. x, 413p. \$50.00, cloth; \$17.95, paper).

News That Matters: Television and American Opinion. By Shanto Iyengar and Donald R. Kinder (Chicago: University of Chicago Press, 1987. vii, 187p. \$22.50).

The arrival of another presidential year is an opportunity to investigate the progress of scholarship on presidential selection. It is also a harbinger, whatever that verdict, of a spate of additional works on the topic. The basic phenomenon that guarantees both this opportunity for reconsideration and this promise of additional scholarly work, however, is not so much the recurring event itself, the politics of presidential nomination and election. Rather, it is the fundamental and apparently reliable *unpredictability* of that politics. This unpredictability is, of course, an implicit warning to anyone undertaking such a review: what we think we know may shift with alarming speed here. Nevertheless, such a review still inherently represents a nested set of opportunities. It is an introduction, first and foremost, to a group of individual books. It is a chance, through them, to examine the general state of knowledge in the field. It is, inherently and simultaneously, an opportunity to

examine the way in which a scholarly community has gone about the task of developing that field. It is, as a result and finally, an invitation to look at the degree to which practical politics, scholarly work about it, and the overall organization of that scholarly work have come to cohere in a reasonable whole.

The normal scholarly urge toward systematic understanding—in this case, the search for recurring factors that shape the politics of presidential selection—has in fact characterized most of the work in this burgeoning field, as it has certainly characterized each of the scholarly works that are the specific impetus to this review. Yet it is an even more pervasive sense of unpredictability, of a new political world that is both different and *recurrantly different*, that ultimately impels this particular scholarly quest. The usual dividing line for this new and insufficiently explained politics is the period between 1968 and 1972, as the origin of a set of extensive formal changes in the procedures

of presidential nomination, which have ramified (and ricocheted) across the ensuing years. Yet many scholars have come to see these reforms as themselves the product, and then the embodiment, of other large changes in the society around them, changes that might not have featured as precise a breaking point but which certainly promised a very different sort of presidential politics, in and of themselves.

In any case, when a rapidly changing society meets a reformed institutional structure, initially unpredictable outcomes are almost guaranteed. And unpredictable outcomes there certainly were: the McGovern nomination, the Nixon landslide and its unhappy aftermath; the Ford presidency, the Carter nomination, and its surprising result; the Kennedy challenge, the Reagan nomination, the Reagan presidency and its alleged revolution—these are only the most stark summary events in a chain of major and minor surprises. More to the point, this tendency toward what might be called “microsurprise” has not, to all appearances, abated. As this is written, presidential 1988 shows every indication of taking an equal place on the chain, suggesting—perhaps superficially?—that the institutionalization of unanticipated consequences is the defining characteristic of the new era in presidential politics. At this peculiar level of analysis, then—the level involving the prediction of precise twists and turns within a complex, diverse, and amorphous national process—political scientists have probably been no more prescient than journalistic commentators or the intelligent lay public.

Where they have had a comparative advantage, instead, has been in two other realms. The first is in possessing an intellectual tradition that reduces the onus of an inability to predict precise (sub)outcomes. From this perspective, a composite social system with numerous influences on a given result—indeed, numerous partially independent influences, which vary

both in relevance and in strength—*should* put inherent limits on the specificity of prediction. Such a system can still be analyzed and explained satisfactorily, through its composite elements—while defying, in principle, precise prediction. The second advantage is in belonging to a larger professional community that could go to work on the diverse elements of apparent anomaly in a new presidential politics and attempt to make those elements individually comprehensible in a systematic fashion. The five works at the center of this review essay are all a product of that orientation, by that professional community. Each in fact proceeds by isolating particular aspects of the politics of presidential selection, trying to treat them systematically, and trying to use that treatment to gain insights on the larger whole.

In *Media and Momentum: The New Hampshire Primary and Nomination Politics*, Gary R. Orren and Nelson W. Polsby have collected essays organized around a particular and strategic contest site, and have used that site to integrate diverse observations about the politics of presidential selection. In *A Horse of a Different Color: Television's Treatment of Jesse Jackson's 1984 Presidential Campaign*, C. Anthony Broh has focused instead on a particular candidate as he appeared in network television news and has used the distinctiveness of the resulting portrait to comment on various facets of the process. In *Selecting the President: The Nominating Process in Transition*, Howard L. Reiter has sought instead to survey major elements of the nominating process, both formal and informal, so as to get at an array of central products. And in *Presidential Selection*, Alexander Heard and Michael Nelson have tried to isolate various subaspects of nominating politics, assign each to individual authors, and reassemble the product, side by side, in a single volume. How have they, collectively, done?

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In formal terms, U.S. presidential nominations are accomplished through a succession of state primary elections and state party caucuses, in which the outcome of preceding contests is likely to have major impact on those which follow. *Media and Momentum*, conceived, assembled, and edited by Gary Orren and Nelson Polsby, focuses on a key point, the New Hampshire primary—disproportionately the key point, as many of the authors note—to illuminate the larger dynamic and its products. Data on demographic and political change in New Hampshire, in three of their chapters, emphasize the importance of *sequence* by showing its continuing influence under changed social conditions. Extended analyses of press coverage, at the center of three other chapters, explain the *means* by which a key early placement in this sequence attains its influence. The longest and most ambitious chapter then moves on from New Hampshire to the process as a whole. The success of this volume in using New Hampshire as a “window” on the larger politics of presidential selection is such that it may well spawn successors—perhaps on the Iowa caucuses and nominating politics, the southern regional primary and nominating politics, and so forth. If these meet the same standard, they may rightfully be welcomed.

In informal terms, U.S. presidential nominations are also powerfully shaped by the available field of candidates for the total contest and not just by its sequence of its events. Different candidates mix different constituencies and issues—not to mention different skills—in different ways; nothing can guarantee a particular sort of field in any given year. *A Horse of a Different Color*, from Anthony Broh, focuses on one of these candidates, Jesse Jackson, in 1984, though the analysis of press coverage presented in this short monograph features continued comparisons with the other major candidates in the Democratic field of that year as

well. Broh finds press treatment of Jackson to have been relatively even-handed and easily explained, though rather consistently undercutting his nominating potential, a perspective contrasting—on the order of the bottle being half empty versus half full—with the long essay from Henry E. Brady and Richard Johnston in *Media and Momentum*, which finds Jackson to have been overly covered when judged by horse race standards and overly well treated by comparison with the other candidates. An evident sympathy for the candidate and his larger cause, however, in no way mars the extensive tables in *A Horse of a Different Color*, which can surely be used to address the fate of other individual candidates or of a composite field.

A very useful effort to marry the formal and informal influences on presidential selection is Howard L. Reiter, *Selecting the President*. Reiter was among the first—and remains among the most determinedly empirical—to argue that recent changes in the character of nominating politics have as much or more to do with larger social changes as with procedural reforms. In this, the decline of political parties carries a heavy portion of his argument, but *Selecting the President* goes on to search for further elements of continuity (and change) in presidential nominations after that decline. The pursuit of this goal is occasionally overzealous. One need not, for example, strive to fit all of presidential politics into one among two or three integrating hypotheses: the outcome of nominating politics may well be best explained by evolutionary forces, the character of convention delegates by procedural reform. Just as one need not insist on methods in the narrow, to the point where they obscure larger explanations: rigor *can* become rigidity, and all tables are not created equal. Yet the larger continuing argument about the roots of the current process in evolutionary social developments is almost surely right—and

convincingly sustained—while there is an absolute goldmine of research and information, carefully extracted and clearly presented, in the material mobilized to support that argument. This material seems destined to be integrated quickly into our general understanding of nominating politics.

A stimulating supplement to most such work to date is the collection in *Presidential Selection*, commissioned by Alexander Heard and edited by Michael Nelson. The working papers for a study funded by the Sloan Foundation on the nominating process, these chapters have the distinct advantage of *slighting* the conventional aspects of presidential selection—rules for delegate selection, dynamics of nominating campaigns, and even patterns of media coverage—in order to concentrate on other, consequential, less frequently considered topics. Most authors explicitly share the basic premise of Reiter in *Selecting the President*, that the weakness of party-organization is central to their particular focus; though they would differ tremendously (with him and among themselves) over the virtues of the result. The most provocative of these essays of almost uniformly high quality are probably those on implications for international policy and comparative policy-making, from Ralf Dahrendorf, Ernest May, and Richard Rose. These add an aspect of the debate over U.S. selection procedures that is often completely absent, though they also proceed with their own somewhat ironic avoidance of the fact that European selection processes appear increasingly to follow “the U.S. model.” Not just these essays but almost all those in the volume, however, are additionally distinguished by their willingness to address available structural options to current practice, with a clear and simultaneous realization of the limits of the possible in such restructuring.

Each of these works explores, carefully

and in detail, one or more major elements shaping the politics of presidential selection. Each also attempts to use its particular approach to gain some additional purchase on the larger process. Accordingly, each implicitly proceeds on the basis of two further, larger, linked assumptions. The first is that a focus on the full and careful exploration of major *elements* within this politics is the best way to move toward a more comprehensive understanding. The second (and more directly methodological) is that the actual operation of the politics in question is subject to so many lesser but intermittently consequential influences and that these influences are so partially and loosely connected to each other as to make this the most appropriate way to proceed. While each individual author might prefer his own summary statement, then, each proceeds *as if* he were part of a larger scholarly community, the sum of whose investigations will in fact explain the phenomenon in question, just as each proceeds *as if* that phenomenon, even more fundamentally, was itself structured so as to require this approach.

If these individual authors had reason to stop and address this second and more basic assertion—that the world of presidential politics is subject to numerous, intermittently influential, only partially connected factors and that this justifies the evolved way of addressing that world—they would find that they already possess substantial theoretical arguments, from related realms, which could be mobilized to support it. Or at least the standard arguments about the distinctive characteristics of U.S. politics would serve quite well to defend this way of proceeding—indeed, to explain why it is the essential way. That received wisdom, at its most global, asserts, first, that U.S. society is characterized by multiple, overlapping, but shallow social cleavages; second, that the politics flowing from that society is characterized by weak inter-

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mediaries, and hence by segmented and evanescent coalitions; and third, that the institutions driven by these intermediaries are themselves divided, competitive, and facilitative of perpetual (re)negotiation.

In other words, an additional virtue of these individual works—from Orren and Polsby, Broh, Reiter, and Heard and Nelson—is to provide detailed instances of these larger generalizations about U.S. politics, instances characterizing the more specific politics of presidential selection. Together, for example, these books certainly convey the sense of a politics built on numerous and overlapping, but not terribly deep, social cleavages. This is, after all, why the *sequence* of individual state contests matters, why we are concerned with the recurring impact of a New Hampshire primary. The Jackson candidacy does raise the question of whether black-white divisions remain an exception to this. But apart from that potential exception, this multiple and overlapping social base is also why different fields of candidates, mixing and matching old and new issues in different ways, make presidential politics appear so continually unpredictable. Indeed, seen from a different perspective, multiple and overlapping social divisions explain why any *individual voter* is differentially responsive to different issues and different candidates in different years.

In some systems, that sort of unpredictability would be restrained by strong parties, probably with further, fixed coalitions of organized interests within them. The U.S. nominating process, conversely, underlines the weakness of parties as determining organizations and the truncated and shifting nature of coalitions among the organized interests that are simultaneously components of and direct competitors to party. Parties no longer have much formal place in the nominating process, though the Republicans do remain nominally kinder in this regard. Even informally, it is *candidate cam-*

paigns—evanescent organizations almost by definition—that serve as the basis for selection politics. To this is added multiple, segmented and fragmented, organized interests, which are encouraged to participate directly. While their participation itself is a further contribution to fluidity, even this is exaggerated by the way in which shifting *fields* of candidates, along with shifting issues of the day, can cause individual organized interests to be concentrated or dispersed in any given campaign and influential or irrelevant depending on the further drift of that campaign.

Finally, it is hard to imagine an institutional arrangement better suited to exaggerating these tendencies than the one that has evolved for presidential selection. The sequence of *open* participatory caucuses and *public* presidential primaries is ideal for reopening most issues in every presidential year—and often at several points within that year. As if that were not sufficient, the precise character and sequence of these institutions changes just enough from one quadrennium to another to guarantee some institutional arrangements with which no one has had any experience, producing some reliably unanticipated effects. And as if that were not enough, all of these institutions—participatory caucuses and public primaries—are crucially dependent on a low but shifting voter turnout, so that the electorate itself may have a noticeably different composition within the *same state* from one presidential year to the next.

A world constituted in this loosely structured and constantly shifting fashion—hence a world that must be *described* in this fashion as well—remains unsatisfactory at some visceral level for many scholars. Some of this dissatisfaction derives from the continuing absence of an ability to progress toward specific predictions. The personal identity of two nominees and then of one president, after all, is the consequential outcome—or so it

perpetually seems—and being unable to generate a theoretical framework that will reveal these identities is thus inherently frustrating. More of this dissatisfaction, however, stems from a basic scholarly dislike of situations that are insufficiently specified and insufficiently explained. The same motives that drive an individual to attempt to develop a full and proper explanation of any social phenomenon, whatever those motives may be, fuel the desire to circumvent and push beyond apparently inherent limits on that explanation. Such a resolution presents possibly insurmountable difficulties, given the evolving description of the actual structure of presidential politics, because it involves the hunt for a dominant and unifying factor—ordinarily a *single* factor, often one overlooked by others. Nevertheless, each presidential election also resurrects this particular response to the underlying mission.

By far the best known of these—and also the most theoretically developed—is the “critical election” framework, hence the search for a “realignment” in the current election cycle. If this nominating contest were to conduce toward a classical realignment, for example, and if this general election were to confirm it, then something new and different—indeed, central and dominant—would be present on the landscape of presidential politics and should be reflected in scholarly accounts thereof. Books on a given presidential year now reliably include the search for this phenomenon, and 1984 was no exception. One or more chapters address the question in *The Elections of 1984* (ed. Michael Nelson); *The Election of 1984* (ed. Gerald Pomper); *The American Elections of 1984* (ed. Austin Ranney) and *Election 84: Landslide without a Mandate?* (ed. Ellis Sandoz and Cecil V. Crabb, Jr.) (all published 1985). The bad news—the title of the last of these really summarizes the conclusions of all—is that such a classical realignment has yet to

emerge. Worse news would be a consensual victory for the view—the perspective known as “dealignment”—that U.S. politics has now changed sufficiently that such a critical, continuing shift is unlikely ever to recur.

That victory has so far not been conceded. In the meantime, other efforts to locate some dominant structuring factor outside the cluster of attitudes associated with partisanship and voting, also continue. Perhaps the most provocative of these for 1984 was Thomas Ferguson and Joel Rogers's, *Right Turn: The Decline of the Democrats and the Future of American Politics* (1986), an attempt to find the consistent, underlying explanation by looking at party financiers and “money men.” The dilemma of most such works, however, is essentially the same: In a world where a shifting variety of factors clearly does make important contributions to shaping the ultimate outcome, efforts to argue against this conclusion end up, at best, by introducing yet another factor of some consequence into the equation. At worst and all too often, they are forced instead to *assume* that the concerns associated with their focus are the central concerns of politics and that matters unassociated with that focus are (therefore) unimportant; to believe, even then, that the behavior of their focal factor is principally as initiator and influencer rather than as respondent and reactor; and thus to ignore, in effect, the context in which their explanation occurs.

A less controversial effort along these lines, however, one distinctive in both argument and approach, is *News That Matters: Television and American Opinion*, from Shanto Iyengar and Donald R. Kinder. Presidential politics, by itself, is only a piece of what the book aspires to explain, so that much of its substance should be addressed under other headings. Yet the authors believe—and provide numerous ingenious experiments to assert—that a subtle but pervasive,

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deliberate but nonconspiratorial influence by television news is a central piece of what gives (all) U.S. politics its continuing structure. The authors find this influence in the processes of "agenda setting," which focuses public attention on certain matters, and of "priming," which establishes standards of judgment for them. In this, they differ very sharply from those who find the essence of media influence in either inherent characteristics of news gathering or in explicit "bias"—categories that would include William C. Adams, Emmett H. Buell, Jr., and David W. Moore from the Orren and Polsby volume, as well as Anthony Broh in *Horse of a Different Color*. To say that they differ from these others, however, is also to say that even their ingenuity and diligence cannot entirely overcome the standard dilemma of experimental research, namely, transferability to the real world. Thus at the final grand and global level they cannot escape, for example, the problem of differentiating pure media effects, that is, those flowing from *press decisions* about coverage, from agenda setting and priming, which are only the by-product of *press coverage* of real acts and conflicts among major political actors. What they end up doing, then, in line with most others in this field, is adding yet another element to the array of those which certainly do shape the politics of presidential selection.

Is that, accordingly, as far as scholarly analysis can go? There are some basic notions that are fundamental to understanding the politics of presidential selection, in overlapping and cross-cutting cleavages, in weak and segmented intermediaries, and in semi-independent and competitive institutions. While these both advance and limit an understanding of the resulting politics, it is possible to add further specific elements *and* to explicate them systematically—as Orren and Polsby, Broh, Reiter, and Heard and Nelson have certainly done. Efforts to go beyond this

with a dominant alternative, on the other hand, as with numerous searches for a realignment, have been unsuccessful to date. Indeed, even a distinctive effort like that of Iyengar and Kinder ultimately provides only further introduction of an additional factor shaping the politics of presidential selection. Nevertheless, when all these studies are melded into the continuing *collective* scholarly effort on the topic, some additional, recurring outlines may be emerging. These build on emerging regularities in mass participation and in the dynamics of (elite) campaigns to produce a recurring factional structure. If these factions remain somewhat fluid in terms of their programs and their constituencies, they still help to constrain vague assertions about "shifting coalitions" and "programmatic flux."

An underlying pattern to the turnout in presidential primaries and party caucuses is in fact increasingly apparent, though by itself it is a very blunt instrument of prediction. Those who do come out to participate, in forums that vary in aggregate turnout from an occasional 40% or above at the high end to an occasional 5% or below at the low, tend disproportionately toward one of two categories. Either they are the most highly educated and attentive, those most likely to get there on their own, or they are the best organized (among the supporters of their party), those most fully integrated into the *organizations* likely to participate in nominating politics. This tends to divide these participants additionally, within each party, between those who are attached to the national party program as well as to its main organized interests on the one hand and those who are self-consciously independent of (and sometimes alienated from) that same policy coalition on the other—into an "establishment wing," which includes the official party and the major interests associated with it, versus a "dissident wing," which is reliably made up of those who oppose

"politics as usual" and who believe that "the special interests" (by which they ordinarily mean the establishment wing) are too influential for the public good.

The dynamics of nominating politics, as they have come to be understood, are tailor-made to convert these incipient divisions in the mass (participating) public—quickly—into campaigns centered on a specific standard-bearer for each wing. In a nationwide sequence of open primaries and the occasional key caucus, very few candidates will (indeed, can) begin with nationwide name recognition and strong voter attachment. Most candidates will be profoundly local, in the sense of not being from (or known within) the opening states. They must thus triumph in one or more of these early contests or never move to national prominence at all. The exceptions, those candidates who *are* lucky enough to begin with national name recognition, still ordinarily begin with a broad but shallow public attachment. They, too, must succeed early or fall by the wayside, though they can potentially weather an early loss or two, unlike the true unknowns. What is additionally crucial, however, is that these two inherent, structural categories of candidates tend to coincide with the divisions within the participating mass public.

That is, those in the second category, of broad but shallow initial support, are more likely to be from the establishment wing of their parties, having built up their national name recognition through an extended career in national politics and having acquired the ties to dominant organized interests that go with that career. Those in the first category, conversely, of little or no national name recognition, tend to be from the potential dissident wings, else they would already have been crowded out by those (establishment) figures with their extended careers and national ties. This, of course, explains why it is harder to gauge the identity of dissident standard-bearers than of establish-

ment champions in advance. More to the point, however, these simple factors—this gross outline of mass participation and this crude summary of elite dynamics—explains not only why there is a recurring factional structure within each party, one that belies some of the appearance of flux and disconnection, but also why the two parties are roughly mirror images of each other.

At bottom, ordinarily, there are *two* initial contests within the nominating politics of *each* party, with certain reliable contours to them. There is the contest to secure leadership of the establishment wing of the party, and there is the contest to secure leadership of the dissident wing. Occasionally, one candidate can blend support so successfully—or one contest is effectively won by so dominant (or weak) a contender—that this settles the nomination. But ordinarily two factional leaders emerge and then duel for that nomination. Indeed, it is possible to put some detail into their programmatic positions, too. The division between establishment and dissident in fact tends not to be over economic policy, where Republican contenders are reliably conservative, Democratic contenders reliably liberal. Instead, the division tends to be over cultural and life-style issues—over what are known in the U.S. as "social issues"—where establishment contenders tend to be moderate, while dissident champions lean toward the extremes (left for the Democrats, right for the Republicans). Finally, it is possible to specify this factional situation even further by noting that it is not reliably a 2-faction but rather a 2½-faction contest, again in both parties. Or at least the possibility of a secondary conservative insurgency within the Democratic nominating contest or a secondary liberal insurgency within its Republican counterpart is still present, though these are no longer reliable occurrences and do not reliably attain the level of expression that they still secure in congressional elections.

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The pattern to these factions is not sufficient to provide hard and fast, prior expectations for the precise twists and turns of nominating politics. Yet this factional structure does help additionally in explaining an even larger element of regularity in presidential politicking, the process by which individual contenders for the nomination can take on the dominant concerns of the mass public during the course of nominating politics—can *find* both their message and their constituency along the way, being themselves reshaped into something rather different from what they may originally have been. Factions, accordingly, provide a way of making some further sense of new presidential contests, a sense larger than the usual “If X does well here and Y does well there, then this will happen, but if Y does well here and Z does well there, then *that* will happen”—an inherently unhappy form of (non)explanation. On the other hand, even the use of recurring factions merely gives shape to—and constrains—

the continuing fluidity in presidential politics.

The background to that politics—multiple and overlapping social divisions, weak and truncated intermediaries, and an institutional structure ideal for forcing constant renegotiation—is still its dominant factor. It would be too much—and far too uncomfortable—to call the resulting malleability the “genius” of U.S. politics, since that very malleability also makes every analyst nervous from time to time. Yet to call it the “essence” of that politics may not be too strong. As a result, a scholarly community working on that politics will still have to attend principally to this background, by isolating significant pieces and giving more systematic attention and elucidation to them. In that way, this scholarly community should continue to make a collective contribution to understanding, by continuing to respect the essential character of the phenomenon under study.

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POLITICAL THEORY

Critical Encounters: Between Philosophy and Politics. By Fred R. Dallmayr (Notre Dame, IN: Notre Dame Press, 1987. xi, 296p. \$28.95).

Reviewing this book presents me with a delicate task. The subtitle is (purposely) the title of my recent book, and the introduction pointedly addresses my claims about contemporary academic political theory's estrangement from the particularities of politics and about the problems associated with its absorption in issues defined by academic philosophy. I have great respect for Dallmayr's work, and I share many of his theoretical interests and assumptions. But as he recognizes, this volume is quintessentially representative of the genre that I criticized in *Between Philosophy and Politics: The Alienation of Political Theory* (1986).

The collection of 11 essays, with the possible exception of those devoted to Nietzsche (as interpreted by Jacques Derrida and Heidegger) and Theodor Adorno (approached in terms of Heidegger), consist of critical explications of the work of contemporary academicians in the fields of social and political science and philosophy (Jürgen Habermas, Karl-Otto Apel, Hans-Georg Gadamer and Jacques Derrida, Richard J. Bernstein, Alasdair MacIntyre, Michael Theunissen, Anthony Giddens, and Niklas Luhmann).

There are no grounds for disagreeing with the prepublication assessments that Dallmayr is a "trustworthy interpreter" who is "very much in touch with the principal currents of contemporary thought"—Anglo-American and Continental—and who "has the extraordinary ability to engage in sympathetic critical dialogue" and to communicate "the perspective of those he interprets with . . . clarity and charity." With respect to authors such as Theunissen and Luhmann, who have been less accessible to an English-speaking audience, the accomplishment is particularly valuable. The reviewers, however, wish to stress that more

has been accomplished than faithful rendition embellished by critical analysis. Dallmayr's own position "becomes more sharply defined," and "his own voice reaches a new level of definition."

Dallmayr is sensitive to suggestions that his past work has been marked by "a talent for empathy" that has "virtually submerged [him] in interpreted texts" and that was "thus purchased by lack of a clearly defined 'standpoint'" (p. ix). He notes, however, that this project does not "focus" on producing such a standpoint; even though the essays, like his other recent work, are the product of his own attempt "to chart a course leading from critical phenomenology (or critical hermeneutics) in the direction of what [he] came to call 'practical ontology'"—through a context of "critical theory, transcendental hermeneutics and neo-pragmatism" (p. ix). He is especially concerned with issues generated by varieties of post-modernist philosophy and with the implications for social thought of such philosophical events as the "decentering of the subject" or the loss of the idea of an autonomous individual as the basis of action and judgment (p. 2).

What Dallmayr's essays have to do with politics, except in some very abstract or historically remote sense, is consistently opaque. He claims to deal with authors "whose arguments capture and pinpoint the salient issues of our age"; but those issues turn out to be philosophical in the narrowest sense of that term. It may be true that "the intellectual [maybe more precisely, 'academic'] agonies of our time are unintelligible without attention to phenomenology, hermeneutics, linguistics, critical theory, deconstructionism, and various forms of pragmatism" (p. x). But the "agonies" discussed and the "agonism" practiced by Dallmayr are only by occasional allusion connected to concrete past and present forms of political thought and action.

A prominent political theorist once responded to this kind of criticism by saying "after all we are philosophers, and it is hardly

our task to say how much money a welfare mother should receive." One may agree—but not necessarily for the reason implied. The problem, for example, of how to manage solid waste may be beneath the dignity of philosophy, but it may also be beyond its competence and authority. What exactly has philosophy to do with politics? The answer need not be "nothing"; but the problem, both descriptively and normatively, is a complex practical and historically situated one that depends on the character of both politics and philosophy. It does not admit to a general philosophical solution. Try to explain to a person providing support services for AIDS patients or someone working on agricultural development in an African village that the agonies of our age revolve around the decentering of the subject.

Dallmayr's notion of "ontological praxis" may suggest something paradigmatically oxymoronic, but it is intended to convey an idea of philosophy with a substantive and practical bent. His claim, however, that this kind of "lived engagement" is not "removed from 'real-life' occupations" and does not merit "the verdict pronounced by John Gunnell on most versions of contemporary social and political theory" (p. 3) is less argued than asserted. He employs my words to indicate political theory's problematical situation when he refers to his essays as inhabiting "the curious demimonde" between philosophy and politics. But his critical encounters do not take place in that realm. They move quite distinctly within the conceptual world of professional academic discourse and among arguments that are for the most part equally distanced from political life.

Dallmayr incorrectly claims that I presuppose a "neat segregation between philosophy and politics," present them as "opposed to one another (along a scale of authenticity)," and intimate "having access to, or being in command of, a basically unalienated, authentic, or genuine mode of political discourse" (pp. 3-4). This kind of misreading prevents bridging the issue that divides us. I posed no such necessary separation or invidious comparison but rather stressed the practical fact of distinction, the tendency of political theory to assume that philosophical discourse is in some way identical with political discourse, and its failure to confront its own situation and the existential

complexities of the relationship. My claims about the alienation of political theory were not predicated on access to any privileged discourse. On the contrary, my most persistent critical argument focused on the manner in which various forms of academic political theory have looked to philosophy for a basis of transcendental political judgment.

It may well be that political theory as a professional academic pursuit should no more be expected actually to speak to and about politics than, for example, the philosophy of science speaks to and about the everyday activity of science. Its alienation is less a function of its differentiation and esotericism than of its pretension to both practical and theoretical involvement. *Wir Gelehrten* need not be ashamed of who we are, but we must reflect authentically on our identity and its possibilities.

JOHN G. GUNNELL

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Feminism and Political Theory. By Judith Evans et al. (Beverly Hills: Sage, 1986. vi, 155p. \$28.00, cloth; \$12.95, paper).

Feminism and Political Theory, a group of Political Studies Association conference papers, exhibits the strengths and weaknesses of the conference paper genre. On the one hand, the reader gains a sense of what passed for state-of-the-art thinking on a particular delimited topic at a given time and place and in light of an articulated agenda. On the other, essays of this sort are often highly condensed, with assertion frequently substituting for fleshed-out argument, and quickly dated. This is especially true in a field that is as rapidly developing and immodestly encompassing (purposely so) as feminist political theorizing. A volume dedicated, as this one is, to a specific political purpose—legitimizing feminism as a subject of study in British political science—may also lose, over time, some of the energy that initially made the subject matter exigent.

A warning, then: do not expect too much. If the reader is looking for new approaches or provocative alternatives to either mainstream (or "malestream" as several authors insist on putting it) theorizing, or by-now established feminist theorizing, he or she will be disap-

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pointed. If, however, the reader approaches the volume to gain some sense of how various debates are couched within British feminist academic circles, and how texts by U.S. feminist writers, academic and nonacademic, are, or have been, appropriated within the British milieu, the essays offer helpful summaries and provide a decent map to the discursive landscape.

All the authors share an animus against the New Right, not so new anymore and never as cohesive an entity—either here or in the United Kingdom—as the contributors suggest. All oppose liberalism, claiming that it has “little to offer the feminist cause in that its values of autonomy, tolerance and the preservation of the private realm lead to a contradiction which becomes for feminism an impasse.” And, as the quotation in the previous sentence indicates, all yearn for a world in which tensions are eased and contradictions eradicated. None questions whether this is or should be an aim either of politics or theorizing about politics.

Judith Evans, in her essay, “Feminism and Political Theory,” makes, but does not defend compellingly, the claim that “the essential techniques of political theorists are value neutral,” a curious avowal that locates “bias” in the assumptions of theorists but not in “techniques which they employ.” What these “techniques” are and whether there is some generic technique of the value-neutral sort shared by all political theorists is never clarified. What appears to be at stake here is Evans’s desire to distance herself from a feminist viewpoint she opposes, one that “demands the dethroning of logic and reason.” As there is no serious feminist theorist I know who promoted decapitation of the thinking portion of the body, human or political, her attack is oddly without a target save for thinkers who are quite marginal to serious feminist political theorizing. Her larger point—that feminist theorizing deserves to be considered by political theorists more generally is, however, well taken.

Ursula Vogel details the different strategies for women’s liberation that emerge from what she tags the rationalist and romanticist traditions. Vogel leaves open the possibility that a thinker might “acknowledge specific, gender-based characteristics of reason without thereby implying the superiority or inferiority of one against the other.” Her subsequent defense of

Friedrich Schlegel’s celebration of Greek exemplars of strong womanhood, including Spartan women, against Rousseau is a bit odd, however, in light of the fact that Rousseau valorizes the Spartan mother who loses five sons in battle but gives thanks to the gods for Sparta’s victory, as the exemplary female citizen. Why Schlegel’s exaltation of Spartan mothers is recommended to feminist thinking but Rousseau’s is anathema is unclear. Karen Hunt takes a look at “the socialist construction of women’s politicization” with especial attention to Britain’s Social Democratic Federation and offers a useful historic and rhetorical summary of debates internal to socialists about the “woman question.” “Those of us who are socialists and feminists,” she insists, require “a more analytically persuasive means of cementing the two analyses.”

Tessa ten Tusscher, in a short piece on three big topics—patriarchy, capitalism and the New Right—throws down the gauntlet with abandon, claiming that “feminist political theory is only of value if it is of use to the political project of ending women’s subordination.” In an essay attacking the New Right for its narrow moralizing, ten Tusscher’s tendentious approach to thought is troubling and very much a discursive double to what she opposes. She and the New Right, as she depicts it, enlist ideas and give them their marching orders with specific aims in mind. Ideas that do not serve are to be jettisoned. But she does offer entrée into the British debate over Thatcherism for the uninitiated.

Georgina Waylen, in “Women and Neo-Liberalism,” targets neoliberalism as “the antithesis of the ideas of the women’s movement,” with Milton Friedman and Frederick Hayek singled out as strong exemplars of a mode of thinking that leads to “the intensification of the subordination of women.” Evans weighs in with a second contribution on “Feminist Theory and Political Analysis” that urges total abolition of the public-private distinction, without bothering to look at potentially problematic implications. One U.S. example: in the United States abortion rights flow directly from the assumption of a zone of privacy. *Roe v. Wade* builds on *Griswold v. Connecticut*. Finally, Elizabeth Meehan’s survey, “Women’s Studies and Political Studies” is rather dated in terms of the literature she draws upon, but she does use-

fully encapsulate debates over whether women's studies should be an integrated, or a "separate," enterprise.

JEAN BETHKE ELSHTAIN

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The Philosophical Discourse of Modernity: Twelve Lectures. By Jürgen Habermas. Translated by Frederick Lawrence, with an introduction by Thomas McCarthy (Cambridge: MIT Press, 1987. xvii, 430p. \$27.50).

The phalanx of critical modernity has taken up its position on the beach, engaging the forces of postmodernity as they swarm ashore. The threat must be met and driven back to its barbarian homeland. The rather staid title of this impressive book belies its drama, polemics, and philosophical power. We are lead on a direct charge into the heated issues of modernity and postmodernity. The difficulty of surveying such a significant work in a brief review is eased somewhat by Thomas McCarthy's excellent introduction which lucidly summarizes the key themes. I will thus only give a sense of the overall thrust of the book from two perspectives: its place within the context of Habermas's work as a whole, and its attack on those who threaten the "discourse of modernity."

With the translation of this book, as well as that of the second volume of *The Theory of Communicative Action* (1988), the project Habermas has been pursuing since *Knowledge and Human Interests* becomes fully available to the English-speaking world. Its theoretical heart is a "communicative" conception of reason and action that claims to be distinctive enough to (among other things) withstand the various critiques leveled against reason in its modern form: the reflective calculations of the individual or collective subject. The communicative model claims to be able to perform this feat because it grounds itself in neglected potentialities of the life-world structures of modern culture, and yet also generates a perspective from which modern society and its prevailing conception of reason can be critically assessed. Habermas thus offers us a way of understanding modernity that is critical but that nevertheless develops its tools of critique

from the materials of modernity itself. This ambitious undertaking is carried out in *The Theory of Communicative Action*.

Implicit in such a task, however, is a rejection of attempts to generate more radical distance from modernity and its dominant concepts, attempts that can be loosely gathered under the umbrella term *postmodernity*. Habermas leaves no question here as to who his main opponents are: Derrida and Foucault and their forerunners, Nietzsche and Heidegger. Each claims to have developed a mode of thinking that somehow lifts their critical perspectives out of the "discourse of modernity" (pp. ix, 74); something they see as necessary to getting free from the entanglements of modern subjectivity and its attendant form of reason. Against this position, Habermas sees his goal as twofold: first, to reconstruct the philosophical discourse of modernity historically so that both his own position and that of the radical critics can be better understood and evaluated and second, to show that these critics do not achieve what they intend.

The philosophical discourse of modernity starts, according to Habermas, with Hegel, not because he is the first modern thinker, but because he is the first to take modernity as the central theme of philosophy. This thematization takes the form of asking how modernity can achieve "self-reassurance" (p. 16). The question becomes pressing when, in the wake of the Enlightenment, the key principle of subjective reason—which brings the achievements of modernity—increasingly falls under the suspicion of being corrosive of the unity of any possible form of social life. The problem, then, for modern philosophy is to show how to combat this corrosiveness and yet do so in a fashion that does not regress behind modernity, that is, in a fashion in which modernity still "creates its normativity out of itself" (p. 7). Hegel's solution to this problem—absolute subjectivity—is found wanting; but Habermas takes as definitive his statement of the central "theme" of the discourse of modernity, as well as of the "rules within which the theme can be varied" (p. 51). The bulk of the book surveys the ways in which the radical critics have tried and failed to bid farewell to this discourse. In the end, their approaches simply do "not seriously lead beyond the philosophy of the subject." That breakthrough can only be achieved

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by a decisive shift from "the paradigm of consciousness" and subjectivity to "the paradigm of mutual understanding" and intersubjectivity, a shift that at the same time respects the theme and rules of the discourse of modernity (pp. 74, 296).

Given the level of analysis and the chosen "rules" of discourse, Habermas's rebuttal of the critics is persuasive. Yet one feels just a bit uneasy gloating on the beach with the victorious Greeks. Are there perhaps ways of engaging these critics in which they are less easily disarmed? Let me simply allude to one. As is made clear in Habermas's discussion of Derrida, the communicative model of reason and action emphasizes the action-coordinating function of language over the "world-disclosing" function stressed by the critics (pp. 190-210, 329-35). Among other things, Habermas suggests that systematic social theory and political philosophy will not be plausible if we take the critics' position. In this I think he is essentially right. But what does one then do with the world-disclosing dimension of language? Elsewhere, Habermas has tried to assign this dimension a kind of derivative status, but the attempt is rather unpersuasive, (as is the reverse strategy of the critics).

My underlying concern can now be stated as a question: What would it mean for Habermas, political reflection, and the discourse of modernity as a whole to give neither dimension of language such a strongly preeminent status? In order to think this question through, we may have to invite the barbarians ashore again.

STEPHEN K. WHITE

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Power and Marxist Theory: A Realist View.

By Jeffrey C. Isaac (Ithaca: Cornell University Press, 1987: x, 238p. \$24.95).

As much as political scientists bemoan the quagmire called the power debate, they keep coming back and asking epistemological and ontological, if not methodological, questions. It has become de rigueur that each study of power also be a study of how to study power. This book is no exception and even offers a spirited defense of the necessity of such self-

reflective work, especially in the case of power. This is an ambitious book that seeks to pose a serious challenge to the existing state of affairs in the debate over power. Its goal is nothing less than showing how all the combatants in the "three-faces-of-power" debate share a consensus on a Humean view of causality, a nomological understanding of science and an empiricist concept of power. The power debate is trapped in empiricism; and as long as it is mired there, it will not be able to make much sense of power. In lieu of empiricism, "realism" offers a better possibility for establishing a science of power, and Marxism constitutes just such a realist approach to power.

Realist science seeks to specify the real character of things in a way that informs us of their enduring capacities. It does not assert the claim that it can specify the character of things in an absolute sense or that the real is directly experienced as things that exist independently of the knower. It also stresses the idea that while things possess enduring properties, things never exist in their pure form and that the extent to which they exhibit these characteristics is contingent upon how they play out in the real world in any particular instance and at any particular time. Realist science, however, does emphasize that the world consists of things rather than events. From this perspective, science is concerned essentially with what kinds of things there are and is only derivatively concerned with predicting whether events will happen. And while it recognizes that theories may be *underdetermined* by facts, it stresses that they are not *undetermined* by them. Marxism is a realist science in that it seeks to specify the enduring properties and character of capitalist regimes *per se*, recognizing that such regimes will take various social formations in various historically specific circumstances.

Realist social science differs from its empiricist counterpart especially as exemplified by the problem of power. Empiricism focuses on behavioral regularities to the neglect of structure. For instance, all parties to the debate over power fail to account for how structure implies the capacity to exercise power over one who assumes a certain role within society—be it master or slave. In what is bound to be a controversial analysis, Isaac shows how even scholars like Steven Lukes, who argue for tak-

ing structure into account as "the third face of power," fail to specify how structure entails power because they are trapped by empiricism into seeing power in strictly behavioristic terms. Lukes emphasizes behavior over structure because power is a concept of human agency and structure implies determining limits on action. Relying heavily on the work of Anthony Giddens, Isaac suggests that the distinction between structure and agency is overdrawn. Instead, we should recognize "structuration," or the dialectic process by which structure creates the potential for agency and agency renegotiates the conditions for structure.

Empiricism promotes a behavioristic, sequentially causal view of power as "power over" to the neglect of "power to," as in the enduring potential or capacity inherent in a social role. Just as realist science studies enduring capacities of things, the realist concept of power focuses on the potential for, and limits on, power inherent in any social role. The ability to realize such "relational" power is contingent on how social actors negotiate within their structured circumstances. Structure creates the potential and limits for the exercise of power, and agency reproduces and remakes structure. Only such a realist view of power can account for structure and agency and get us beyond the limitations of the power debate in its current state.

For Isaac, Marxism as a realist science offers just such a theory of power; and he tries to convince us of that by reviewing debates over Marxism on power, class, and the state. In the process, he deftly defends a realist Marxist perspective on power from a diversity of criticisms inside and outside of Marxism. There is much to interest readers here in thorough reviews of the debates about the deterministic character of Marxist structuralism, the meaningfulness of class as a way of understanding human struggle, the greater utility of the "state" as compared to "government" for understanding power, the character of the capitalist state, and poststructural criticisms of the totalizing and reifying tendencies of Marxism. What emerges is a nonreductionist Marxism that forgoes economic determinism and recognizes that power relationships grow out of the structure of society. While some of this gets far afield of the original problem of the debate over power, in the end a contemporary

neo-Marxist understanding of power comes through effectively.

This is an important book. It is a thorough, readable, and scholarly work that raises fundamental questions about the contemporary power debate and in so doing poses a meaningful alternative that will not be easily dismissed.

SANFORD F. SCHRAM

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The Critique of Pure Modernity: Hegel, Heidegger, and After. By David Kolb (Chicago: University of Chicago Press, 1986. xvii, 316p. \$25.00).

During the past decade the currents of European postmodernist thinking have slowly worked their way into the U.S. academic world, initially through the humanities and more recently in the social sciences, with political science one of the last to be affected. Meanwhile in U.S. philosophy, a native but similar movement, antifoundationalism, has grown up as a speculative alternative to traditional metaphysics and to nonmetaphysical analytic philosophies. The two tendencies were brought together in Richard Rorty's *Philosophy and the Mirror of Nature* (1979), which has become the prime focus for discussion of U.S. postmodernism. Now David Kolb, professor of philosophy at Bates College, has presented his contribution to the project of "overcoming modernity," following in the antifoundationalist line but drawing upon continental European thought as his subject matter.

Kolb's commentary on Hegel and Heidegger and his own theory of modernity are of interest to political theorists and to reflective political scientists in specialized branches of the discipline because they center on the possibility for constituting a coherent way of common life under contemporary conditions. Both Hegel and Heidegger, according to Kolb, try to get beyond the standard Weberian description of modern society as a conjuncture of vacant willing subjects standing over against a neutral field of fact by enfolding human beings into a finite context. In Hegel's case that context is the circular motion of spirit running through its various dialectical moments; and in Heidegger's

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ger's it is the "propriative event," that is, the occurrence in which man and beings are gathered together in distinctive relations that constitute historical epochs. Kolb finds both German thinkers to be inadequate, but on different grounds in each instance. Hegel is successful in combining multiplicity and unity on the level of speculative thought, but he is unconvincing in demonstrating a tight relation between his dialectic and the various details of historical development. Heidegger, in contrast, provides too much substantial unity in his account of history, destroying continuities by setting up hermetic epochs constituted by invariant relations of man to beings. Both, however, have the same worthy project, which is to reduce the isolation of modern subjectivity without returning to the relative deindividuation of traditional societies:

Kolb's commentaries are done carefully and responsibly, evincing conversance with contemporary scholarship and a broad and fine understanding of the original works. He presents a strong, though not thoroughly convincing, picture of Hegel as a transcendental philosopher, rather than a metaphysical idealist, and he gives one of the more lucid and accessible analyses of Hegel's logic in the literature, showing how it makes the political works intelligible. But the high point of the commentaries comes in his elucidation of Heidegger's term *das Gestell*, which refers to how beings are "presenced" to man in the current technological age. Here Kolb shows his virtue of clarity to best effect, making difficult and significant texts readily understandable without sacrificing subtlety. The three chapters on Heidegger, which emphasize his late work, offer a gateway for political scientists unfamiliar with postmodernism to grasp some of its basic insights.

The strengths of Kolb's discussion lie with his skill as a lucid and responsible interpreter and scholar. He has mastered his subjects in a technical sense, attending to richness and variation in meaning, disentangling complexities, and keeping the wider structure of thought in mind as he works through each part. But his power of mind is not matched by his sympathy and depth of understanding, particularly when he encounters Heidegger. Indeed, he fails to grasp how seriously Heidegger took the turn in modernity through which "everything faces everything else as ready for

ordering and use," and "men too belong as standing reserve ready for human engineering" (p. 147). Here, in the thought that "every entity is encountered as open and pliable for an ordering that proceeds without any center from which an ordering force radiates" (p. 147), Heidegger anticipates such poststructuralists as Jean Baudrillard, who argue that our activities and meanings have lost reference to any primary reality. Heidegger found "universal imposition" to be a cause for profound concern, but Kolb does not even seem to sense that concern.

Kolb's lack of depth and, indeed, seriousness comes clear when he offers his own pluralistic and antifoundationalist interpretation of life, which does not overcome modernity but loosens it so that experience resembles William James's pluralistic universe. Without any hint of ground or justification Kolb expresses the democratic and reformist sentiments typical of the U.S. pragmatic tradition. Political scientists will recognize his description of life as a naive version of the one Arthur Bentley presented in *The Process of Government* eighty years ago—only now we are playing a "game" which we must be sure not to take "perfectly seriously" (p. 251). Kolb's commentaries are admirable in a scholarly sense, but he failed to gain enough substance from them.

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Human Rights and Human Diversity: An Essay in the Philosophy of Human Rights.

By A. J. M. Milne (Albany: State University of New York Press, 1986. i, 186p. \$39.50, cloth; \$12.95, paper).

Locating a value-neutral basis for a concept of human rights and then defining the minimum essential rights within the concept may be an insuperable task. This frustration arises from value preference (bias), from lack of cultural relativity, from disregard for advances in science and understanding, and from proneness to deductive-prescriptive methodology. Milne slashes at these veils with the earnest iconoclasm necessary to make a real advance in the literature. The result is a coherent basis and architecture for conceptualizing human rights. This achieved, others may

build upon Milne's concept in the operational sense for the tangible betterment of human rights. In these respects, Milne's essay merits serious reception among the disciplines of law, philosophy, government, international relations, anthropology, sociology, and comparative religion-comparative ideology.

Milne begins his search for the basis of human rights with Kant's humanity principle but finds that it suffers from the same perceptual frustrations mentioned above. He therefore grafts upon Kant's humanity principle two moral principles of his own ascertainment. These are the moral principles of beneficence (it is natural to choose good over evil) and of practical reason (which requires that like cases always be treated alike, with differentiated treatment only where there are relevant differences). These two principles constitute the basis of a "common morality which is not further reducible and permit the concept of community to be actualized. Owing to contextual circumstances, each of these communities (the term is taken broadly) evolve their own 'particular morality'" (p. 82). It is through this particular morality of communities that the irreducible common morality attains its diversity. It is precisely this diversity, according to Milne, which must be taken into account when resynthesizing the minimum human rights located in common morality. Milne finds these resynthesized human rights located in common morality to be the right to life and the right to justice in the form of fair treatment, these being morally prior to all the rights which follow. Following from the right of justice in the form of fair treatment are the rights of fellowship (the right to aid), of social responsibility, of freedom from arbitrary interference, of honorable conduct, of civility and of child welfare. Milne sees no possibility of community without these, only the sort of poor, nasty, and brutish life possible in a Hobbesian state of nature.

Milne's contribution to the multidisciplinary literature pertaining to human rights is his rigorous explication, in terms of philosophy, of the content of each of these resynthesized rights. In this he strives conspicuously and admirably, because he is trailblazing, to avoid philosophical self-impeachment. Transcending wishfulness, Milne resorts with rationally persuasive results to the philosopher's inductive methodologies, enhanced by falsification tech-

niques. To this he adds somewhat awkwardly the right-obligation correlation analysis articulated early in this century by the English legal philosopher Hohfeld. Further in the name of cultural relativity, Milne disengages his common morality-particular morality construct, and also his methodology, from any system-specific moral precedent. In this his product does make a claim of universality (common morality) and of verifiability. He is soberingly persistent in comporting human rights philosophy with the empirical "is" of the human experience, through time and across value systems.

Further thinking about Milne's work requires refinement, however, of two operationally distinct but conceptually linked questions not squarely confronted in this expressly limited essay. First, who actually sets the norms or operationalizes morality within communities (and within nation-states)? Stated simply, who articulates particular morality and by what process is this accomplished? Secondly, what is the source and function of revolution within communities (and within nation-states)? Where Milne does approach these questions he assumes a subjunctive style and a level of marked abstraction. These two still open questions in no way represent the self-falsification of the construct Milne has undertaken but indicate instead that the complex task has just begun. Indeed, the key to norm articulation and to the source and function of revolution likely may be found to lie in Milne's principles of common morality, that is, in beneficence and practical reason, taken together. It is not sufficient, however, simply to array norms without ascertaining the process by which they are formulated (question 1) and adjusted (question 2). Further and definitive work on the universal human rights standard is called for, and Milne has laid a valuable basis as a starting point.

EUGENE D. FRYER

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Gender and History: The Limits of Social Theory in the Age of the Family. By Linda J. Nicholson (New York: Columbia University Press, 1986. 238p. \$29.00).

Feminism and Equality. Edited by Anne Phillips (New York: New York University Press, 1987. 202p. \$35.00, cloth; \$15.00, paper).

Gender and History and *Feminism and Equality* are strikingly complementary books. They both concern the relationship between feminist theory and liberal political thought.

Linda Nicholson denounces the public-private distinction that lies at the core of traditional liberal theory but insists that Western culture's understanding of what is private and what is public has changed significantly between the seventeenth and late twentieth centuries. The heart of the book is part 2, which argues that liberalism, Marxism, and feminism each embodied different understandings of the nature of, and the relationship between, the private and the public, understandings that in turn reflected different historical conditions. In Nicholson's view, liberalism reflected the emergence of "a new form of social organization wherein the family and the state as we now comprehend them were being created out of the older institution of kinship." Marxism, in turn, reified the "growing separation and dominance of the sphere of the economic" and "ignore[d] the historically contingent nature of the separation of the family and the economy" (pp. 2-3). Political feminism emerged when a realignment between public and private led to an increasing "individualization of social life" and "the replacement of the household by the individual as the basic political unit" (p. 4). Only by a careful attention to history, Nicholson argues, can feminism avoid the universalizing tendencies of both Locke's and Marx's thought, tendencies that undercut the accuracy and usefulness of those theories.

Nicholson's attention to the treatment of kinship, family, economics, and politics in Locke and Marx is intelligent and stimulating. Nicholson effectively criticizes past theory and demonstrates the necessity of historical consciousness in political theory but leaves to others the construction of a political theory that will take account of the history of gender relations.

Anne Phillips's collection of essays,

Feminism and Equality, raises the kind of questions any adequate future theory will have to answer. All the essays have been previously published, but Phillips has selected and organized them with great intelligence so that they speak to and illuminate one another in remarkable fashion. (In a serious oversight, however, the authors are nowhere identified beyond their names, leaving the reader to guess their nationalities, backgrounds, institutional affiliations, if any, and so forth.)

The questions these essays raise fall roughly into two categories: What is the relationship between equal rights and equal opportunity for men and women and What does equality mean in a world of sexual difference?

Juliet Mitchell's "Women and Equality" and Michele Barrett's "Marxist-Feminism and the Work of Karl Marx" point out the links between liberalism and feminism and defend the continuing importance of attention to equal rights even as they criticize liberal theory's inability to account for economic and sexual oppression or liberation. Bell Hooks's "Feminism: A Movement to End Sexist Oppression" is less patient. Hooks, a black theorist and activist, wants to redirect the attention given to "rights" to systems of economic, racial, and sexual oppression.

Zillah Eisenstein's "Elizabeth Cady Stanton: Radical-Feminist Analysis and Liberal-Feminist Strategy" and Ellen DuBois's "The Radicalness of the Women's Suffrage Movement" use examples drawn from the nineteenth-century suffrage movement to show that liberal theory in the hands of dedicated feminists forces a thoroughgoing revision of the public-private distinction and of our understandings of both family and civil society. Carole Pateman's "Feminist Critiques of the Public/Private Dichotomy" shows how the public-private distinction masks a patriarchy which is an "essential, indeed constitutive part of the theory and practice of liberalism" (p. 109). One of feminists' major tasks will be to show how it is that "although the personal is not the political, the two spheres are interrelated, necessary dimensions of a future, democratic feminist social order" (p. 120).

Jean Bethke Elshtain's "Against Androgyny," Sally Alexander's "Women, Class, and Sexual Difference," and Denise Riley's "The Serious Burdens of Love? Some Questions on Childcare, Feminism, and Socialism" focus

attention on the sexual differences between men and women. Given those differences, they ask, what would real equality between men and women look like? They also ask whether, as Alexander puts it, "antagonism between the sexes is an unavoidable aspect of the acquisition of sexual identity." If so, then while the organization and meaning attached to gender can differ markedly, personal and political tension stemming from sexual difference is inevitable (p. 173).

Feminism and Equality is a splendid introduction to the range of feminist analysis concerning equality and gender difference. It shows conclusively that equality cannot be reduced to equal opportunity in the "public" realm and raises all the right questions about the implications of sexual difference for future feminist theory.

MARY LYNDON SHANLEY

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New Ways of Knowing: The Sciences, Society, and Reconstructive Knowledge. By Marcus G. Raskin and Herbert J. Bernstein (Totowa, NJ: Rowman & Littlefield, 1987. ix, 293p. \$34.95, cloth; \$15.95, paper).

For most political scientists the focus of interest in this multiauthored book will be chapter 3, "Toward a Reconstructive Political Science," by Raskin and Bernstein. The essential message of the book is in this chapter. Although the other chapters assume viewpoints and contain analyses that many social and behavioral scientists might find interesting, most political scientists will find in chapter 3 and the introduction by Bernstein as much as they might profitably invest in time and attention.

From the outset it is evident that the book is not really about "new ways of knowing"; the authors approve old ways (those of traditional societies) and also advocate ways already adopted in some fields of modern scholarship. I found none new to me. The book is a contribution to "liberation" scholarship—*liberation* meaning freedom from the biases of class, race, gender, and technocracy that are alleged to have thoroughly dominated science.

The authors declare that "reconstructive knowledge is itself a myth for generating social

reconstruction and teasing out implications of present dominant modes of thought" (p. 100). Reconstructive knowledge, say the authors, is political. It seeks to decode the various languages of classes and specialties to show their values and premises (p. 88). But neither method nor finding appears to constitute a new form of inquiry. Students of semantics; of epistemology; of the history, philosophy, and sociology of science; and even of political science in the conventional sense have been at this analytic form of inquiry for a long time and clearly not always in the service of a dominant socioeconomic paradigm. The real political science of the twentieth century, say the authors, is "comprised of political physics, political agronomy, political biology, political economics, as well as a very political political science." The authors hope that "a reconstructive political science in all its branches will come to replace the present enterprise" (p. 101).

This appears to mean that the bad biases dominating science today should be replaced by good biases inherent in reconstructed knowledge. Since the authors believe no knowledge to be unbiased or value-free, it is logical that they should urge their values as against others. They declare that "once scientists move to correct the direction of the work of other scientists by seeing the methodology of particular disciplines as class, sex, or race biased or intentionally false, it is there that reconstruction will emerge." It already has in the Soviet Union, but the authors neglect the Soviet experience, which might be regarded as the most impressive effort thus far toward new ways of knowing and reconstructive knowledge.

I find the book to have a dual agenda that could mislead or confuse naive or unperceptive readers. On the one hand it is a critique of the assumptions and values underlying a great part of present-day science and learning. I found this analysis to be the most useful and valid part of the book. On the other hand the book is advocacy for a particular point of view and while frankly stating its values and purposes, leaves the reader to infer its assumptions. The authors concede that their well-intentioned analysis must be "subject to evaluation" (p. 82).

A reasonable evaluation of chapter 3 would be that its argument is not helped by the

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stating of opinions as if they were demonstrable facts. For example, a note on page 84 declares without qualification that interdisciplinary work "merely brings the contradictions and flaws of any specific discipline together with other flawed disciplines, causing not a corrective, but an additional series of distortions in analysis." That the opposite or corrective effect might also be true is not acknowledged. A second example on page 83 describes environmental impact analysis as a "blind ritual" and "cosmetic step"; and note 8, which explains the author's description, indicates no knowledge whatever of how the impact statement has been implemented over the years. Their assessment is unjustifiably dated and contrary to serious empirical studies of the strengths and weaknesses of this device.

New Ways of Knowing is more significant for what it represents than for what it says. It is another of many indicators of a major shift in paradigms that characterize the present era. Much of the intellectual world today is in the predicament described decades ago by Matthew Arnold in his *Stanzas from the Grande Chartreuse*: "wandering between two worlds, one dead, the other powerless to be born." The world that Raskin and Bernstein dislike is not yet dead but is certainly undergoing transformation. As in the historical anatomy of revolutions, liberation and libertarian concepts and life-style, which reconstructive knowledge would celebrate, have characteristically been interim phenomena that gave way to new regularities when a new regime became established. Will the findings of an advancing free science (unless curbed by liberation ideology) arrive at findings that will place new ground rules and limitations on untrammelled freedom? Cigarette smokers, for example, have begun to think so.

LYNTON KEITH CALDWELL

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Citizenship and Capitalism: The Debate over Reformism. By Bryan S. Turner (London: Allen & Unwin, 1986. xii, 160p. \$18.95, cloth; \$8.95, paper).

Turner's essay is essentially a political reading of capitalism, starting from the assumption of the relative autonomy of the

political process from the economic. Drawing heavily on the work of T. H. Marshall, Turner argues that the growth of citizenship from civil to social rights has basically transformed capitalism and that the working class (or what remains of it) has been a major beneficiary. In pursuing this line of argument he engages in a running polemic with Marx and Marxists, whom he describes as the enemies of the "reformism" he strongly advocates.

The argument proceeds through a cursory survey of ancient society and the world's major religions, followed by a critique of the writings of Marx and an exegesis of Marshall. Turner then turns to the historical processes (social struggles and social movements) that have engendered modernization and citizenship, citing class conflict, warfare, migration, and egalitarian ideologies. He concludes by examining what he describes as the paradoxical relationship between individualism and citizenship, linking the growth of social citizenship to the expansion of individualism.

This is a useful and succinct summary of the positions and arguments in favor of one strand of reformist-socialist politics. However, as an exposition and analysis of the relationship of citizenship to capitalism, it is problematic. The substantive argument for reformism lacks intellectual depth and rigor, frequently distorting the positions of Turner's adversaries and assuming what needs to be demonstrated. To begin with, the term *citizenship* is very elastic in Turner's usage. It describes at times social rights, social membership, or social participation—or all three. It is described as a mechanism for the "abatement of class struggle" and a consequence of "real struggle." It is a criterion for "universalism" and "justice" and a sphere of battle between capitalist politics and economies. *Citizenship* is loaded with so many different meanings, functions, and attributes as to cover all the vices and virtues of contemporary political discourse, so much so that even the most catholic of political confessions would have a difficult time sorting them out. The notion of *citizenship* is at times associated with vacuous terms like *social membership*, which is made compatible with a wide range of nondemocratic as well as democratic regimes. If it is made so intentionally, that compatibility is not explained.

Secondly, Turner amalgamates a variety of "reformisms" that are profoundly different.

There is the Kautskian variety (pre-World War II), which proposed reforms of capitalism leading gradually to socialism. There is the post-World War II "reformism" of German Social Democracy, which sought reforms to create a capitalist welfare state. And there is the contemporary "reformism" of Hawke, Gonzalez, and Mitterand, who seek to modernize capitalism and increase productive forces at the expense of the welfare state. Turner neither recognizes nor discusses these different reformist positions, nor does he analyze the significant shifts (the underlying reasons for the changes) or their consequences for social citizenship.

In criticizing Marx (and Marxists), Turner resorts to knocking down straw men. He accuses Marx of basing his analysis in the absolute misery of the workers ("starvation"), and of rejecting democratic reforms as "shams." As Hal Draper, among many other Marxist scholars, has pointed out, Marx fought for democratic reforms as transitional struggles toward socialism: his critique was based on the contingent and formalistic basis of democracy under capitalism. Marx's wholehearted support of the Chartist Movement and the eight-hour day are common knowledge. The real issues that Marxists raise and that reformist advocates like Turner should address are the violent constraints that capitalism has imposed on democratic institutions when workers have challenged management prerogatives. There is no discussion of the capitalist overthrow of reformist democrats in Italy in the 1920s, Germany and Spain in the 1930s, or Chile in the

1970s.

Even more surprising is Turner's failure to examine the dismissal of the democratically elected Whitlam government in Australia. Turner is correct in noting the possibility of working-class reform in some democratic capitalist settings, but he displays a singular lack of rigor in specifying the historical context, political conditions, and economic cycles in which more changes occur. Nor does he attempt to specify the limits of such reforms. He assumes a continual process of growth and improvement through struggle and democracy, with a few general caveats about the possibilities of "reversals."

Classical political theorists may also be disturbed by Turner's characterization of Plato as a "totalitarian" (p. 13)—mass party as philosopher king?—or his comparison of the Greek city-state to a modern corporation.

While the debate between reformism and revolution has been joined in this book, today what is striking is how irrelevant it is, since there are virtually no revolutionaries, and the reformists are in retreat throughout the West. The problem is not the evolution of citizenship but the demise of civility on all levels of society. Moreover, after a decade of stagnation and the collapse of the stock market, capitalism does not seem too far behind. In a word, the arguments appear somewhat anachronistic from the vantage point of the late 1980s.

JAMES PETRAS

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AMERICAN POLITICS

Presidential Power and Management Techniques: The Carter and Reagan Administrations in Historical Perspective. By James G. Benze, Jr. (New York: Greenwood, 1987. xii, 157p. \$32.95).

It would be hard to imagine two presidencies that offer more interesting contrasts than those of Jimmy Carter and Ronald Reagan in terms

of their strategies and styles in the management of public affairs. One president was compulsively interested in facts and had the intellectual capacity and stamina to absorb enormous amounts of information but was limited in his abilities to communicate the broad themes of his administration; the other is largely indifferent to the day-to-day details of his presidency and apparently ignorant about

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important actions taken by his foreign policy advisors but is unusually skilled in the arts of political communication. One was elected after promising to make federal programs more effective and to give the United States a government as good as its people; the other was elected on a pledge to get government off our backs and to dismantle much of the federal colossus. One took on a very large agenda of domestic and foreign policy initiatives with some successes and failures across the board; the other took on a narrower set of issues with better results in achieving a limited list of objectives. One lost his bid for reelection; the other won a large national majority and a second term.

The material taken up by James Benze has tremendous potential, but unfortunately, in Benze's treatment of that material much of that potential is missed. The first three chapters of Benze's book summarize the efforts of postwar presidents to implement their decisions and policies and provide a brief history of the principal management techniques used to accomplish these purposes—budgeting, reorganization, and the manipulation of personnel. These chapters are reasonably well done but contain little that would not already be familiar to students of the presidency and public administration. The chapters on Carter and Reagan are too brief and give us only a sketchy account of how specific management techniques were used in these recent administrations. Finally, the book contains several chapters that analyze the results of surveys conducted by the author in 1980 and 1984, when questionnaires were sent to senior federal administrators (mostly bureaucrats but also some political appointees) asking for their evaluations of presidential management in the preceding four years.

The decision to survey bureaucrats in order to evaluate presidential management makes it difficult to say very much about whether or not Carter and Reagan were successful in getting their policies implemented. Benze offers no objective measure of policy implementation or compliance and the impressions of federal officials on these issues constitute a poor substitute. It is a bit like rating a chef by interviewing the waiters. The survey results do tell us something about how senior bureaucrats have viewed our recent chief executives, and these results may well be of interest to students

of public administration and U.S. bureaucratic culture. The portraits of Carter and Reagan that emerge from the surveys roughly conform to prevailing media portraits of the two presidents. Carter is described as a failed chief executive who was intelligent and honest but unable to perform as an effective leader; and Reagan is described as a successful president whose political skills compensated for his much more detached approach to policy making.

In his conclusion Benze asserts that successful presidential management depends on both the technique he has described in his book and on the skillful use of political power. This is no doubt true, but it gives such a broad interpretation to what is included as part of presidential management that it becomes difficult to imagine anything that a president might do which would not be part of a management strategy. And if management has this expansive definition, it is hard to understand why Benze wishes to draw our attention to budgeting, reorganization, and personnel.

James Benze raises fundamental issues about how presidents, and particularly our two most recent presidents, can and should deal with the government they ostensibly run—questions that have received better treatment in Colin Campbell's new book, *Managing the Presidency*. In the end he offers few original or lasting answers to these important questions.

ROBERT A. STRONG

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Disabled Policy: America's Programs for the Handicapped. By Edward D. Berkowitz (New York: Cambridge University Press, 1987. xiii, 280p. \$27.50).

As researchers begin to discover the vast social and political problems created by disability, a spate of books has appeared with titles using *disabled* as a modifier to suggest the feebleness of prior attempts to cope with these issues of public policy and human rights by various aspects of the political system. Berkowitz's study is clearly one of the best of these. This comprehensive review traces the evolution of legislation about disability from worker's compensation in the Progressive era to the formulation of the Social Security Dis-

ability Insurance program as well as vocational rehabilitation to the contemporary independent living and disability rights movement. Berkowitz not only documents the inadequacy of earlier policies but also provides a valuable introduction appropriate for anyone seeking to become familiar with this important field.

Essentially, however, Berkowitz's conclusion is the same as his opening statement: "America has no disability policy." He laments the fact that disability programs are used primarily as a means of withdrawing workers from the labor force rather than as a method of enabling them to find a productive role in society, but he has no dramatic alternative to propose. Operating almost exclusively within the conventional paradigm that defines disability as an inability or limitation on the amount or kind of work that a person can perform, Berkowitz is unable to find a satisfactory answer to the questions that he raises. While there is obviously great value to be derived from insightful criticism of the present chaotic state of disability policy, a resolution of these problems would of course be even more valuable.

Part of the difficulty might be ascribed to differences in disciplinary orientations. As a historian, Berkowitz believes that history "brings order to the labyrinth of disability programs." He asserts that historians understand, as other policy analysts might not, that the present follows logically from the past (p. 228). With these insights, a historian can open up a clear view of the larger picture that is denied by the structure of modern policy-making. Yet the perspective of history also involves the danger of becoming mired in prior definitions and paradigms rather than accepting new approaches to policy problems.

Ironically, the major strength of Berkowitz's work also reveals its principal weakness. Unlike many other researchers, Berkowitz devotes significant attention to the political changes currently being sought by the disability rights movement. This chapter is the most valuable feature of the book. But in the policy recommendations that he commendably offers in his conclusion, he fails to accept the view that discrimination is the primary problem facing citizens with disabilities—he proposes the enforcement of civil rights laws, along with the prevention of disability, almost as an afterthought. Hence his reforms amount to minor

tinkering, and the basic inconsistency encompassed by existing disability policy is never fully addressed.

This position poses an additional dilemma for policy analysts who wish to move beyond historical background to evaluate alternative remedies. Some political scientists, such as Aaron Wildavsky, believe that disability policy, like many issues, can be studied objectively. This view, however, has been criticized for ignoring the fact that people—in fact, an oppressed minority—comprise the object of such programs. Others, pointing to the significant contributions made by black, Hispanic, and feminist scholars, contend that the value of such research can be enhanced by infusing it with perspectives derived from intimate experience with a disability. The analysis presented in *Disabled Policy* seems to imply the impossibility of resolving this quandary.

As Berkowitz perceptively points out, there appear to be vast differences between the policy prescriptions appropriate to the needs of a middle-aged laborer who may feel it is too late to benefit from retraining, and to the desires of prominent leaders of the disability rights movement such as Judy Heumann, Ed Roberts, and Lex Frieden, who acquired their disabilities as young people. Yet this paradox may point the way to a solution to the fundamental problem of the politics of disability. There seems to be no similarity between the anonymous laborer who wants to leave the work force and these leaders, who are seeking active or productive lives for all disabled persons, because there is presently no basis for a shared sense of political identity. This laborer, like millions of disabled citizens, appears stymied by the prejudice evoked by disability, which is also the primary source of oppression. From this perspective, therefore, the principal remedy for the social and political problems produced by disability might be found in the development of a positive common identity that could be used as a basis for organizing a powerful electoral constituency of disabled voters, rather than in the reforms recommended by experts and professionals.

Although efforts to develop a positive sense of political identity have not yet been successful in securing a consensus among disabled citizens, there seems to be little doubt that these strivings will affect both the substance and the style of future research on disability

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policy. While seemingly a minor quibble about Berkowitz's highly readable book, this process might appropriately encourage the use of *disabled* solely as an adjective, never as a noun, the latter implying that a person's entire self-identity is subsumed by a physical impairment. Like other minorities, people with disabilities are seeking the right to strike down the stereotypes that have oppressed them in the past and to be called by names they choose for themselves. According to a recent Harris survey, a major proportion of disabled citizens consider themselves to be members of a minority group; and many are beginning to recognize that their principal problems stem from discriminatory attitudes and behavior rather than from their own functional limitations. There seem to be compelling reasons, therefore, to make scrupulous efforts to avoid the subtle implication that a feeble policy is a natural or appropriate reflection of a supposedly feeble minority.

HARLAN HAHN

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Pesticides and Politics: The Life Cycle of a Public Issue. By Christopher J. Bosso (Pittsburgh: University of Pittsburgh Press, 1987. xvi, 294p. \$29.95).

Christopher Bosso's book is a welcome addition to the policy studies literature on two counts: it covers an area that has been relatively neglected in environmental policy analysis, and it makes an important contribution to the wider debate over how we arrived at the current state of policy gridlock in Washington. Bosso traces the course of federal pesticides policy from the passage of the Federal Insecticide, Fungicide, and Rodenticide Act (FIFRA) of 1947 through the great gypsy moth and fire ant eradication campaigns of the 1950s and 1960s, the environmental reform movement culminating in revision of FIFRA in 1972, the banning of DDT and several other pesticides in the 1970s, and the subsequent deadlock over implementation and further revision of the law in the 1980s. On the whole it is a sad story, not only because pesticide control has yet to make a serious dent in the chemical dependency of U.S. agriculture but because of what it shows about the difficulties of achieving any policy

closure against entrenched interest group opposition in our political system.

That is not to say that this is just another case study in interest group politics. Bosso takes a much more comprehensive approach, introducing numerous policy models and drawing on many of the richest veins in political science, particularly the work of E. E. Schattschneider and Charles O. Jones, one of his mentors. As a longitudinal policy analysis, the book perhaps most resembles Jones's *Clean Air*, though it has the advantage of longer perspective on problems of implementation than did Jones's book on the Clean Air Act circa 1974. Many of Bosso's conclusions about "policy failure" are also similar to Jones's: the requirements of the 1972 Federal Environmental Pesticide Control Act (FEPCA), which revised FIFRA and transferred responsibility for pesticide registration from the Department of Agriculture to the Environmental Protection Agency (EPA) were far beyond the technical and budgetary capabilities of EPA to implement and invited endless litigation and obstruction. However, Bosso places these shortcomings in the larger context of the changing *structure* of policy-making, particularly in Congress. Drawing here on Schattschneider's fundamental observations that structure biases policy outcomes, and that the scope of group conflict and management of public participation are key variables in the process, he is able to show that pesticides policy-making has moved through three stages: from (1) firm control by a closed "subgovernment" (dominated for many years by Representative Jamie Whitten of Mississippi, who chaired the House Appropriations Subcommittee on Agriculture), through (2) a phase of "pluralist competition," in which participation was expanded to include environmentalists and public health advocates, to (3) its current state of "presence politics," in which policy authority is thoroughly fragmented in Congress and the policy network (Heclo) has been extended to incorporate policy "experts" on all sides who skirmish at the margins but make little real progress toward policy resolution.

Bosso's book summarizes much of recent thinking in policy studies; indeed, if there is a criticism to be made of his book, it is perhaps that it is overburdened with concepts and models to the point where one loses touch with

the main thrust of the analysis. There is also not quite enough focus on the substantive problem, namely the costs and benefits of pesticides. Although Bosso is correct in arguing that there is a vast gray area of uncertainty about the environmental and health effects of pesticides, the book would have been stronger had he made more of an effort to review the evidence. Nor, despite the attention he gives to the "pesticides subgovernment," does he identify or discuss the corporate interests involved in chemical agriculture. Finally, although many of the points Bosso makes about the failures of pesticide regulation are valid in other areas of environmental policy, his analysis should not be generalized to all policy in this field. Many pieces of environmental legislation reflected a basic shift in public opinion (not simply a Downsian attention cycle or spate of "pluralist competition") that has proven durable and supportive of genuine environmental improvement. Nevertheless, Bosso's sobering warnings of policy stasis are well taken.

NORMAN J. VIG

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The White House and Capitol Hill: The Politics of Persuasion. By Nigel Bowles (New York: Oxford University Press, 1987. ix, 261p. \$39.95).

This work focuses on presidential-congressional relations. It begins with a description of the context and organization of the Johnson White House's Office of Congressional Relations (OCR), including the development of the office, presidential involvement in congressional liaison, the nature of the OCR staff, the handling of congressional correspondence, the OCR's relations with departmental liaison offices and congressional leaders, the role of non-OCR White House staff, head counting, and congressional committee politics and assignments.

This discussion is followed by one chapter on projects and patronage and another on the role of small favors and lobbying. The latter include gifts, meetings with the president, photos, trips on the presidential yacht or Air Force One, congressional receptions at the White House, bill-signing ceremonies, election

assistance, and personal lobbying. The author describes the Johnson White House's use of these potential resources, and concludes that they are more useful in creating a favorable political climate for presidential influence in the long run than in obtaining short-term support on a specific bill.

The author then presents two chapter-length case studies of executive-legislative relations from the Johnson administration. One focuses on the passage of the income tax surcharge of 1968, while the second examines the nominations of Abe Fortas as chief justice and Homer Thornberry as associate justice of the Supreme Court in the same year. In both studies the author reveals the weakness of the OCR in Johnson's last years in office. Finally, there are chapters that provide broad overviews of the congressional relations of Jimmy Carter and Ronald Reagan.

Most of this is well-trodden ground, but the author adds some interesting bits and pieces to the portrait of Lyndon Johnson's relations with Congress. Since they have become part of U.S. political folklore, the author's critical correctives to the legends that have accumulated around the Johnson White House are particularly useful.

Nevertheless, readers would be better served if the author had integrated his work with those of others who have done work on White House legislative liaison. There is now substantial literature on the subject, and the author largely ignores it. As a result, opportunities to build on previous scholarship are lost.

The presentation of the small picture is generally well done. The author often shows considerable astuteness in his discussion of discrete aspects of presidential-congressional relations. He does not accept things at face value, and his discussion is not constrained by a rigid perspective.

The big picture is less satisfactory, however. There is no theoretical underpinning to the study, no questions to guide an analysis. Thus the descriptions of White House behavior do not lead us to generalizations in which we can have confidence.

Essentially, the author begins with the assumption that the White House legislative liaison operation is important in influencing members of Congress to support the president and therefore begs the essential question of just

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what contribution it can make to presidential leadership. Much of the material in the book actually belies this premise. For example, the author concludes both case studies with the view that circumstances made it unlikely that the president could have done any better than he did, even if the OCR had displayed more skill.

The author concludes that "this book has shown that . . . the skill with which presidents orchestrate their relations with Congress through the OCR . . . materially affects the fate of their legislative proposals" (p. 247). What it actually does is provide additional evidence that the contribution of presidential legislative skills is at the margins rather than the core of presidential leadership of Congress.

GEORGE C. EDWARDS III

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All Organizations Are Public: Bridging Public and Private Organizational Theories. By Barry Bozeman (San Francisco: Jossey-Bass, 1987. xi, 184p. \$21.95).

Do public organizations differ significantly from private organizations? If so, how do those differences affect managerial effectiveness? That is what Barry Bozeman calls the "publicness puzzle." His main purpose is to show that the public-private dichotomy is more imagined than real and that all organizations are public. It is a conclusion that seems radical at first thought, but on more reflection is common sense. It is a corollary to the conclusion that every economy is a political economy. All economies are political because they operate under rules and procedures established by political authority. Bozeman argues that "all organizations are public because political authority affects some of the behavior and processes of all organizations" (p. 83).

Two main authority types—economic and political—are identified and discussed at some length. Economic authority is explained in the context of property rights theory. The concepts of private and public goods are discussed. The importance of residual claimants is recognized. Linking authority to act with responsibility for consequences is acknowledged as an important contribution of private property rights. Bozeman suggests that prop-

erty rights theory is useful because "a wide variety of organizations, including public administrations, can possess property rights or their equivalent" and that "just as publicness has pervasive effects on business, 'privateness' (conveniently measured by constructs derived from property rights theory) has pervasive effects on government and other nonprofit organizations" (p. 59).

Political authority is more difficult to define than economic authority, but Bozeman does an admirable job by identifying different levels of political authority, combining them into a "triadic model of the effects of political authority on organization behavior" (p. 79), and showing how the model is applicable to business, third-sector, government enterprise, mixed form organizations, and traditional government agencies. He concludes, "The organization subject to political authority may, as a result, become more or less powerful, more or less effective, and/or more or less adaptive, but it is sure to be changed" (p. 82).

Bozeman suggests we might better understand and evaluate the management of public and private institutions if we thought of them as having a mix of economic and political authority. That mix is described in the Cartesian plane defined by economic authority on an x-axis and political authority on a y-axis. When various organizations are arrayed in the space it is possible to recognize that economic authority and political authority are sometimes complementary and sometimes countervailing, depending on the nature and purpose of the organization. Although Bozeman does not explore it, positing this Cartesian plane suggests that economic entrepreneurs should scan the private *and* public horizons when looking for new opportunities or better tools for thwarting competitors.

A problem with the book is that it only begins to unravel the "publicness puzzle." But Bozeman recognizes that and proposes how his model might be applied to management research, education, and application. His "most fundamental lesson" is straightforward and unequivocal: "Public management is too confining. What is needed is managerial theory and prescription for managing publicness."

The book is a significant contribution to understanding public and private organizational theories. Just as political economy provides greater understanding into the processes

and outcomes of private and public sectors, the concepts proposed by Bozeman will help us to understand the operation of public and private institutions better. As we learn to apply these concepts, we may even avoid falling into the trap of assuming institutions are either public or private and think of them as either "more or less public."

RANDY T. SIMMONS

Utah State University

Leadership and Innovation: A Biographical Perspective on Entrepreneurs in Government. Edited by Jameson W. Doig and Erwin C. Hargrove with a foreword by Richard E. Neustadt (Baltimore: Johns Hopkins University Press, 1987. xii, 459p. \$39.50).

Eugene Lewis's 1980 book, *Public Entrepreneurship: Toward a Theory of Bureaucratic Political Power*, was innovative in at least two respects. First, it was at the time one of the few works in U.S. political science to point to the fact that individuals sometimes do make a difference. Second, it demonstrated in no uncertain terms that such individuals—known in common parlance as leaders—can be found in the unlikelyst of places, that is, in public organizations. We were taught, in short, that political power and influence are exerted not only by elected officials but also by "public entrepreneurs" who manage, in large part by force of personality, to "alter greatly the existing pattern of allocation of scarce public resources" (p. 9).

Doig and Hargrove's collection of essays on "entrepreneurs in government" is an offspring of Lewis's pathbreaking volume. They, too, were interested in individuals who held high-level executive positions during a time when their agencies changed course and who appear to have played major roles both in devising and implementing these significant initiatives. This book then includes chapters on twelve government officials who fit the above criteria and one on a private executive whose terrain is similar to his counterparts in the public sector.

Doig and Hargrove coauthor an introductory chapter that sets the stage for the biographical essays to follow. They make the point that while their own emphasis is on the

impact of individuals, context is probably equally important. In particular they cite three external factors that "provide fertile ground for leadership." They are a governmental system characterized by fragmentation and overlap and therefore relatively hospitable to experimentation; a public voice that is capable of making itself heard on behalf of new social values; and new technologies that provide visionary government officials with heretofore unimagined opportunities.

The editors also cite three personal characteristics that "may be crucial" to successful leadership: the capacity to engage in rational analysis, the ability to see new possibilities, and the desire to make a difference. And they touch on an original theme when they point out that almost all of their thirteen subjects experienced "significant reverses" after their striking success. "How," Doig and Hargrove ask, "does one account for failure after success has been achieved?" (Their tentative answer: a shift of political support away from the entrepreneur.)

I dwell on the editors' all-too-brief opening essay because thereafter readers are on their own; that is, it is left up to us to extract the similarities and differences in how leadership was exercised and to draw our own conclusions about the implications of these for others in similar positions. It would have been helpful if the editors had drafted a concluding essay to sort and comment on the mass of biographical data their book provides. Such an ordering would, in and of itself, have constituted a theoretical contribution.

Most of the essays are not, in fact, "biographical perspectives," for they do no more than touch on life history. Rather—and this is in line with the editors' instructions to the contributors (p. 8)—they are chronicles of the professional initiatives for which the subjects of this book were best known. In keeping with the two clusters of skills that the editors identify as being especially important—the adroit use of rhetoric and coalition-building skills—the book is divided into two parts. Part 1, on rhetorical leaders, features chapters on David Lilienthal, Gifford Pinchot, Hyman Rickover (written by Eugene Lewis and based on *Public Entrepreneurship*), Austin Tobin, and James Webb. Part 2, on administrative entrepreneurs, has essays on Nancy Hanks, Wilbur Cohen and Robert Ball (covered together in a

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strong chapter by Theodore Marmor), Elmer Staats, Marriner Eccles, Bernard O'Keefe, James Forrestal, and Robert McNamara.

This, then, is primarily a collection of stories about organization men, and one woman, who successfully exerted power, authority, and influence to bring about change. Their tales are in the main well told and they provide further evidence—should any still be needed—of the impact of key individuals on political life. As Doig and Hargrove point out, such documentation is not only an intellectual corrective to a past bias in U.S. political science; as well, it has the salutary effect of conveying to our best potential leaders that the private sector is not the only outlet for their energies.

BARBARA KELLERMAN

Fairleigh Dickinson University

The New American State: Bureaucracies and Policies since World War II. Edited by Louis Galambos (Baltimore: Johns Hopkins University Press, 1987. 227p. \$27.50, cloth; \$11.95, paper).

Crisis and Leviathan: Critical Episodes in the Growth of American Government. By Robert Higgs (New York: Oxford University Press, 1987: xix, 350p. \$24.95, cloth).

In a slim but provocative volume Louis Galambos has assembled a diverse and spirited collection of essays on the "new American state." In separate chapters on environmental policy, social security policy, national security policy, and macroeconomic policy, four social scientists ask who really runs the modern state. An introduction by the editor and a conclusion by Matthew Crenson and Francis Rourke help to integrate this inquiry without fabricating an artificial consensus.

All the authors agree that we have an administrative state and that it is here to stay. They also agree that the federal bureaucracy has been challenged by politicians, citizens, or both. Samuel Hays, in his essay on environmental policy, stresses control from below, with strong citizens groups articulating the concerns of a consumer-minded middle class. Heywood Fleisig, in his essay on macroeconomic policy, stresses control from above, with presidents and legislators exerting leverage over fiscal and monetary policy.

Crenson and Rourke, who discuss both foreign and domestic agencies; see evidence of control from above and below.

While the authors agree that the bureaucracy is under siege, they disagree on the bureaucracy's response. Charles Neu, in a chapter on national security policy, argues that no recent president has succeeded in harnessing the national security bureaucracy. Similarly, Carolyn Weaver concludes that the Social Security Administration has managed to defeat most efforts to limit benefits or introduce private competition. On the other hand, Crenson and Rourke perceive a resurgence of political control since World War II, and Fleisig doubts that politicians have ever truly relinquished control over macroeconomic policy. Hays sees a strong bureaucracy but also a permeable one. Thus when environmental agencies fail to deliver environmental benefits, the middle class becomes aroused and insists on reform.

Although the central focus of the book is who runs the state, the authors offer many valuable insights into how the state is run. A recurring theme is the central importance of information—computer models, environmental impact statements, cost-benefit analysis, and so on. For example, the Social Security Administration has used a data monopoly and sophisticated computer models to deflect external challenges. Conversely, regulated industries and environmental groups have employed research to challenge administrative agency decisions. Clearly, information is a double-edged sword in the new administrative state.

Despite its considerable strengths, this book has two weaknesses. First, neither the introduction nor the conclusion develops an analytical framework that might explain why some agencies are highly permeable while others are not. A model based on issue characteristics, bureaucratic characteristics, and constituency characteristics would have been a welcome addition. Second, the authors sometimes treat political control in gross terms, failing to specify subtle but important differences that flow from legislative oversight as opposed to statutory controls, public hearings as opposed to proxy advocacy, personnel reforms as opposed to executive orders. Ultimately, we learn more about how control is exercised in the new state than about the consequences of exercising control in different ways. Still, we

learn a great deal, and the chapters are well written. The Galambos book is likely to be well received in both public administration and U.S. history courses.

If Galambos and his collaborators have learned to live with the administrative state, Robert Higgs has not. In *Crisis and Leviathan*, he analyzes and laments the growth of "big government" in the twentieth century. He explains that growth by focusing on major crises and the ideologies they generate. The cumulative legacy of these crises, he argues, is a "cost-imposing, cost-concealing" command economy.

Crisis and Leviathan will strike many readers as a throwback to the "great events" school of history that preceded the "social history" approach with which Samuel Hays is closely identified. From Higgs's perspective, mass values simply mirror elite values, which in turn are shaped by critical episodes, such as wars and depressions. Higgs would seem to agree with Emerson that "events are in the saddle and ride mankind."

In one respect, Higgs's book is an improvement over recent empirical studies of government growth. He does not confine his attention to increases in government spending or employment. Rather, he examines diverse forms of government coercion, including conscription, wage and price controls, and the seizure of private property. He also looks beyond legislative appropriations to executive orders, administrative agency decisions, and Supreme Court rulings.

Unfortunately, however, Higgs focuses disproportionately on two kinds of crises: wars and depressions. Consequently, he neglects significant accretions to the administrative state that are not attributable to either: for example, the Administrative Procedure Act of 1946, the War on Poverty, and the new social regulation of the 1970s. He also has trouble accounting for deregulation of the airline industry, the banking industry, the transportation industry, and the communications industry, all of which are inconsistent with the "dominant" ideology of a command economy.

Stylistically, the book is disconcerting. It is repetitive and tendentious. It contains too many quotes, too many definitions, and too many sweeping assertions. It is often glib in its treatment of both politicians and social scientists. For example, Higgs dismisses Pro-

gressives and New Dealers as closet socialists. Similarly, he derides the "quasi-statistical" research of behavioral scientists, whose thinking he finds "simple-minded." In fact, behavioral scientists are not the only observers to attribute considerable historical significance to social movements, urbanization, and demographic changes. By focusing on a limited number of key events, Higgs has written a history that is old-fashioned and incomplete.

WILLIAM T. GORMLEY, JR.

University of Wisconsin, Madison

The Sound of Leadership: Presidential Communication in the Modern Age. By Roderick P. Hart (Chicago: University of Chicago Press, 1987. xdi, 277p. \$39.95, cloth; \$14.95, paper).

This work makes an important contribution to the recent group of studies exploring the sources, patterns, and consequences of presidential rhetoric. The analysis is based upon a coding of the nearly 10 thousand speaking roles engaged in by presidents between 1945 and 1985 as recorded in the *Public Papers of the Presidents*. A comprehensive coding scheme allows the author to make extensive comparisons regarding changes since 1945 on such factors as types of audiences addressed, location of speeches, primary topics, and timing within the reelection cycle. In addition, the work includes a comparison of network responses to a group of major addresses between 1969 and 1978, and frequent quotations from speeches for illustration and analysis.

The book is particularly effective in providing a clear documentation of the dramatic changes that have occurred in presidential speaking roles since 1945. Presidents increasingly (1) engage in ceremonial activities, (2) undertake international addresses and speeches outside Washington, and (3) conduct during the first three years speaking schedules that resemble those undertaken during the reelection year. Overall, the changes show a dramatic increase in the total amount of activity. Presidents Kennedy, Johnson, and Nixon all substantially increased the levels attained by Truman and Eisenhower, while Ford, Carter, and Reagan sharply outdistanced their predecessors. Irrespective of partisan differ-

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ences, recent presidents have been devoting much more time to their speaking roles.

Readers will find useful insights into individual presidents in the interpretations of basic data. Kennedy is seen as having fundamentally increased the media sensitivity of modern presidents. Johnson was uniquely a home state speaker, seemingly in search of more hospitable audiences. For Nixon, international ceremonies held an unusually strong attraction. Ford was a uniquely frequent speaker, particularly in 1976—with speeches occurring on the average of once every six hours. In efforts to use speaking to improve his popularity, Carter was singularly ineffective. For Reagan, the continuation of high levels of speaking occurred along with a greater tendency to speak in controlled settings to reduce potentials for difficult questions. By carefully reviewing longitudinal data, the author provides an important example of the manner in which the study of distinctiveness and trends in presidential behavior can be usefully integrated.

As an interpretation of the consequences of changes in presidential uses of speaking, the book is less complete. Perhaps in part because of the desire to explore a large data set, the author confines the analysis of consequences largely to implications that can be drawn from that basic data. In those analyses, some readers will be disappointed with the lack of more precise modeling of explanatory relationships. There is also little attention in those discussions to studies that have examined public opinion changes on specific issues, or the consequences of presidential speaking on legislative behavior. Similarly, while the location of presidential speaking during election years is carefully documented, that discussion is not integrated with studies that have looked at elections as coalition-building efforts. To reach conclusions about consequences more convincingly, studies of presidential speaking will ultimately require greater integration of findings in related areas of specialization than is present in this volume.

The author's conclusions are nonetheless well worth careful reflection in efforts to understand the major changes that are clearly taking place in presidential politics. Those views are presented in the context of considerable concern with the emergence of a media-oriented presidency and include the following:

(1) presidents speak more, but do not educate, (2) presidents increasingly plan their activities simply to generate publicity, (3) there is an increasing tendency for presidents to define their role as simply giving a speech rather than actually developing policies, and (4) Reagan's leadership style represents a logical outcome of changes toward a media-oriented presidency. Provocative interpretations plus useful basic data make this book a valuable contribution.

WILLIAM W. LAMMERS

University of Southern California

The Life of the Party: Democratic Prospects in 1988 and Beyond. By Robert Kuttner (New York: Viking, 1987. xi, 265p. \$18.95).

Robert Kuttner, an economics and politics writer for the *New Republic*, the *Boston Globe*, and *Business Week* offers a provocative populist treatise suggesting how the Democratic party might reclaim its historic soul and its electoral majority. His basic theme, diagnosis, and message is that Democrats, especially of the Jimmy Carter and neoliberal mold, have lost sight of their traditional strengths and should return to being the party of economic populism and the wage-earning citizen.

Kuttner rejects the notion that to win the White House and similar political prizes the Democrats have to move to the center and imitate Reaganism. He also rejects the idea that citizens are apathetic or that they are turning against the New Deal programs. He acknowledges a change in demographics, a decline in coalition politics, and the identity crisis Democrats have undergone as a result of Vietnam and Reagan's popularity.

Like many analysts, Kuttner describes how Democrats have fallen behind the Republicans in fund-raising and the technological tools of modern day campaigns. However, he faults the Robert Strauss (former Democratic National Committee chairman) approach of trying to outdo Republicans at their own game by courting business Political Action Committee (PAC) money, and suggests instead that they seek larger numbers of small contributions and lessen their dependency on big business and PACs in general.

He decries the decline of parties and, like V. O. Key and others, notes that a regime without parties is necessarily a conservative regime. Kuttner pays too little attention to the fact that the decomposition of parties has been taking place for a very long time. It is not just that the Democrats are badly split or themeless or that the Republicans are better at raising money and using polls. The Progressives mounted a purposeful and in some ways successful campaign to undermine parties in the early part of the century.

Kuttner is especially critical of the neo-liberals. He sees them as mildly helpful in diagnosis yet virtually useless as strategists for the future. What does Kuttner propose? His prescription is a left-wing "back-to-the-future" course. He would have the Democrats return to their successes of the 1930s and 1940s as a means for winning both blue-collar and new-collar votes. Issues of social and foreign policy disrupt the party. Economic populism unites Democrats and their natural constituency. Only a broad economic program that serves the workers and the little guy makes sense. The Democratic party, he insists, must be the party of opportunity, inclusion, and the affirmative use of government to temper the extremes of the capitalist market system.

Thus he promotes national health insurance, accelerated investments in public education, comprehensive child care policies and Paul Simon's WPA-style measure to put unemployed people back to work rebuilding roads, bridges, parks, and other public facilities. He also points to Jim Hightower, the elected state agriculture commissioner in Texas, as a positive example of economic populism combined with culturally mainstream politics. In short, if Democrats are going to win, it will be because they affirm that government is not part of the problem but rather—in the Wilson, FDR, Truman and JFK tradition—part of the answer for those who live from paycheck to paycheck.

Kuttner writes well and borrows liberally from political scientists and economists. He is a decided partisan. Doubtless he is correct that economic issues tend to unify Democrats, while social and foreign policy issues have fractured the party in the past generation. He also points to good old traditional U.S. interest politics; provide the voters what they need.

However, he glosses over how much

economic and foreign policy are increasingly and irreversibly intertwined. He also ignores rather completely the various cycle or pendulum theories provocatively put forward by James Sundquist, Albert Hirschman, Arthur M. Schlesinger, Jr., and James David Barber, among others. Kuttner inadequately addresses how—even if Democrats emphasize economic populism—their disputes over foreign and defense policy might be resolved. Kuttner favors tax increases and, at least implicitly, would cut defense spending. How would he retain the Sam Nunn wing of the party? Defense cutbacks put people out of work along Route 128, in South Carolina, Colorado, Texas, and southern California. Kuttner probably has ideas for employing these folks in other manufacturing, research, and service industries, but his program is fuzzy on this subject. If the Democrats have reaped political dividends from the rise of the welfare state in the United States—albeit a conservative welfare state—so also the Republicans have reaped dividends from the rise of the warfare state. Kuttner pays this less attention than it deserves.

He is convinced, as most political scientists are, that Reagan's 1984 victory was due to the widespread belief that the economy was doing well, and far less attributable to Reagan's repetitious government-bashing rhetoric. Kuttner is at his best when he calls on Democrats not to try to mimic the Republicans, but to build their own party, develop their own strategies, reclaim those FDR and Great Society roots, and thereby become a distinct and successful programmatic party that serves its true constituencies.

Kuttner is a bit less convincing that this will work in the suburbs, the Southwest, the Rockies, or the South. Boston-based Kuttner, who occasionally lectures at the JFK School of Government, may have a Massachusetts lens through which he views his subject. He may also underestimate the depth of the anti-government, antiwelfare, and antiregulation mood that characterizes middle-class voters.

Still, his book is a welcome counterpoint to the now tiresome Reagan rhetoric, to the ceaseless pounding affirmative government takes from both the neoliberals and neoconservatives and from the majority of the so-called think tanks in the U.S., which regularly trivialize government problem solving by dwelling on failed policies or unanticipated

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consequences of otherwise effective federal programs.

THOMAS E. CRONIN

Colorado College

The New Black Politics: The Search for Political Power. 2d ed. Edited by Michael Preston, Lenneal Henderson, Jr., and Paul Puryear (New York: Longman, 1987. x, 293p. \$29.95, cloth; \$12.95, paper).

This is the second edition of a fine and widely used text. It is a mark of its distinction, in fact, that it has already gone into the second edition. As fine as the first volume was, the second is not just a reprint. The readings here are mostly either new or substantially revised, building on the ever-changing circumstances in this field. This is an excellent book that thrives as much on the independent strength of the various pieces as on the efforts of the editors to achieve unity of theme. The rich array of data, subtlety of analysis, creative presentation, and range of subjects offered are outstanding. These (mostly mature) scholars know their subjects intimately and bring a wealth of analytical skill to their tasks. This edition will be as deservedly popular as the last, and there is already reason to await a third edition.

The text focuses on the "new black politics," which could be construed to refer to post-1968 electoral politics. Obviously, much more than that is involved. But consider that such a volume, documenting almost entirely cases of black electoral politics, could hardly have been written 25 years ago. The phenomena these scholars describe are, from that standpoint, "new." Consequently, the themes that dominate the analyses are particular to the electoral arena: mobilization of black constituencies via registration and voting, strategies for institutionalizing social change ideologies, censuses of electoral success, short-term public policy outputs, and revisionist theories about the fit of black political analyses with general U.S. political analyses. And yet perhaps the most common (and quintessentially familiar) theme is the importance of racial polarization in biracial political competition.

There are but three parts to the book, although there are any number of other ways the editors might have divided the material

and any number of other subjects they might have considered. Despite this, what is offered has breadth and the three parts are in any case less discrete than they may seem.

Part 1 deals with national and state politics and includes pieces on presidential, congressional, and gubernatorial elections. (A piece on the courts is the sole one not dealing with actual election circumstances in the entire book.) Lenneal Henderson surveys black participation in presidential elections, with an emphasis on the post-1960 period. He makes some significant points about the consequences various political environmental conditions may have for black influences on such elections and the independent importance of "black political values" upon any presidential elections. Lucius Barker shows the vital importance of the Jesse Jackson campaign as a tool for bargaining for political goods and as a symbol for inciting and intensifying black mobilization. By this view Jackson is seen as the best model to do for black voters what other candidates do for citizens in the general electorate.

Two articles deal with electoral defeats. Mary Coleman and Leslie McLemore offer up a subtle analysis of failure in a congressional district in the Mississippi delta. While brief—one wishes for more—the article is solid and raises some tantalizing issues about candidate selection, the conventional wisdom of the impact of socio-economic status (SES) on black behavior, and the independence of black electors even in majority districts. It is interesting how these authors' findings for rural Mississippi compare and contrast with those of Preston on urban Chicago later on in the volume. Moreover, one awaits the Coleman and McLemore analysis of the 1986 election in this district, where a black candidate did win. No less successful in this section is Charles Henry on the defeat of Mayor Bradley for governor of California. The author moves through a mass of election return data to show convincingly that race was a significant factor in Bradley's loss. Racially polarized voting here, as in Mississippi, seemed to turn back a candidate representing centrist values that are usually admired in the general population. Moreover, the candidate also sought to diminish his racial background and sensibilities as factors in the election.

In part 2 Linda Williams does an excellent historical survey of who votes, who is elected,

and to what offices. This analysis shows the great range—albeit not a representative one—of offices held by blacks in this new era. Organizationally her piece might best have preceded the others.

The final section is on urban politics and public policy, and considers only big city mayors. Good pieces are offered on Detroit, New Orleans, and Philadelphia by Rich, Perry and Stokes, and Ransom, respectively. Michael Preston takes up the analysis of Chicago and as usual does it with consummate skill. He works from a body of theory in the general literature but uses it in a novel way to elaborate the particular circumstances of the black citizen mobilization, personal leadership, and ultimate success in the city. The author provides some interesting and confounding twists on the SES model, which seems to work in Chicago sometimes but not in other cities. The article is accompanied by masses of excellent data, though it is a relatively short piece. William Nelson—also as usual—returns to Cleveland with profound new insights. The analysis goes further than others herein in the comparative vein because of the longer experience in Cleveland. Nelson compares Stokes's relative success in meeting the challenge in serving the black electorate, to the lack of success of the succeeding regime. Indeed the latter may be merely overseeing the demobilization of the black insurgent forces in Cleveland, according to the author. Two of the strongest analytical points here are regarding the need for independent black organization and for some consistent means for maintaining organizational integrity for the sponsoring constituency. All told, these analyses of mayors provide good range for useful comparisons and contrasts.

MINION K. C. MORRISON

Syracuse University

Breaking the Rules: Bureaucracy and Reform in Public Housing. By Jon Pynoos (New York: Plenum, 1986. xiv, 221p. \$29.50).

Jon Pynoos's *Breaking the Rules* is a detailed attempt at examining the nature of bureaucratic discretion. Pynoos examines how applicants are assigned to vacancies at the Boston Housing Authority. He wishes to delimit the

circumstances under which selection rules are broken and why attempts at reform succeed or fail. Although discretion generally resides among the top echelons in bureaucracy, scholars such as Dahl, Lipsky, and Protts have clearly demonstrated that street-level bureaucrats make many independent decisions in their daily contact with the public, decisions that can affect the well-being of citizens. Pynoos's effort is an extension of the street-level literature. He is able to look at discretion over time and under changing conditions.

In fact, Pynoos finds himself addressing a conundrum. Various writers on bureaucracy have noted that when goals are absent or vague, rules can become the goals for employees. Yet at the Boston Housing Authority, goals were vague but rules did not become goals; rather, rules were circumvented or ignored. Pynoos's detailed examination of tenant selection practices over a decade clearly illustrates the failure to follow specified rules.

Tenant selection rules themselves were changed more than once at the Boston Housing Authority, internally and by Housing and Urban Development (HUD) mandate, causing a lack of continuity and clarity. In this setting, conflicting desires by staff made compliance with existing rules less likely. Some of these contradictory objectives included integration, stability, housing those most in need, and maintaining the presence of higher-rent-paying families. In addition, area politicians as well as welfare staff exerted pressure on staff to house certain families immediately.

Housing authority staff were divided into several groups, chiefly reformers who favored integration and traditionalists whose vision of public housing remained caught in the original aims of the 1930s. Given these divergent directions, changing rules were often altered to suit certain preferences. For example, a staff member might immediately assign a family to a vacant unit after a call from an elected official. Or a manager might forget to offer a unit to a family he felt would cause problems. For projects in white areas of the city, a manager might be reluctant to admit too many black families, both for safety and to prevent resegregation. Yet the policy of the authority and of the HUD was to integrate without delay. Interestingly, the authority's top brass paid little attention to tenant selection. Change accompanied periodic HUD audits, but old

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patterns of selection soon resumed. It is only when the Boston Housing Authority is placed in receivership that tenant selection practices become more uniform.

As a contribution to the documentation of the use of discretion by street-level bureaucrats, *Breaking the Rules* serves a useful purpose. However, the book could have made a more comprehensive contribution to the social welfare policy literature as well. Although his examination was limited to tenant selection practices, Pynoos could have discussed the rationale for public housing and its shortcomings in Boston and elsewhere in greater depth. Pynoos occasionally does mention projects' faulty design, crime, vandalism, and other operational problems. A more complete treatment, however, would place rule breaking in Boston in a larger perspective and would provide more insight for those not already familiar with public housing. It would clarify the divergent concerns of central office staff and project managers regarding tenant selection.

Pynoos does not fully elaborate the policy area and he also does not provide a complete picture of the state of public housing in Boston. For example, the reasons for the Boston Housing Authority being placed in receivership—especially project and financial conditions—are not discussed in depth. If Pynoos had highlighted the external and internal environments of this public housing authority, it would have placed his findings concerning discretion in a larger context and made the book more useful to a larger readership.

Lack of clarity and varying motives and pressures can lead to a subversion of the rules in a bureaucracy with vague and contradictory goals. Pynoos makes this clear in his very detailed treatment of tenant selection practices in one authority. However, his work would have been strengthened by greater and more systematic attention to the policy and operating environments of the Boston bureaucrats he studied.

LANA STEIN

University of Missouri, St. Louis

Steering the Elephant: How Washington Works. Edited by Robert Rector and Michael Sanera (New York: Universe Books, 1987. xd, 356p. \$24.95, cloth; \$12.50, paper).

Mostly written by conservative Reagan political appointees, this collection of essays tries to explain what the authors view as the failures of the Reagan administration to bring about radical policy change. Nine essays discuss urban enterprise zones, juvenile justice, education, and programs under the auspices of ACTION, the Legal Services Corporation, the Federal Trade Commission, the Small Business Administration, the Urban Mass Transportation Administration, and the National Oceanic and Atmospheric Administration. Eighteen other essays deal with the broader topics of political strategy, congressional and media relations, personnel staffing and administration, and coalition building.

Most of the essays are forward-looking; they seek to instruct future political conservatives in the next conservative presidential administration. How useful is the instruction? That depends on your view of U.S. democracy.

If you believe that those who elect a president are united in their commitment to an unambiguous policy course and that a president's ability to adopt change is not severely hampered by such things as statutory law, the principles of separation of powers and minority rights, and institutional checks and balances, then you will find this a revealing and infuriating indictment of the extent to which the Reagan revolution was betrayed. This resulted from Ronald Reagan's lack of awareness, courage, and skill, especially in screening the ideological correctness of the people he appointed; the White House staff's lack of political resolve in fighting for ideological principles; the politically liberal and self-serving civil service's ability to seduce presidential appointees and subvert presidential purposes; the willingness of Congress and the press to engage in character assassination; and from self-serving special interest groups' being artificially sustained by federal subsidies. Moreover, most of the contributors suggest that future conservative political appointees, forearmed by this knowledge of the conspiracy against conservative principles, should be

more ideologically pure and less willing to compromise for the sake of getting things done. Instead, they should be secure in the knowledge that principled stands will redefine the political landscape and mobilize a conservative coalition strong enough to dominate it.

If, however, you believe that electoral mandates are ambiguous and that presidential powers are severely constrained, then you might find many of these essays to be either reassuring or frightening. They will be reassuring to those who profit from the status quo, since they should be more secure in their belief that ideologically conservative presidential appointees who follow the imperatives of these essays will remain blind to the need to become more politically astute and skilled as managers of policy change. They will be frightening to those who worry about what might be done by those who know they're right, who believe they are being frustrated by their enemies, who sense they are being betrayed by their friends, and who feel justified in circumventing both disorderly political practices and more orderly administrative and legal procedures.

PAUL A. DAWSON

Oberlin College

The Supreme Court and the American Family.

By Eva R. Rubin (New York: Greenwood, 1986. 251p. \$35.00).

In this volume summarizing the highlights of the decisions of the U.S. Supreme Court affecting the family in U.S. society, Rubin assesses both continuity and change in the jurisprudence of the high Court. What her work demonstrates is that the Court's role in family law has been much like the Court's role in social change in other respects; it generally lags behind societal changes but simultaneously allows such change to continue. Such a role is inherent in the nature of the function and power of the U.S. Supreme Court, indeed of the federal courts as a system. Policy issues most associated with social change are predominantly the domain of the state and local governments. Only when federal regulatory policies such as AFDC or Title 9 are implicated or one argues that local policies violate a

federal constitutional right, do the federal courts have jurisdiction over these matters. And their function is, with very rare exception, only negative. A federal judge or the U.S. Supreme Court generally serves no greater role in the service of social change than to proclaim that legislative or administrative judgments are constitutionally flawed because they violate individual (or in the case of Rubin's work, familial) rights. But the courts have not and probably never will serve a substantially positive or proactive role in social change, particularly as it affects the family in the United States. For example, federal courts are unlikely to require that government at any level fund or encourage the building of child care centers, the critical family issue in the contemporary United States. In sum, the function of federal courts in support of family life in our system is fundamentally to set limits on legislative and administrative authority over the *privacy* of the family, so that it may function with relative autonomy. While this may, as an empirical matter, be the probable extent of the role of the federal judiciary, questions about how it exercises this authority must be asked.

Perhaps the most critical point in Rubin's book is her analysis of the Supreme Court's very narrow, very prejudicial, and very outdated conception of the U.S. family. Unmarried people living together, with or without offspring, do not qualify; students living together do not qualify; gay and lesbian couples do not qualify; old or impoverished people living together to survive economically, emotionally, or otherwise also do not qualify for the protections of familial relations. While persons living in such arrangements do not necessarily lose their civil liberties under the Constitution and while, as Rubin points out, they often win their battles for food stamps and other governmental services on other grounds, their right to function under the protections of the status of family in the eyes of the government is by and large not recognized by the U.S. Supreme Court. The Court's avowed preference in its decisions and especially in its dicta, is for the family of our folklore, a ceremonially married man and woman with only their own shared biological offspring, in which the male is the sole breadwinner and the female runs the home. As recently as 1961 in *Hoyt v. Florida*, the high Court upheld the exemption of women from

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jury service on the basis of their responsibility for maintaining the household, a responsibility that the Court found arguably to be incompatible with jury service.

When analyzing familial rights, one necessarily analyzes issues of sexual equality as well. While *Hoyt* was paradigmatic of its time in the circumscribed role perceived for women as civic actors in society, more important to Rubin's work is the effect of Supreme Court decisions on the relations *between* the sexes in family life. She quite correctly identifies the protection of the right to reproductive choice as supported by the Court in *Roe v. Wade* (1973) and its progeny as the critical women's rights issue in the relationship of women to the government. Implicit, however, in her assessment of the effect of the abortion decisions on family is that *Planned Parenthood v. Danforth* (1976) was far more important. In that decision the Court prohibited the states from requiring the consent of a woman's husband before she could obtain an abortion and it was also the first in a series of decisions that have substantially narrowed the role of parents in the reproductive choices of their minor daughters. In sum, while the Court's decisions determining the limits of the authority of the government over familial matters are important, its decisions that broaden *individual* rights against the government may ultimately have far more impact on the relations among family members and may slowly redefine those power relations. Thus, those who want the traditional family hierarchy preserved—and, therefore, want government limited in its authority over the family—must, paradoxically, support greater governmental authority over various areas of *individuals'* lives, particularly adolescent children. Among the relevant areas cited by Rubin are adolescent sexuality (including access to contraception and abortion rights), school discipline (including restrictions on the Fourth Amendment right of students to privacy in their lockers, purses, and so on), the return of prayer in the schools, and the censorship of reading matter deemed to be offensive to fundamental values.

Rubin has provided a very wide ranging study of the ideology and impact of Supreme Court decisions that have implications for family life and especially for traditional familial patterns of patriarchy. It would serve especially well as an introduction to the subject

for someone who wants to see all of the disparate pieces drawn together.

GAYLE BINION

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Why Presidents Succeed: A Political Psychology of Leadership. By Dean Keith Simon-ton (New Haven: Yale University Press, 1987. xi, 292p. \$22.50).

The author of this book is a psychologist who wishes to develop a "political psychology of the presidency" consisting of analysis of the personal attributes that explain effective leadership, the situational constraints on leader potential, and the attributions that followers and observers make in judging the performance of leaders. He applies personal, developmental, and social psychology to political phenomena to develop a transhistorical quantitative framework of prediction in which to assess the relative strength of individual and situational factors in the determination of successful leadership. There are four tests of presidential success: winning elections, popularity in the polls, administrative performance, and evaluation of historical greatness. Empirical research on each of these themes is analyzed.

The analysis of who wins presidential elections and why is a straightforward summary of political science research. The factors that explain victory are personal attributes, campaign rhetoric and strategy, and the match of these things to the "political zeitgeist" (not the most precise phrase). Along the way we are given some trivia about height and birth order as they contribute to success. It is concluded that situational factors, especially war and peace and the state of the economy, override personal characteristics most of the time in explaining victory and defeat, but suggested that personal characteristics are congruent with the historical situation. Thus the power-maximizing leader is better suited to crisis than is the leader with affiliation needs.

There is a useful analysis of the research literature about the decline of presidential popularity with the conclusion that international crises do rally support, that prolonged wars are damaging, and that inflation—which affects all voters—is more damaging than unemployment, thus putting Democrats at a

disadvantage. Publics sometimes punish presidents for the sins of their predecessors and hold presidents responsible for events they cannot control. Situation overrides personality, although some presidents may act to get themselves in trouble.

Effective administrative performance is a function of the match of historical situation to presidential characteristics. For example, power-maximizing presidents—who are first or only children and whose views match those of a majority of voters—will lead effectively and be prone to international expansion in a period requiring strong government. Again, we see congruence. The key to success is to be the right person at the right time.

The author loses perspective in the chapter on presidential greatness. He distills several polls of historians to achieve a consensus ranking on the great presidents, and then analyzes those presidents to develop five predictors of greatness, which he would apply to future presidents: record as a war hero before becoming president, years in office, engagement of the country in a war, the absence of scandal, and assassination. This is a transhistorical analysis that deliberately ignores unique factors in performance or attribution. The difficulty with the predictive equation is that the five predictors do not tell us why Lincoln, FDR, and others are at the top. Unique achievements account for the ranking and the shared characteristics mean little by themselves. The author's transhistorical method fails here.

The final chapter is an attempt at a theory of political leadership in which leader characteristics, situational variables, and follower attributions are joined. The analysis is more of a summary of findings than a theory; as the author admits, it is scaffolding for a future theory. There are interesting insights about the degree to which contemporary and historical attribution override actual personal characteristics or performance as presidents become symbols of an age.

My first reading of this book was unfavorable. I reacted negatively to the silliness of the inquiry into birth order, height, and so on, and to the crudity of some of the transhistorical measures. A second reading persuaded me that this is a serious effort to integrate the individual, the situation, and historical action. Presidential scholars will find that most of the

conclusions match their intuitions. But idiographic analysis, the author's term for most presidency research, would perhaps not have developed such data and propositions within a theoretical framework.

Much of the presidency is suitable for nomothetic study (the author's term), inasmuch as quantification of regularities is feasible. This includes voting, public attitudes, and legislative support data, those aspects of the presidency in which large numbers and aggregate measures are relevant. However, decision making and administrative behavior are less susceptible to such analysis for at least two reasons: the evidence is not easily quantifiable, and the primary data are gestalts, whole stories, that are distorted if disaggregated. Nomothetic and idiographic strategies of research are complementary, the former providing structures of the political dynamics within which decision making takes place and the latter filling the black box of policy-making within the nomothetic framework. Thus, case studies of economic policy-making explain best when set within a framework of economic policy and popular politics provided by quantitative analysis.

There is a tension today within political science between the positivistic, behavioral approach to knowledge and the phenomenological approach. Study of the presidency suggests how these two approaches might be brought together constructively.

ERWIN C. HARGROVE

Vanderbilt University

Breaking the Impasse: Consensual Approaches to Resolving Public Disputes. By Lawrence Suskind and Jeffrey Cruikshank (New York: Basic Books, 1987. xi, 288p. \$19.95).
The Politics of Environmental Mediation. By Douglas J. Amy (New York: Columbia University Press, 1987. x, 255p. \$30.00).

Professional mediation emerged as a highly touted alternative to adversarial politics in the early 1970s. Proponents suggested that innumerable conflicts—from landlord-tenant disputes to environmentalist-developer confrontations—could be resolved through refereed discussions. Interest groups could get beyond antagonism, stereotyping, and misunderstanding

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ing; identify their similarities and compromise their differences; and settle their disputes without the costs or coercion of government arbitration.

Lawrence Susskind and Jeffrey Cruikshank's *Breaking the Impasse* is a sophisticated book advocating professional mediation. It tells of the promise and practice of negotiating consenses. Douglas J. Amy's *The Politics of Environmental Mediation* takes the standpoint of scholarly skepticism. It dissects the problems and prejudices in mediating environmental conflicts. The explicit debate here is over the benefit-cost ratio of professional mediation. That is the strength and weakness of both books.

Susskind and Cruikshank begin with the suggestion that U.S. politics is at an impasse. For every interest, there tends to be an equal and opposite interest, which often produces political gridlock. Judges, elected representatives, and public agencies are notoriously bad at settling these confrontations. If they are willing to render decisions, politicians' solutions rarely satisfy the disputants, usually prolong the battle and its costs, and almost always erode public confidence in government.

The authors' alternative is "negotiated approaches to consensus building." These negotiated approaches do not absolutely require professional mediators, but Susskind and Cruikshank, connected with the MIT-Harvard Public Disputes Program, clearly believe that the presence of a neutral expert is essential in most cases. He or she can help opposing interest groups meet informally and transform their antagonism into "integrative bargaining." That involves changing a zero-sum conflict into an "all-gain," or "win-win," solution.

For example, citizens' groups, developers, and local government officials might identify their underlying common interest in a community's quality of life and prosperity, trade off zoning variances for community amenities, and arrive at a "responsible" development agreement that is then ratified by the appropriate government agency. Susskind and Cruikshank provide numerous examples of *apparent* public impasses where voluntary mediation generated creative consensus and all-round contentment.

Most of their book is a how-to primer intended for professional mediators and potential parties to mediation. When is a mediation

process likely to be successful? What is the role of the neutral expert? How can a powerless citizens' group use mediation to overcome an imbalance in resources, technical expertise, and political clout? What do political officials have to gain from informal dispute resolution? Why do businesses have a long-term interest in settling through the consensus process? How can pitfalls be avoided and creative cooperation conceived? People considering a negotiated settlement over litigation, for example, should read this book.

But they should also read Douglas J. Amy's *The Politics of Environmental Mediation* before making a decision. Amy skillfully presents the arguments for informal mediation and then shows that the practice of negotiated settlement seldomly delivers on its promise. On the one hand, conventional adversarial politics is not as forsaken as professional mediators imply. It is often the most advantageous arena for environmental groups. On the other hand, mediation as a *neutral* mechanism for conflict resolution is a myth. Amy shows in detail how powerful interests use mediation to force and cajole their weaker opposition into "sweetheart deals." He, too, draws on a wealth of recent examples.

Environmental groups are usually the weaker parties in negotiated settlements. Their power deficit means that some of them, especially the least compromising, are ignored in mediations. Those invited to discussions have minimal resources for hiring professional negotiators, bringing in technical experts, or gaining access to proprietary information so important in environmental disputes. These groups are also vulnerable to pressures to settle for far less than what might be considered just because their powerful business opponents can blackmail them with threats of prolonged litigation or perhaps powerful lobbying campaigns. Amy states, "It is clear that unequal power between participants in environmental mediation can undermine the extent to which this process is representative, fair, and voluntary."

Can professional mediators right the imbalance as Susskind and Cruikshank suggest? Amy is doubtful. Professional mediators have a stake in convincing disputing groups that everyone can gain by joint problem solving and splitting residual differences. In practice, this means that mediators pressure environ-

mental groups to be "reasonable"—for example, to agree to accept a "controlled" public health risk in return for limited safeguards subsidized by business. Such a trade-off is questionable; environmental groups must cede a basic principle while business spends a few dollars extra. And as long as mediation proceeds on a case-by-case basis, citizens' groups have to negotiate each conflict anew, business faces no systematic restraints, and professional mediators are guaranteed a steady stream of clients for their services.

Amy argues that mediation is a structure of "political control" used by industry to manage the opposition. It can usually be seen as "an effort to disempower environmentalists" by distracting them from more effective political strategies like litigation. From an environmental standpoint, mediation is promising only when there is a clear balance of group power that fosters stalemate. But given the imbalance of power, recently skewed further by the Reagan administration, negotiated settlements are increasingly a form of "second-class justice."

Susskind and Cruikshank do not necessarily disagree with this assessment. They, too, recognize that power imbalances prejudice negotiated settlements, and they note that mediation is not appropriate when moral principles rather than resource distributions are at stake. But their optimism that voluntary agreements generally "offer the wisest, fairest, most efficient, and most stable outcomes possible" involves overlooking the systematic inequalities in the pluralist marketplace and the moral stakes inherent in distribution disputes. In my view, Amy is the better political analyst.

To summarize, Susskind and Cruikshank treat informal, face-to-face mediation as a form of participatory democracy that can supplement and humanize U.S. pluralism and adversarial politics, while Amy sees mediation as a form of pseudoparticipation that reproduces and legitimates pluralist inequalities that are better addressed in courts and legislatures. Neither book, however, shows much sensitivity to democratic political struggle and protest by citizen groups as perhaps a more important determinant of outcomes.

Frances Fox Piven and Richard A. Cloward have pointed to the significance of mass disruption in forcing the United States' powerful interests to grant concessions. My own studies

of progressive politics suggest that failure to sustain citizen pressure and protest virtually guarantees a loss of concessions. Whether the concessions are negotiated through professional mediation or conventional political institutions may be a secondary strategic concern. Both *Breaking the Impasse* and *The Politics of Environmental Mediation* provide excellent analyses of the benefits and costs of mediation. But both are so concerned with "pragmatic" resolutions of conflicts that they have ignored the citizen disorders that make democratic politics "work."

MARK E. KANN

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Why Americans Don't Vote: Turnout Decline in the United States 1960-1984. By Ruy A. Teixeira (Westport, CT: Greenwood, 1987. viii, 149p. \$32.95).

This excellent little book adds a great deal to our understanding of the perplexing problem of declining turnout in U.S. presidential elections. Ruy Teixeira, who was a graduate student in sociology at the University of Wisconsin when he wrote the book, brings the perspective of a sociologist to the study of non-voting. He pays careful attention to demographic factors (such as age, race, and region) and includes new hypotheses that might account for the "disconnectedness" of people from the political system. In particular, he raises the possibility that the marked decline in "rootedness" in U.S. society (as measured by the decline in people married and living with their spouses and the increase in residential mobility) could bring about less political involvement.

Yet Teixeira is also well aware of the political science findings regarding turnout and includes three important "sociopolitical" variables in his study: partisanship (strength of party identification), efficacy, and campaign newspaper reading. The latter variable has not been included in many turnout studies and is particularly interesting.

Teixeira skillfully and effectively employs probit analysis as his major methodological device. The data source is the Michigan National Election Studies. Although all tables in the book present findings in probit format,

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even readers unfamiliar with probit analysis will find the book understandable; mathematical coefficients are given clear verbal interpretations throughout.

Beyond his skillful use of probit analysis, Teixeira makes two major methodological contributions to the study of the decline in turnout between 1960 and 1980. First, he builds into his model a method of accounting for the rise in educational levels in the population—a rise that should produce higher, not lower, turnout. Second, he examines the possibility that not all variables were acting in the same way during this entire 20-year period. In other words, he controls for something he calls “periodization” by examining the decade of the 1960s and the decade of the 1970s separately. Both of these methodological insights produce findings that help us come closer to solving the turnout puzzle.

In all, Teixeira’s model explains 88% of the 1960–80 decline in turnout. This is an impressive result. The political variables explain more than the sociological variables, by a ratio of about 5 to 3. Yet, without the sociological variables, clearly, much less of the decline in voting turnout would have been explained. To say anything more about the findings in this short review would not do justice to the complete picture that Teixeira’s analysis offers. The book should be read to fully appreciate that many different variables were important in explaining the decline in turnout and to see the different effects of the variables in each of the two decades.

Teixeira adds a chapter on the 1984 election as a kind of postscript at the end of the book. In 1984 there was a slight increase in turnout. This increase would be expected, given the change in distribution of some of the key variables, particularly the marked increase in political efficacy. Applying his model, Teixeira finds that turnout should have gone up even further than it did. Apparently new, unexplained, factors are having a more decided effect on turnout in the 1980s. And so there is need for still more research into this intriguing problem.

My only major criticism of the book is that Teixeira’s historical and political interpretations often show a lack of thorough understanding of the events and problems of the time and a failure to comprehend the nuances of political phenomena fully. For example, the

treatment of political parties and campaigns is very rudimentary. However, readers can supply their own interpretations, since the variables are laid out so clearly.

In all, this book makes an important contribution to our understanding of turnout. It examines an unusually comprehensive set of variables and handles the data skillfully and with insight. It should have a wide audience.

DAVID E. REPASS

University of Connecticut

The Constitution under Pressure: A Time for Change. By Marcia Lynn Whicker, Ruth Ann Strickland, and Raymond A. Moore (New York: Praeger, 1987. vii, 223p. \$38.95, cloth; \$14.95, paper).

“Never,” the authors write, “has the time been more propitious to examine the basic fundamental structures of U.S. government.” To this end their book evaluates the Constitution’s capabilities viewed from the demands placed upon it by contemporary circumstance, with special attention given to its role in shaping government and representational issues. The study is aptly historical, beginning with an examination of British misrule in the colonies, the failings of the Articles of Confederation, and a recitation of the conflicts played out in the Constitutional Convention. In an important chapter the authors then survey the political ideas upon which the constitutional enterprise was erected, including the principles of individualism, private property, contracts and law, freedom and equality, and democracy, followed by a statement of liberal constitutional principles. A rather brief section relates capitalist thought to these constitutional principles. This historical background is followed by chapters on federalism, the courts, and the amendment process, all of which are arranged to emphasize historical sequences. The final three chapters are more analytical and focus on the major evaluative effort of the work. These deal substantively with issues of representation (chap. 6), presidential versus parliamentary government (chap. 7), and policy specialization (chap. 8).

For a multiauthored work, the book reads well. It is nicely organized as a teaching text. For example, all chapters have tables that summarize the text in useful fashion. Overall, the chapters are constructed to familiarize the new reader with the major issues to which each chapter is devoted. Descriptive historical material is supplemented with interpretive material drawn from the constitutional or institutional literature. The interpretation of the amendments (chap. 5) in the context of the social transformations that either gave rise to them or have come to influence their application is especially good. All chapters end with a reference section.

The last three chapters are more explicitly analytical, and it is also here that one feels the book loses some of its thread. Chapter 6 is devoted to a discussion of representation and begins in the manner of the other chapters, surveying various issues and criticisms of representation (for example, the philosophical base, constitutional limitations on participation, direct election of senators, the electoral college, and so on). After a brief discussion of parties it moves with stylistic abruptness to a rather detailed discussion of voter apathy and a conceptual scheme for linking public opinion, legislative issues, and representation. The style of analysis here is quite out of character with what precedes it and not well integrated with what follows. Chapter 7 is written in the style of the first five chapters, and weighs presidential-style government against the presumed virtues of parliamentarianism; chapter 8, in the style of contemporary policy literature, offers five criteria for a good legislature and then evaluates Congress according to these criteria.

The book ends without a concluding chapter that leaves one with the sense of its having run down without fulfilling the promise of its theme. The integration provided in the early chapters by the historical experience of the Constitution does not seem to be matched by the choices made toward the end of the book as to what should be emphasized and for what purposes.

A more serious difficulty is the overall relationship of the book to its stated intents, signaled by the urgencies of its title. It never finally comes to grips with the task of examining the Constitution in light of the massive social changes of the past two hundred years.

The predominant emphasis is on "institutional" analysis, while the burden of the argument would appear to lie more in the direction of relating the constitutional structure to the transformations of the political economy, both domestic and international. The closest approach to this task comes, as indicated, in the chapter on the amendments in which they are seen as critically related to the demands placed on the governmental structure by a changing social fabric. By contrast, the second chapter, entitled "Liberalism and Capitalism," contains only five pages on capitalism and two paragraphs on contemporary capital. It is fair to say that the subject is not really discussed. One wonders what "pressures" upon the Constitution are referred to in the title. A significant amount of current U.S. politics and policy literature would emphasize the political economy, especially the role played by increasingly large and concentrated wealth structures. More recent literature would emphasize the equally powerful impacts on U.S. political and social life of a world economy into which the U.S. is fully insinuated but the dynamics of which it can no longer wholly control. Yet other literature would emphasize the emergence on the world scene in the past decades of transformative institutions such as the transnational corporation, a new manifestation of capitalism that has an impact on all governments, democratic and nondemocratic alike.

None of these themes, however, are taken up in this book, and while it is not totally fair to hold against a book what it does not do, one is led to in this case by the professed intentions of this volume. Viewed outside this frame and within the more limited boundaries of a text devoted to a recitation of constitutional issues in government, it remains a useful and welcome book. Viewed from the perspective of its ambitious intentions, it is too limited a treatment.

DEANE NEUBAUER

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COMPARATIVE POLITICS

Tanzania: Crisis and Struggle for Survival.

Edited by Jannik Boesen et al. (Uppsala, Sweden: Scandinavian Institute of African Studies, 1986. 325p. \$26.50).

Once hailed as an exemplary case of how to pursue development because of its emphasis on equity within the context of an Africa-inspired strategy (*ujamaa*), Tanzania often suffers rebuke today. In little more than a decade the pendulum has swung from one extreme to the other. Because of its political stability, relative openness, and innovative development policies, Tanzania received more scholarly attention than most other African countries in the 1960s and early 1970s. In recent years, however, its position in the comparative politics literature has suffered a serious decline. Although it seems that permits to carry out research have been easier to obtain for expatriate scholars in the past five years, other factors, notably disillusion with the *ujamaa* experience and logistical problems, have limited the production of academic books on the country. This trend has been particularly apparent in North America.

It is in that perspective that this book deserves attention by scholars interested in comparative development experiences. It is the first solid scholarly assessment for many years of Tanzanian policy. The authors are all social scientists from the four Nordic countries—Denmark, Finland, Norway, and Sweden—some of Tanzania's strongest supporters in the international community. Their account, however, is no apology for the way the Tanzanian government has pursued its development strategy in the past 20 years. Examining one sector after the other, the volume conveys a penetrating critique of much of what was being done in the name of *ujamaa* in past years.

Each chapter tends to follow the same format: a presentation of how the crisis manifested itself, how it came about, its roots, and the response attempted by the Tanzanian authorities. Though the authors do not follow an identical conceptual framework, they tend

to agree that the real root of the present crisis lies in the failure of the government's agricultural policy. Here the book tends to follow the argument of other critics of agricultural policies in Africa, notably Robert Bates and Michael Lofchie, but goes into greater detail in its analysis of various aspects of both the agricultural and the industrial sectors. Knud Erik Svendsen—once the economic advisor to President Nyerere—provides the macroeconomic analysis that helps place the various case studies in a broader perspective.

The book has two principal shortcomings. While it does provide valuable insights into the outcomes of specific policies, it fails to analyze the political structures in which policy-making and implementation take place. The authors are aware of this gap in the analysis but they provide no clue as to why they fail to provide a discussion of the Tanzanian state and its decision-making structures. As such, the book lacks the elements that a political scientist would particularly look for in explaining policy.

Another shortcoming is the limited attention the book pays to the social sector. Much of Tanzania's performance record to date is associated with creative and innovative policies in the health and educational sectors. Few countries have made as much progress in adult literacy as Tanzania has in the past two decades. How has the crisis affected these achievements? How much of the social sector is holding up in a situation of budgetary retrenchment? What alternative institutional arrangements, if any, are being considered? The picture of Tanzania in crisis remains incomplete without further discussion of these questions than the book provides.

It must also be noted here that although the authors have made an extra effort to get their research published without delay, the book is already overtaken by one important event: the final agreement between the International Monetary Fund and the Government of Tanzania to restructure the country's economy. This accord has changed the basis parameters

of policy beyond what the authors of this volume are considering.

In spite of these limitations the book is the most thorough and up to date overview of Tanzanian development policy currently available. It lends itself very well to use in development policy seminars. Finally, a compliment to the sponsoring institution of this project, the Scandinavian Institute of African Studies. After a lull in its seminar and publications effort, it has more recently returned to the academic publishing scene with several valuable monographs and issue-oriented volumes, for example, on African refugees. The book about the crisis in Tanzania is another confirmation that the research it sponsors is worth taking seriously.

GÖRAN HYDEN

University of Florida

Muslim Hausa Women in Nigeria: Tradition and Change. By Barbara J. Callaway (Syracuse, New York: Syracuse University Press, 1987. xx, 242p. \$29.95).

This book is a detailed study of the Hausa Muslim women in Kano, northern Nigeria; Kano is the largest state in the Nigerian Federation. Muslims represent the largest minority (38%) in Nigeria, and 10 out of 19 states have Muslim majorities (p. 103). The study asks whether education and political activism would affect the culturally assigned roles of women. It concludes in the negative; the overwhelming power of religious beliefs circumscribe the potential for change.

The strength of this study lies in the rich data, drawn from field work and extensive survey research, collected between 1981-83. Fascinating insights are offered on women's views of education, polygamy, marriage, divorce, and religion. The contrasts between male and female views on a specific subject and the two sexes' perceptions of each other are interesting. The Hausa society is disheartening to any feminist and humanist; this is a tightly secluded and segregated society with high infant and female mortality rates. Most girls are married by age 12 and life expectancy for women is only 36 years as compared to 45 for men (pp. xvi, 36).

Many of Callaway's findings are intriguing.

For instance, the two sexes often use different terminologies to describe a single woman who is not a member of the family. While the women's reference clearly means nonmarried, the men use a term which translates into prostitute, implying sexual and economic independence (pp. 42-43). After a detailed discussion of the role of reformist male leaders, especially Mallam Aminu Kano and Isa Wali, who advocated Koranic principles of justice and equality for both women and men, the author states that reform in the status of women is more likely to succeed if urged by men (p. 129). This is a common view, and it stems from the frustration borne by reformists who wish to bring about change in abusive cultural practices. A similar opinion was expressed to this writer by a Sudanese female physician who had actively campaigned to stop female circumcision in her country to no avail.

Certain themes in this study can hold cross-national relevance in other Islamic societies, such as the strong belief in destiny, closeness of women to each other, old age as a variable establishing a female's legitimate authority and relative independence, and deep distrust of Western ethics.

Despite these possible similarities one must be cautious not to generalize the Hausa case to other Islamic societies; regrettably, Callaway attempts to do just this in the last chapter by presenting Islam as the central theme. Some of these generalizations are sweeping and not supported by available evidence (pp. 191, 202, 205-7, 209). Hausa demonstrates an extremely closed society; it is a classic case of women in purdah where deviations are hard to come by. Not even the Islamic Republican regime in Iran has been able to exert such a tight control on its female population. The State of Kano appears highly reactionary and static. While the basic precepts are Islamic, the unbending severity and rigid adherence to implementation is unusual. Perhaps what makes the Hausa Muslims so strict is their forced coexistence in the Nigerian Federation where there are large numbers of non-Muslims. This self-preserving tendency might have strengthened their continual adherence to the most conservative Islamic schools of thought. Also, changes involving women are very new in comparison to many other Islamic countries. Universal primary education was only introduced in

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1976 (p. xv), women voted for the first time in 1979 (p. 101), and there was no family planning in the area until 1983 (p. 28). To generalize from Hausa's Islam to other societies is to miss the complex economic, sociopolitical, and nativist elements that shape a national milieu.

The book states in error that polygamy has been prohibited in Jordan, Syria, Morocco, Iran, and Pakistan (p. 188). These countries have devised bureaucratic means to discourage and restrict the practice of polygamy, not to outlaw it. Also, it was Iraq, not Iran, that passed a personal status law restricting polygamy in 1959; Iran followed suit in 1967 and later in 1975. Since the inception of the Islamic Republic, the government has officially encouraged polygamous marriages providing incentives and labeling it as an act of heroism and devotion to Islam.

Despite these shortcomings, Callaway's work is impressive. This book makes a very important contribution to our knowledge of women in a closed Islamic society and makes a statement that *change* is a concept more liberally used in theory than applied in practice.

ELIZ SANASARIAN

University of Southern California

Ideological Conflicts in Modern China: Democracy and Authoritarianism. By Wen-shun Chi (New Brunswick, NJ: Transaction Books, 1986. xiii, 372p. \$39.95).

China's Establishment Intellectuals. Edited by Carol Lee Hamrin and Timothy Cheek (Armonk, NY: M. E. Sharpe, 1986. xix, 266p. \$35.00, cloth; \$14.95, paper).

These two books examine the relationship of Chinese intellectuals to authority. The late Wen-shun Chi resurrects the variety of ideological viewpoints eclipsed by communism and compares what he sees as a consensus informed by a synthesis of traditional Chinese ethics with Western liberalism against Mao's political thought. Carol Lee Hamrin and Timothy Cheek and others analyze the roles and careers of intellectuals who found a place on the Communist rostrum only to fall victim to some ideological dispute.

Kang Youwei and Liang Qichao, with whom Chi begins his discussion, both belong to the

first generation of Chinese intellectuals exposed to Western political ideas. He goes on to discuss the careers and ideas of Sun Yatsen, Hu Shi, Zhang Junmai (Carsun Chang), Zhang Dongsun, Liang Shuming, and Chen Duxiu, as well as Mao. Chi sees the essence of early twentieth-century Chinese political thought as a search for democracy. This is not entirely convincing, for his sample includes both unabashed liberals such as the student of Thomas Dewey, Hu Shi, and the conservative Confucian reformer Liang Shuming, not to mention the Confucian-inspired visionary Kang Youwei, or the radical Chen Duxiu. As a symptomatic Chinese scholar, Chi implicitly assumes that liberalism is compatible with—if not actually inspired by—the Chinese ethical tradition reaching back to Mencius. Not surprisingly he reviles Mao's effort to uproot that tradition.

Chi is most clear-headed in his analysis of the failure of these intellectuals to secure a liberal outcome. They were still animated by a desire to be "tutor to the king" as a method of bringing about good government and were themselves indifferent politicians. Mao, on the other hand, was a man of action. While the Communists organized and strived to realize their ideals, "the reformers of the scholar group just talked and waited for competent leaders to invite them to advise and adopt their programs" (p. 292).

Chi deplores the tragic consequences of the revolution, and intends to redress the historical balance by exposing Western readers to modern Chinese political thinkers who were not "revolutionary ideologues." His book, in addition to reacquainting us with some neglected Chinese thinkers, helps correct a perspective that sees the past century of Chinese history in exclusively revolutionary terms. Indeed, some of the thinkers discussed may yet find a new audience in the current era of reform.

The eclectic selection of representative intellectuals in the Hamrin and Cheek volume ranges from apparatchiks like Peng Zhen to the writer Bai Hua. Somewhere in between sit the historian Wu Han, the editor and essayist Deng Tuo, the Marxist philosopher Yang Xianzhen, the economist Sun Yefang, and the readers and editors of *China Youth*.

The introduction by Cheek and Hamrin provides a sensitive analysis of the role of senior

intellectuals in the Chinese Communist regime and puts the selected characters in context. One may dispute calling the lifelong party cadre Peng Zhen an "intellectual" but cannot dispute his importance, especially in a volume treating his close intellectual and political associates, Wu Han, Deng Tuo, and, to a lesser extent, Yang Xianzhen. Pittman Potter's chapter treats Peng's legal-political ideas in a way that clearly sums up his steadfast Leninism. Carol Lee Hamrin's portrayal of Yang Xianzhen highlights an orthodox Marxist philosopher whose academic views were enmeshed in personal and national political rivalries out of all proportion to their substantive merit. He is a victim of a system where ideological and personal authority became synonymous. Following his rehabilitation in 1979, Yang turns into a curious anachronism, a rehabilitated victim who persists in wielding the stick of Marxist orthodoxy against his erstwhile persecutors now turned reformers.

Deng Tuo, the founding editor of the *People's Daily*, emerges from Timothy Cheek's sympathetic portrait as a "Mozartian commissar" who combined strict orthodoxy with sensitivity and erudition. Deng Tuo, a Marxist reincarnation of the upright Confucian scholar, reinterprets Mencius's "kingly way" as "the honest ideological workstyle of a mass line based on practical reality," while the "tyrannical way" was "the blustering ideological workstyle of willful acts based on subjective and arbitrary decisions." The latter "workstyle" drove Deng to his death.

Barry Naughton's Sun Yefang comes closer to the Western ideal of the principled scholar's allegiance to the truth. He consistently defended the objectivity of the economic discipline against the arbitrary voluntarism of Mao. Secure in his Marxist convictions, Sun saw no contradiction between his principles and loyalty to the party.

Tom Fisher emphasizes Wu Han's attachment to the concept of *ging guan* or "upright official." Yet Fisher dismisses the Maoist accusation that Wu wrote *Hai Rui Dismissed from Office* as an Aesopian protest against the sacking of Peng Dehuai. Fisher shows that Wu's interest in the Ming official predates the Lushan Plenum, and notes the strict adherence to historical fact in the play. The attack on Wu was "a clear case of the 'manufacture of deviance,'" (p. 175) and Fisher concludes Wu

was in no way a dissenter.

Richard Kraus depicts Bai Hua as a "cultural gadfly" who never loses sight of his political patrons. Kraus applies the model of the poet-official Qu Yuan to contrast the Western liberal intellectual and the Chinese scholar. Bai Hua seeks to maximize the power of artists to influence state policy rather than apply the liberal prohibition against state interference in the arts.

David Ownby's essay on youth provides the epitaph for the Confucian pattern of scholarly political engagement in both these volumes. Communist intellectuals, like their Confucian forebears, saw a moral obsession to find self-fulfillment in service to the state. In post-Mao discussions on the meaning of life in *China Youth*, Ownby finds the generation who grew up entirely under Communist rule and experienced the Cultural Revolution cut off and alienated from this tradition. This generation seeks fulfillment in the private sphere, completely detached from state concerns.

Ultimately the outcome Wen-shun Chi desired may be better served by this ethic. The urge to serve the state rendered efforts at moral suasion over Mao (or Chiang Kai-shek) futile. By rejecting the role of adjunct to the state the new generation may be better able to turn the state toward their own purposes, an ethic much closer to liberalism than Confucian morality. *China's Establishment Intellectuals* thus answers the question of the failure of liberalism much better than did Chi, who posed it. Nevertheless, both books enrich our understanding of the relationship of intellectuals to authority in modern China.

JEREMY T. PALTIEL

University of Alberta

Professions and the State: The Mexican Case.

By Peter S. Cleaves (Tucson: University of Arizona Press, 1987. xv, 147p. \$19.95).

The selection of Carlos Salinas de Gortari to be Mexico's next president underscores a significant trend in that nation's politics: the saliency of "modern" professional training. Mexico's political elite traditionally added extensive experience in party and elective positions to its law education at a Mexican university. Salinas earned a masters and doctorate in

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economics at Harvard University; his professional expertise, coupled with relatively little political experience, promoted his meteoric rise. Although other authors have already documented the turn from law toward economics, Peter Cleaves provides new insights into this and other tendencies concerning professions and the state.

The interesting core of Cleaves's argument is that relations between professions and the state in Mexico, as well as key issues confronting those professions today, cannot be understood according to the standard liberal model of the politics of professions. That model posits professions with special characteristics (skills, prestige, norms, monopolies on knowledge, gatekeeping functions, cohesiveness and so forth) that give them considerable power, including influence over the state; private practice and direct professional-client relationships are common. In Mexico, however, professions typically lack those special characteristics. For example, employers are the gatekeepers determining who among the professionally uncontrolled supply of university graduates will find desired employment, and government is the dominating employer. Overall, Mexico's professions lack the prestige and organizational power to control their own destinies, not to speak of influencing state policy. It is the state that powerfully shapes the professions.

This is stimulating stuff. It runs counter to important thought about development and therefore to policies of organizations trying to shape professional development in the third world. Cleaves himself, who spent 10 years working for the Ford Foundation in Latin America, tentatively takes a "pro-profession" (p. ix) view and uses terms such as professional "preconsciousness," but he does not necessarily endorse the liberal model (or any clear alternative).

The approach is original; so is much of the information on professions per se, whereas information on the political and university systems is sketched in. Other works on Latin American politics touch on the professions, but Cleaves makes the study of professions central. He identifies several useful analytical dimensions. A good example is his national-international cleavage. Mexico's professions differ among and within themselves according to where their members were trained, whence

they draw their inspirations and research agendas, and so forth. Foreign assistance has naturally promoted the internationalist side, with both positive and negative consequences for Mexico.

To explore such dimensions, Cleaves chooses five professions critically intertwined with the state: law, economics, agronomy, petroleum engineering, and medicine. Comparisons among the five at least partly offset the lack of cross-national comparative scope. That lack is understandable in terms of the lack of parallel work on other underdeveloped nations. Still, the book's findings would have taken on more meaning if they had been set alongside even nonsystematic comparisons to Latin American nations. What happens where the state has been less stable and civil society has been stronger? Similarly useful would have been a closer look at state-profession relations in European nations from which Latin America drew its model of professional training in public universities, nations that do not conform to the liberal model based on the U.S. and British cases (as Cleaves recognizes).

Indeed the big problem with this work is its limited scope. It is so limited as to appear more like a rich essay than a book from a major press. Cleaves manages to say quite a bit in 105 pages of text, based on 43 interviews. More interviews, information from other sources, data on professional association characteristics, and contextualizing and comparative information from other works on professions and on Mexican politics and higher education could all have helped substantiate, elaborate, and delineate the findings. They might even have helped suggest an alternative model of relations between professions and the state, beyond the critique of the liberal model.

Several other reservations might be raised. For example, based on Cleaves's own evidence I would see somewhat more strength in Mexico's professions than he does and thus less consistency with certain interpretations of Mexican authoritarianism. But such reservations are legitimately debatable in a good work and pale by comparison with the limitation concerning scope.

Perhaps thinking of the book's limited scope, Cleaves set a modest standard of success: to spark "additional interest in a fascinating subject" (p. x). That standard he

met, and more. Cleaves brings fresh perspectives and information to the topic, and he delivers his material in a well-written and judicious manner. The book may disappoint for what it does not cover; it impresses for what it does cover.

DANIEL C. LEVY

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China's Continuous Revolution: The Post-Liberation Epoch, 1949-1981. By Lowell Dittmer (Berkeley and Los Angeles: University of California Press, 1987. xv, 320p. \$35.00).

This is a broad-ranging and learned interpretation of the first 32 years of the People's Republic by one of the more reflective observers of the Chinese scene. A book that will be appreciated most fully by the seasoned China specialist, it contains Dittmer's detailed analyses of three crucial periods in the party's unusually long effort to "continue the revolution": 1955-60, when the party, having replaced old social structures with new socialist ones, flirted briefly with liberalization; finally lurching back toward repression and mass mobilization; 1964-67, in which elite polarization spawned a revival of Mao's charisma and a destructive and incoherent mass movement; and 1968-76, in which radical triumph gave way to elite dissensus, political deadlock, and eventually the purge of Mao's radical supporters. As part of a conclusion, Dittmer illuminates yet a fourth pivotal period: the de-Maoization and reform of 1977-81.

Yet this is not a political history. Dittmer is preoccupied with the overall *meaning* of these decades, reanalyzing each of these periods with an eye to his overall interpretive theme: that the Chinese party, up through the death of Mao, was struggling to "continue the revolution," while disagreeing about what this meant. "Continuing the revolution," of course, was the doctrinal justification for Mao's personal dictatorship and political repressions. From the outset, it is clear that Dittmer is preoccupied with what Mao was thinking, why he came to the judgments he did, how he and others reacted to the initial failures of his doctrine's implementation, and what ultimate-

ly were the political and social constraints that defeated his vision in the 1970s.

However Mao-centered this conception may be, Dittmer uses his own, not Mao's terms of analysis. Revolution requires, he argues, a charismatic leader who performs a "salvatory mission"; an "illegitimate authority structure" to be overthrown; and a "mobilizable mass constituency" to propel against this structure. Dittmer effectively illuminates the constraints placed on continuing revolution by China's changing circumstances, and how and why the leadership fell out over the issue. He also fashions a metatheoretical language to convey his arguments, for instance, *emergent* versus *residual structure*, *smashing frames*, *subjective* versus *objective charisma*, and *evacuation of mission*. While such language is initially difficult to unravel, it becomes less so with familiarity and serves effectively to integrate a wide range of material.

Integrating such a broad-ranging study is no small feat. Dittmer ranges widely across the Chinese scene: commenting insightfully on elite politics, the maneuverings of national and provincial bureaucratic constituencies, the building of factional networks, the evolution of Mao's thought, grass roots social organization, and the social origins of Red Guard factionalism. Especially valuable is Dittmer's extended analysis of the "late Cultural Revolution": from the end of the Red Guard movement in 1968 to the arrest of the "Gang of Four" in 1976. Without once resorting to simple narrative, Dittmer analyzes the evolving coalitions and conflicts among top party personalities, their factional networks from the capital to the provinces, and grass roots social conditions and political ferment, and provides a compelling account of why and how Cultural Revolution radicalism lost its appeal and support in the 1970s.

In such an ambitious work of interpretation, it is unfortunate that Dittmer does not engage other interpretations and arguments in scholarly dialogue. How does this interpretation differ from others? Indeed, to what long-standing questions about Chinese politics does it offer new answers? Moreover, how does the Chinese effort to continue the revolution compare with the way that the same problem has been handled in similar political regimes? How is this interpretation relevant to others' efforts to understand the generic features of de-

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Stalinization, political mobilization, and the contest between forces of conservatism and reform? On these and other questions, which would help us to locate the study in a broader scholarly discourse, the author is largely silent.

ANDREW G. WALDER

Harvard University

Mozambique, a Dream Undone: The Political Economy of Democracy, 1975-1984. By Bertil Egerö (Uppsala, Sweden: Scandinavian Institute of African Studies, 1987. 230p. \$44.50, cloth; \$29.50, paper).

Mozambique: A Dream Undone belongs to an easily recognizable genre in African studies: the committed, partisan book, written by a disillusioned sympathizer of a revolution gone sour. The 1960s spawned such books on Algeria, the 1970s on Tanzania, and the 1980s are producing them on Mozambique. Such books have predictable strengths and weaknesses and this one is no exception. Egerö makes a serious attempt to understand what happened in Mozambique since independence, killing the dream of an egalitarian and flourishing society and leading to the present economic and military chaos. He starts from the assumption that socialism can work and in fact should work, and tries to explain why it did not in this case. The assumption is open to question, but it helps Egerö focus on what actually happened in Mozambique, and this should prove helpful to readers of any ideological persuasion. On the negative side, however, the book has a tendency to get lost in a theory of what socialism should be, setting impossibly high standards against which Mozambique is measured.

The result is a very uneven product, probably much more interesting to a reader already familiar with Mozambique than to somebody seeking a basic understanding of the country. The book contains adequate information, although it does not add much to what has been published elsewhere. Unfortunately, the information is poorly organized, unsystematic, and difficult to follow—the book needs some good copyediting. A patient reader could

reconstruct a chronological overview of postindependence Mozambique from the information provided in the book but would have to work hard to do so.

The author is more interested in analyzing issues than in providing information and seems to forget that the two cannot be separated. The most interesting aspect of the book is the discussion of the role of the state in a country following an ideology of *poder popular*, which Egerö translates as “popular power,” rather than the more commonly used, and more distinctly socialist, “people’s power.” Unfortunately, the discussion comes very early in the book, before even the most basic information is provided, and would thus be confusing—or meaningless—to most readers.

Egerö shows how the ideology embraced by Frelimo, before and after independence, called for people’s power. What emerged was state power, the spreading of bureaucracy and centralization resulting in a top-down approach, “power by the popular government for the masses,” rather than “power by the masses” (p. 35). Egerö predictably regrets this outcome. Less predictably, he also tries to explain it, rather than simply condemning it.

“In the post-liberation period, the party-state had before it a far more complex web of interrelations, international as well as internal, through which to develop a strategy for social and economic transformation. At the basis of colonial society was force; the relaxation of repression in the last few years had shown directly in for instance a decline in peasant cultivation of cotton” (p. 31).

The leadership was thus caught in a real bind. With a society used to force, and neither peasants nor workers ready to become the motor of revolution, “Frelimo had to rely on the state, the embodiment of its class enemy, to create the material conditions for advancement” (p. 31).

Mozambique: A Dream Undone is not the book on which to rely for an introduction to postindependence Mozambique. Joseph Hanlon’s *Mozambique: The Revolution under Fire* (1984) contains much better and clearer information concerning the same period. Egerö also stops with 1984 and thus does not discuss the very important changes that have taken place since the signing of the Nkomati accord. For a reader seeking a better understanding of Mozambique and of the process of socialist

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transformation in Africa, the book offers, despite its shortcomings, food for thought.

MARINA OTTAWAY

Johns Hopkins University

Political Change in Greece: Before and after the Colonels. Edited by Kevin Featherstone and Dimitrios K. Katsoudas (New York: St. Martin's, 1987. viii, 301p. \$37.50).

The study of modern Greece has been largely ignored by social science scholars. Since it is not an advanced industrialized state like those of Western Europe, nor a socialist state in the Eastern bloc, nor for that matter part of the Middle East, it has been considered a small country of little interest or importance, except for its strategic role in the eastern Mediterranean. This lack of interest has been particularly pronounced since the end of the Greek civil war in 1950 and Greece's firm consolidation into the Western bloc as a client of the United States. However, Greece's entry into the European Community in 1981, followed by the entry of Spain and Portugal in 1986, has prompted studies both of the European Mediterranean as a region, peripheral both geographically and economically to the core of Europe, and of the individual states in this region. One consequence has been a revival of studies of the Greek policy and society.

Political Change in Greece is a much-needed volume that fills a void in the existing literature on Greece. Its primary purpose is to look at the continuities and discontinuities of the Greek political system prior to and after military rule, which lasted from 1967 to 1974. Consisting of 14 articles, two of which are revisions of previous publications, the volume's focus is on the informal political institutions of Greece. A brief historical introduction is followed by a discussion of the 1975 constitutional framework and of the prevailing public attitudes as the context for the articles that follow. The bulk of the material deals with political parties, interest groups, the media, and the military. Except for two articles, one by Van Coufoudakis dealing with Greece's foreign policy from the end of World War II until 1985 and one by Susannah Verney on Greece and the European Community, domestic politics and policies are not considered.

It is disappointing that this much-needed study, particularly on developments in the postjunta years, fails to provide the reader with a comprehensive understanding of the dynamics of Greek politics. Overall the articles are descriptive historical overviews lacking in analysis. Nor are the individual articles integrated within an overall framework or general theme that would enable the reader to place the discrete articles within a coherent context. As a consequence the volume appears disjointed, leaving unanswered questions as to the relevance or significance of the material presented. Nor is there a satisfactory explanation as to why the military warrants a separate article while other political institutions such as the executive, the legislature, and the judiciary are ignored. While it can be argued that the military has been critical for political developments in Greece, the role of these other institutions—in other words, the nature of the Greek state—has been, and is, of the utmost importance.

This volume would have been immeasurably strengthened if it had included material on the socioeconomic foundations of Greek society and its changing interconnections with the political spectrum. For example, a consideration of both the critical role the petty bourgeoisie has played by shifting party affiliation and therefore electoral outcomes and the roles of other class interests and of political style, would have added depth to the several articles on the Greek political parties. An exception to the volume's failure to provide an overall synthesized framework is Mouzelis's brief conclusion, an abbreviated version of an article that appeared in *Le Monde* in December 1985, addressing the issue of political change in Greece as it pertains to the functioning of the postjunta democratic polity. The issues he raises are significant. His balanced discussion of the persistence of the "personal-fragile" nature of the current Greek party system—in particular of the governing party Pan-Hellenic Socialist Movement (PASOK) despite some changes from the prejunta years—leads him to question the viability of civil society.

Other than Coufoudakis's interesting article on Greek foreign policy, in which he analyzes the efforts of the postjunta governments to enhance Greece's autonomy and independence and the constraints they face in implementation, none of the authors analyze the impact of

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historic foreign penetration on the functioning of domestic politics and institutions. It is difficult meaningfully to understand the political configuration in Greece while ignoring, as these articles do except for Veremis's on the military, the interventionist role of Greece's patrons and their penetration of the state structures and interest group organizations.

Within the parameters of this volume the quality of the individual articles is uneven. Among the best is Kapetanyannis's on the Communists, which, although impassioned, is an incisive analysis of the evolution of the Marxist Left in Greece. Although some of Featherstone's assumptions in his article regarding specifics on PASOK are dubious, his discussion of the party's ideology and its populism is highly informative. Fakiolas's article on interest groups highlights both the linkage between party affiliation and interest groups and their excessive legal regulation by the state. On the whole this volume does contain considerable information on the continuities and discontinuities in the Greek political system following regime change that should be of interest not only to those concerned with Greece but also to those involved with Mediterranean politics and with the results of democratic rule following dictatorial rule.

ADAMANTIA POLLIS

New School for Social Research

China's Second Revolution: Reform after Mao. By Harry Harding (Washington: Brookings Institution, 1987. xvii, 369p. \$32.95, cloth; \$12.95, paper).

Harry Harding offers a case-by-case study of post-Mao China's major political and economic reforms from their origins to early 1987. This book's strength is breadth and detail. As the most comprehensive study of reform in post-Mao China by a single author to date, it deserves attention.

The book is carefully organized in three sections. The first, on the origins of reform, sums up the failures of the Mao period, noting both economic inefficiency and the political "crisis of confidence." Harding argues that after Mao died change was inevitable, but the direction and degree of reform was largely determined

by the reformers. He divides the reformers into moderates favoring incrementalism and radicals favoring fundamental reforms and attributes the cyclical pattern of reform and retrenchment to factional conflict.

The second section, on the content of reform, includes chapters on restructuring the economy, opening China to the world economy, liberalizing political life, and institutionalizing the post-Mao reforms. Harding is careful to balance achievements and problems, drawing careful lists of each before making conclusions. It is difficult to summarize his case-by-case analysis, but he argues that China's economy is headed toward neither capitalism nor orthodox socialism but toward some mixture of plan and market; that China's foreign trade will continue to grow, but China will not become a case of "export-led growth"; and that China is developing "consultative authoritarianism" but not even a "quasi-democratic" system. While Harding finds important improvements in China's political institutions, he is concerned that the reforms may not yet be sufficiently institutionalized to ensure continuity after Deng's departure.

The third section considers the future of reform and its impact on China's foreign policy. Harding finds China's foreign policy pragmatic and increasingly nationalistic. He argues that the United States will benefit from supporting China's modernization, but suggests that minor conflicts are inevitable. He considers future scenarios from a revival of radical Maoism to still more radical reform but argues that in all likelihood, the future will more or less resemble the present.

Harding's detailed empirical analysis is a strength, but he does not develop a systematic theoretical analysis. Some terms are used ambiguously. For example, he commends Deng's characterization of the reforms as "China's second revolution" (p. 1) but also states that "much of the basic structure of late Maoist China remains intact" (p. 4) and that "the core of the reform program . . . is limited liberalization" (p. 48). The "first revolution" was more than "limited" and left relatively few of the structures of Guomindang China intact. Despite labeling the political reforms "limited liberalization," elsewhere Harding argues that Deng does not have liberal values. This ambiguity leaves the reader unsure what significance Harding thinks the reforms have.

A more theoretical analysis might have developed the broader picture more fully and reached different conclusions about the prospects for reform. Harding views the reforms largely as a result of the will of the reformers. In the closest he comes to a structural argument, Harding considers the problems inherent in a semiplanned, semimarket system but argues that the reformers can choose to retreat or to push on. This argument makes an extremely important point: politics and not economics remain the root of Chinese society. But what range of options is really open to reformers? Harding's discussion of the Mao period gets the facts right but does not capture the real horror of Mao's campaigns. If Mao's legacy was a real crisis of state, then the reformers have no choice but radical reform. A comparative perspective would point out that reformers in different socialist countries have chosen similar reforms, which leads to the conclusion that reform strategies reflect structural problems of socialism. A more consistently structural perspective would conclude that the process of reform is also limited by state structures. Harding notes that "a veneer of unity, which masks a cauldron of conflict underneath, often characterizes Chinese politics" (p. 289) but does not examine the sources of conflict. If critical conflict is inherent in the structure of China's state, then the cyclical pattern of reform and retrenchment is not just a matter of conflicting viewpoints but reflects deeper dilemmas. Such dilemmas might lead to a radical departure in one direction or another, as has happened so many times in China's recent history.

This book is well researched and reasonable. Its conclusions will be supported by many China specialists. It is strongly recommended, especially for non-China specialists who want information on concrete issues and for policy-makers or business people who want scholarly research without excessive jargon. China specialists will learn something from Harding's discussion of the strategies of moderate and radical reformers, and may find it a useful reference, especially considering the wealth of statistics. Harding deserves a wide readership.

BARRETT MCCORMICK

Marquette University

Marxism and the Crisis of Development in Prewar Japan. By Germaine A. Hoston (Princeton: Princeton University Press, 1986. xviii, 401p. \$40.00, cloth; \$15.00, paper).

Although Japan's Marxists have never enjoyed great political success, they have had a substantial impact in scholarly and journalistic circles, helping to shape the contours of political discourse even in the early years of the 1920s and 1930s when the radical left was a target of "thought control" and state suppression. This book describes the principal Marxist interpretations of modern Japan through an account of the interwar debate between the two leading schools of the Rono-ha (or Labor-Farmer faction) and the Koza-ha (associated with the Japan Communist party).

The author skillfully blends chronological and analytical frameworks in tracing the debate; the central chapters relate how Japanese Marxists viewed the course of capitalist development in Japan, the concept of the "Asiatic mode of production," the emperor system, and the character of Japanese agriculture. There are also several contextual chapters that will be useful to many scholars unconcerned with Japanese Marxist theory *per se*; for example, an early chapter on the Comintern's policies toward Japan will interest students of Soviet foreign policy, and a concluding chapter on postwar Japanese developments in Marxist thought will serve scholars of the contemporary party system by clarifying the theoretical genealogy of today's leftist parties.

The author sets up each facet of the interwar Marxist debate by describing the Western and Soviet writings that served as starting points for the Japanese theorists, demonstrating in the process a real mastery of both well-known and obscure thinkers who were studied by the Japanese. The analysis then records in some detail the efforts of Japan's Marxists to apply imported theoretical constructs that rarely fit their country's experience without modifications. Japanese Marxism is presented as an example of how many non-Western Marxist schools have had to adjust European strains of Marxist theory to fit non-European patterns of development.

The author emphasizes throughout that although the classical Marxist texts and the

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Comintern's occasional theses on Japan narrowed the boundaries of the debate, their application required considerable creativity and produced some significant novelties in Marxist thought. It is difficult to identify a major new theoretical concept born of early Marxist thought in Japan; an innovation of such magnitude was unlikely given that prewar Japanese thinkers wrote almost exclusively about Japanese capitalism and not about capitalism in general. But their debates did establish some important precedents for later developments in Marxist theory, especially regarding the relative autonomy of the state and the nature of the transition from feudalism to capitalism; and they also generated valuable historical research as each of the principal factions sought to bolster its point of view.

The book's descriptive account reflects careful, painstaking research, and it cannot be faulted but for two gaps. The first is the paucity of biographical background on the debaters themselves. The reader naturally gets interested in these people while absorbing their ideas; but although the debate itself is brought to life here, the same cannot be said of its participants. A second lacuna is the relative neglect of the period from the mid-1930s to the end of World War II. The "conversion" (*tenko*) of so many Marxist theorists imprisoned in this era must have been accompanied by some interesting theoretical reflections, especially on topics such as the relationship of nationalism to Marxism. A more careful treatment of this period could only have underscored the importance of the prewar Marxist debate, because the social analysis conducted by the Left made a profound impression on leading bureaucrats and civil intellectuals of the "renovationist Right," whose ideas become dominant in Japanese politics in the late 1930s. This linkage between leftist and rightist ideas merited closer attention, especially since Hoston did devote an entire chapter to Kamekichi Takahashi's much earlier Marxian apology for Japanese imperialism (penned in 1927).

While the main thrust of this book is to describe the interwar Marxist debate in Japan and to place it in a broader theoretical perspective, Hoston occasionally offers assessments of the validity of the arguments described; these assessments are not as well grounded as the rest of the book. Although the author herself is

clearly not limited to a Marxist perspective, there is no serious consideration of non-Marxist interpretations or extensive marshaling of evidence to test the claims of Marxist intellectuals. For this reason, the author's rather consistently expressed preference for the social analysis of the prewar *Koza-ha* (which supported the Comintern's view that interwar Japan had not yet achieved a bourgeois-democratic revolution) over that of the *Rono-ha* (which saw Japan as a bourgeois-dominated society in this era) lacks a firm foundation. Nonetheless, evaluative statements of this type form a very small part of the book, and Hoston's preferences do not appear to have swayed her descriptive analysis.

Overall this book is a welcome addition that will be of service to many scholars. Japan specialists will find that it contributes much to the established literature on the history of Japan's leftist labor movements (none of which addresses prewar Marxist theory with anywhere near the skill and detail to be found here) and also that it complements the spate of other interesting books on modern Japanese political thought that have appeared recently (e.g., the work of Carol Gluck, Sharon Nolte, and Miles Fletcher). Students of comparative and especially third-world Marxism will be grateful for an analysis of the Japanese case that speaks directly and competently to broader trends in political theory. Indeed, this book is one among many encouraging signs that the relative isolation of Japanese studies from the mainstream of disciplinary scholarship is coming to an end.

GREGORY J. KASZA

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Opposition in Western Europe. Edited by Eva Kolinsky (New York: St. Martin's, 1987. x, 400p. \$35.00).

Opposition is fundamental to the operation of democratic government. Strangely enough, however, relatively little political research has been focused primarily upon opposition. The obvious classic is Robert Dahl's *Political Oppositions in Western Democracies* published in 1966, although early theoretical treatments by other scholars such as Ionescu and Kirchheimer were influential. Santori, Duverger,

and Beyme have more recently dealt with the matter within a more general context of political parties and party systems, but a comprehensive and systematic analysis of contemporary oppositions is needed.

This book is not that analysis. While it includes some rather perceptive examinations of oppositions within individual countries, it lacks a focus or structure to hold it together as a simple comprehensive or systematic study. The official introduction includes four pages that state the general problem, but there is no conclusion. Instead three papers are included by Peter Pulzer, Klaus von Beyme, and Gordon Smith that discuss concepts of opposition. These, however, appear to have been written independently of the individual country studies that follow and of each other. What we get then is a compendium of some thoughts about, and approaches to, the study of opposition. Even though this is a common fate for most edited volumes, it still leaves us without a systematic analysis of contemporary oppositions.

Peter Pulzer's general analysis is perhaps the most useful of the three included in the book. In a chapter entitled "Is There Life after Dahl?" he suggests a trend away from both polarized pluralism and consociationalism. In country after country, he says, there is more confidence in the system's ability to absorb peaceful alternation of governments and to tolerate policy opposition. Paradoxically, the exception is Britain. In much of Europe, however, new concerns have arisen and old loyalties have lessened. Citizen groups and single issue movements call into question not only the old political agenda but also the traditional ways for articulating opinion. Although new methods will come along for expressing it, opposition will go on. Gordon Smith also argues that institutional democracy in Parliament is running well; it is the sharp challenges by diverse minority groups that make the lives of governments difficult. The major problem remains one of translating minority views into some kind of majoritarian assent.

The country studies in the book tend to deal with the questions normally raised within the context of each particular country. David Denver, for example, enters the political debate in Britain over the nature of the opposition since its division and fragmentation in the elections of the 1970s and 1980s. The classic

model of adversarial politics has been lost, he argues, in the highly fluid political situation. In France, on the other hand, Bryon Criddle sees a substantial growth of consensus. It may not be the Butskellism of the 1950s, but it is a long way from the French electoral rhetoric of the 1970s. In reviewing the history of opposition in Germany, William E. Paterson and Douglas Webber find a "preponderantly co-operative cast." There rhetoric belies reality, although the Greens have recently caused the Social Democratic Party to move away somewhat from its essentially cooperative position.

Geoffrey Pridham states that the Italian Communist party (PCI) in Italy is both transformative and accommodative. Its search for legitimacy and the need to come to terms with the Christian Democratic party (DC) drives the first; the second derives its history and identity as a party. Pridham expresses some doubt that it can break out of its traditional role of opposition. Ken Gladdish offers a very interesting view of what has happened to politics in the Netherlands since accommodation. The decline of confessionalism has made the center more attractive, not less. Although the electorate perceives itself as a divided society, there is a high level of consensus about how problems should be handled and how policy should be produced. Accommodation, in other words, has worked. Benny Pollack and Jean Grugel argue that Spain is moving toward a nationally based two-party system with some regional variation. The Left has succeeded in becoming a "natural party of democracy" because the Right remains tainted as a "natural party of authoritarianism."

One of the strengths of the book is the inclusion of a consideration of extraparliamentary oppositions. David B. Capitanchik examines the reduced role of the Trades Union Congress (TUC) as an effective political opposition in Britain and blames this on the decline of traditional industries and their work forces. Michalina Vaughan argues that the "wrong Right" is deeply rooted in French history but has shifted from those who reject the Republic totally to those who reject the standards on which consensus rests. Eva Kolinsky points out that legitimacy of German institutions is still questioned by pockets of opposition. A confrontationalist core does not yet allow parliamentary politics to speak fully to the new issues. Grant Amyot examines new

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political movements in Italy and finds that the decline of the traditional subcultures and ideologies weakens the old parties and leaves more room for new social movements. The parties, he believes, will have some difficulty getting the new demands together with existing interests so that the new movements will increasingly seek a voice inside as well as outside parliament.

The book is not an easy one to read. It has its share of turgid prose masquerading as insightful analysis, and the densely packed typeface is inexcusable. Even so, it is worth reading. It contains some very useful detail for students interested in European politics.

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Development Strategies and the Status of Women: A Comparative Study of the United States, Mexico, the Soviet Union, and Cuba. By Margaret E. Leahy (Boulder, CO: Lynne Rienner, 1986. 167p. \$25.00).

Although not based on original research by the author, this brief and concise text offers a lucid distillation of virtually all available literature in English on the subject of women and the political economy of development in the United States, Mexico, the Soviet Union, and Cuba. While not providing new information, by analyzing and interpreting the available literature on the four countries in the context of liberal-capitalist and Marxist categories as different approaches to development, Leahy presents an engaging perspective that calls for a fundamental reexamination of the role of ideologies in promoting women's equality.

Unlike many "comparative" studies, this book provides throughout the text a genuine comparative analysis of the four countries and the two ideological models as they affect the status of women. After a solid review of the evidence and a penetrating critique of the contradictions and disparities between premises and results within each country and each ideology, the author makes a compelling case in favor of the Marxist model: "Although none of the nations studied has placed issues of women's equality at the top of its development

agenda, those nations that have pursued a strategy for women's equality reflective of a Marxist logic have tended to do so to a greater extent than those that have followed a liberal capitalist logic. In many ways, the present formulations of capitalism and liberal democracy act to constrain progress toward women's equality" (p. 124).

Within the Marxist example, the juxtaposition of the Cuban and Soviet experience is particularly revealing. The Soviet Union has a longstanding commitment to women's equality; women have reached very high levels of participation in public affairs and have achieved considerable gains in terms of economic position, political power, and social status. Yet the achievements of Soviet women have resulted in their assuming a double burden: virtually exclusive responsibility for household maintenance on top of work outside the home. This is the result of unchanged traditional attitudes toward women's role in the home, reinforced by the failure of the state to develop adequate surrogate domestic services. However, given the logic of the Soviet system and its commitment to social equality, the author still concludes that with increasing material well-being, the country will move closer to genuine equality for women. The Cuban example indicates how more rapid progress becomes possible: while in the Soviet case further progress toward equality is conditioned by changes in the material condition of society, in Cuba the impediment of economic constraints has been minimized by a more active and pervasive governmental role in setting up comprehensive social legislation and in supporting political organizations that promote women's equality. And the public commitment has in turn influenced the socialization process. The lesson, of course, is that even within the Marxist logic—with its commitment to equality—a socialist political economy will not automatically engender equality for women unless accompanied by active governmental intervention and an explicit policy of linking women's equality with socialist transformation and economic progress, as happened in Cuba.

On the other hand, in a capitalist country (such as the U.S. or Mexico), the primacy of economic efficiency as the guide to production and profit easily results in conditions that mitigate against women's equality. It becomes

costly and inefficient for private business to assume the burden of providing surrogate support services; and at the same time the role of the state is limited to that of arbiter among interest groups. Given the dominance of the capitalist logic, the resulting and prevailing political ideology in such countries is for a very circumscribed governmental role: promoting equal *civil* rights for women but not special benefits and rights capable of addressing women's special needs.

While cogent and persuasive in the context of the four countries examined here, the fundamental theme of the book is undermined by the absence of any references to other examples of liberal-capitalist countries where the status of women in fact appears to be quite different. Based on Leahy's argument, Scandinavian and West European countries should exhibit a situation similar to that of Mexico and the U.S. Thus the demonstrable differences would seem to indicate that Leahy's argument, notwithstanding, liberal-capitalist societies may in fact not necessarily present "an ideological barrier . . . inhibiting conscious attempts to change social ethos" toward women's equality (p. 127). Similarly, although the Cuban and Soviet examples obviously do illustrate important aspects of growth and development along the Marxist (or more correctly, the Soviet Marxist-Leninist) pattern, can we in fact state that these two countries typify Marxism as a "development strategy" in general and on the issue of women's status in particular? In the absence of an expanded comparative analysis to encompass a greater variety of Marxist and liberal-capitalist countries, Leahy's conclusions can at best provide preliminary material for further investigation and a stimulating debate—but, in all probability, they will not be able to gain acceptance as valid empirical theorizing.

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Changing Boundaries of the Political: Essays on the Evolving Balance between the State and Society, Public and Private, in Europe. Edited by Charles S. Maier (New York: Cambridge University Press, 1987. ix, 417p. \$49.50, cloth; \$14.95, paper).

This volume of stimulating essays, edited by Harvard historian Charles S. Maier, grew out of meetings of the Joint Committee on Western Europe of the Social Science Research Council and the American Council of Learned Societies. Half of the 12 contributors are sociologists, a point that may have lent a certain new-discovery breathlessness to topics long considered by political scientists. Three are political scientists (Suzanne Berger and Ellen Immergut of MIT and Miles Kahler of the University of California, San Diego), two are historians, and one an economist.

Some of the essays are brilliant, but the collection as a whole offers no single subject or thesis. One is hard put to draw connecting links between them. Maier asks what is political; and the answer seems to be—potentially at least—just about everything, a finding few political scientists would quarrel with.

To be sure, specific historical, social, economic, and even political pressures turn the private into the public or the technical into the political. Occasionally, too, things go the other way as issues recede and are depoliticized. There is no clear-cut unilinear trend toward everything becoming political, much to the relief of some of the contributors.

Some of the essays do connect thematically. Alessandro Pizzorno, taking a leaf from certain historians, argues that "absolute politics" owes its genesis to investiture conflict of the eleventh century, in which Pope Gregory's efforts to expand the powers of the papacy triggered contrary kingly claims. The newly emerging kingdoms took over some of the absolute qualities heretofore associated with Catholicism. *Mutatis mutandis*, Suzanne Berger finds that the breakup and political failure of French Catholic "integralism" left its adherents to carry their passions into the labor movement and nonconfessional parties. What the church claimed in the eleventh century its descendants brought with them into the politics and unions of the twentieth.

Two British-focused essays also connect with each other, one on the development of

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the welfare system, the other on the politicization of health care. Massimo Paci finds the shift in welfare provision from charity and market to state-run administrations may have crested. With the return of private insurance we may be seeing a "revenge of the market." Paul Starr and Ellen Immergut, in perhaps the book's best essay, emphasize that there has been pressure to move medical care not only from the private to the public but also from the political to the technical. An area designated as "technical" may thus be removed from the political agenda and placed in the hands of professionals. This is only a momentary respite, though, for a crisis of costs soon grows out of the expansion of benefits and the budgetary autonomy of the doctors and administrators. The doctors got their autonomy as part of the original bribe to get them to go along with the state health system.

Implicit in the essays is the question, Are we on the brink of major political change? Have postwar welfare states reached a logical conclusion in overextension and ungovernability? Some of the essays think so. Claus Offe believes the Greens have turned their backs on the growth-cum-welfare state and evolved an attractive new paradigm of quality of life, authenticity, and participation. Laura Balbo, a Milan sociologist and deputy in the Italian parliament, suggests new roles for women in the postwelfare state.

Probably the main thrust of the essays, however, suggests more of the same: political battles within the pluralist welfare state. Dutch economist Jan Pen sees no clear end to the expansion of transfer payments, and John Goldthorpe suspects Thatcherite neoliberalism will ultimately fail amidst heightened class tensions. Miles Kahler likewise sees no escape for Europe through supranationalism.

The essays suggest enticing comparisons with the United States and Japan, unfortunately left unexplored. Examining Western Europe alone no longer suffices; we should increasingly be looking at the first world as a whole. *Changing Boundaries of the Political* is worthwhile reading for Europeanists, more for the thoughts it provokes than for any answers it gives.

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Politics and Petroleum in Ecuador. By John D. Martz (New Brunswick, NJ: Transaction Books, 1987. xii, 432p. \$34.95).

Considerable disagreement exists among students of Latin American politics about the impact of governing arrangements on public policy. While some contend that differences between authoritarian and competitive regimes are important for understanding the policy-making process and its outcome, others totally dismiss the policy relevance of regime type.

Politics and Petroleum in Ecuador makes an important contribution to this ongoing theoretical debate. It does so on the basis of a detailed analysis of the evolution of petroleum policy in Ecuador between 1972 and 1984. During this period the oil industry emerged as the driving force in the Ecuadorian economy, displacing export-oriented agriculture to become the nation's principal source of foreign exchange. The rapidity with which this development occurred was extraordinary. Whereas in 1971 oil had accounted for less than 1% of the nation's total foreign exchange earnings, by 1973 it was the leading export product.

John Martz traces the way in which four successive Ecuadorian governments responded to the oil bonanza. The analysis is divided into two major sections. The first examines the two military governments of the 1972-79 period, when a nationalist and statist approach to the oil industry was initiated. The second major portion of the study focuses on petroleum policy under elected administrations during the 1979-84 period, when policy shifted away from the nationalist approach of the military. But it is not the policy contrasts between military authoritarianism and democratic pluralism that Martz emphasizes. In a cautious and carefully crafted analysis, he argues that regime differences had little impact on Ecuadorian oil policy. The retreat from the economic nationalism of the early boom years began under the military and reflected changing economic conditions rather than regime change. As a small producer incapable of influencing international oil markets but highly dependent on oil exports, Ecuador's vulnerability to changing external forces was pronounced. Hence Martz asserts, "The foreign debt, declining reserves, stagnating exports, and a sagging international petroleum market

all combined to shift policy emphasis from reformism to an embattled struggle to maintain the recent status quo. . . . Such a dramatic turnabout was largely independent of the nature of the regime" (p. 390). Martz also shows that basic policy orientations and bureaucratic styles remained remarkably constant across the 12-year period. Economic pragmatism held sway throughout, as did a technocratic approach to the management of the oil industry. Hence Martz concludes that "where petroleum policymaking is concerned . . . the conclusion is inescapable that the nature of the regime was not a major determinant" (p. 393).

The obvious question about a study of this type is its broader relevance. As Martz implicitly recognizes in his carefully qualified conclusions, generalizing from a study of a single policy area in a single country is hazardous, especially if there is reason to think that the policy area and country in question are atypical in some way. Would a study of policy in an area of state activity less directly affected by international economic forces than petroleum have led to a different conclusion regarding the impact of regime differences? What about a study of petroleum policy in a country where regime differences have been more pronounced than in Ecuador? In emphasizing the role played by international economic forces, Martz suggests that the answer to the first question may be yes. Yet the possibilities of innovation in all areas of policy were conditioned by external forces due to impact of export revenues on government budgets and the economy as a whole. Ecuador is hardly unique in this regard. Hence Martz's findings, which underline the severe constraints under which decision makers operate in small Latin American countries, are clearly of broader theoretical relevance. With respect to the second question concerning the possibly atypical nature of Ecuadoran politics, Martz recognizes that the differences between military and elected regimes in Ecuador have been rather formalistic due to the strength of elitist influences and the limited nature of popular participation in public affairs. He thereby offers a major caveat to his conclusions, hinting that the alternation in power of inclusionary democracy and exclusionary authoritarianism may produce dramatic policy changes even if shifts between *dictablanda*

("soft" dictatorship) and oligarchical democracy do not.

In addition to its contributions to the study of Latin American policy making, Martz's book represents a major addition to the limited body of literature on modern Ecuadoran politics. It provides a good overview of events between 1972 and 1984, including the Ecuadoran military's seizure of power and the subsequent process of democratization. Students of bargaining relationships between multinational corporations and host countries will also find this an interesting volume.

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Authority and Inequality under Capitalism and Socialism: USA, USSR, and China.
By Barrington Moore, Jr. (Oxford: Oxford University Press, 1987. viii, 142p. \$28.00).

In 1966 Barrington Moore, Jr., concluded his classic work, *Social Origins of Dictatorship and Democracy*, with a pessimistic prognosis for the future of capitalist democracy as a model for modernizing societies. He wrote then that democratic capitalism, "is an historical formation that . . . is now past its zenith. If the revolutionary wave continues to sweep through the backward world in the years to come, that is scarcely the form it is likely to take" (p. 483). More than twenty years later, in his latest book, the doyen of comparative historical sociology is equally pessimistic about authoritarian socialism as a rival model to democratic capitalism. He concludes the current book by arguing that neither model is capable of resolving problems of authority and inequality. If the reasoning is sound, it is a profoundly depressing message for students of politics as well as for leaders of third-world revolutionary movements. Unfortunately, upon examination this reviewer finds that the reasoning is indeed sound and the implications are devastating; we seem to have run out of alternatives to resolve some of the great problems that afflict developmental revolutions.

Where one might take issue with Moore is not with his conclusions but with the extent to which they are widely accepted among revolutionary leaders in the third world. He says that

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the present time is exceptional in that it is the only epoch in which there are no credible models deserving of emulation. While at various points in history Athens, Confucian China, England, France, the United States and, most recently, the Soviet Union and China have served as ideals worthy of copying, today, "the big models have been discredited" (p. 116). Perhaps they have in some quarters; but revolutionaries in Central and South America still seem largely committed to socialist models, and the upheavals in Southeast Asia have led in the same direction. Indeed, the Khmer Rouge in Cambodia and the Sendero Luminoso in Peru are recent examples of unmitigated adherence to an extreme form of state socialism.

But perhaps these are exceptions that only prove the rule that Moore is attempting to formulate in this persuasive collection of essays, a revised and expanded version of a series of lectures that he gave at Oxford University in 1985. In this volume he uses the cases of the United States, the Soviet Union, and China to compare key differences between capitalism and socialism. Specifically, he focuses on systems of authority and inequality interpreted as the principal mechanisms employed by complex societies for placing individuals at different positions in the social hierarchy and for determining how well or how badly each position is to be treated by the society.

Moore's review of the evidence, as always, is presented in a provocative and well-documented fashion. It leads him to a set of generalizations that finds that capitalism and socialism have far more in common than different in their systems of authority. Both capitalism and socialism share a dislike for authority, a disbelief that different social groups will inevitably have conflicts of interest, and a refusal to confront the overwhelming evidence that systems of authority and social group conflict invariably lead to benefits for some groups that come at the expense of other groups.

Moore also finds a great deal of similarity in socialist and capitalist systems of inequality, even though each one each departs from quite different initial principles. Socialism attempts to develop the "new socialist man," one who will be motivated by nonmaterial incentives in a classless society. Capitalism relies upon individual greed for motivation and the guiding

hand of the market to transform individual gains to societal gains. But in the most important socialist systems, the Soviet Union and China, market forces are being increasingly used to supply the missing motivation needed to rescue the economy from complete stagnation. Capitalism, for its part, became increasingly concerned over the failure of the market to provide benefits for the poor and substituted increasing doses of government regulation and larger and larger income transfers to those excluded from the prosperity that was brought on during the period of rapid industrialization. While it is true that the capitalist world of the 1980s is increasingly resorting to a return to the market via privatization of state-controlled enterprises, Moore does not believe that the reemergence of the market in either socialism or capitalism will resolve the underlying problems that face both systems.

Capitalism, Moore shows, remains plagued by unemployment, inequality of wealth and income, and irresistible urges to exploit less powerful nations via imperialistic actions. Socialism, for its part, has failed universally to produce material abundance anything like that common to the leading capitalist societies, while at the same time oppressive socialist bureaucracies have severely restricted freedom of choice and brutal secret police forces have constrained freedom of action and thought. To these failures of socialism, Moore adds their inability to reduce to any significant degree the inequalities characteristic of capitalist societies. Although not addressing directly the evidence marshaled by those who have conducted empirical tests of world system theory showing that income inequality in socialist systems is far lower than that in capitalist systems, Moore argues convincingly that the party elite enjoys material rewards (luxurious housing, excellent food, special health care, and so on) as well as other perquisites of power and influence that make socialism and capitalism virtually indistinguishable. He goes further by arguing that the supposed triumphs of socialism in eliminating poverty are also unsubstantiated, with the socialist poor "making do at the margin of subsistence" (p. 118). While poverty has not been eliminated in the capitalist United States either, the continued rise in living standards there means that those defined as the poor enjoy, from both com-

parative and historical perspectives, an astonishing abundance of goods and services.

Moore concludes this study by arguing that neither socialism nor capitalism will eliminate authority or inequality. Authority in the form of bureaucracy is needed by socialist systems to replace the market function, while it is required in capitalist societies to allocate welfare benefits and protect against the abuses of big industry. Inequality, for its part, is essential to stimulate (rather than coerce) people to work so that goods may be produced to satisfy demands. In light of those conclusions and his analysis of the failure of either model to solve our major problems, Moore argues against those who have turned to religious fundamentalism as the only remaining way out. He suggests instead that we ignore appeals for "politically glamorous" solutions while we encourage our leaders to seek feasible, as opposed to millenarian, dreams.

This is a little book but it is far from a small one. It reflects the distillation of a great deal of thinking about big issues. As such it would be an ideal volume for a wide range of courses on the politics, sociology, or economics of development. One can only hope that Oxford University Press will soon publish a paperback edition to facilitate its use in the classroom.

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Transitions from Authoritarian Rule/Prospects for Democracy. 4 vols. Edited by G. A. O'Donnell, P. C. Schmitter, and L. Whitehead (Baltimore: Johns Hopkins University Press, 1986. xix, 710p. \$49.50, cloth; \$9.31 ea., vols. 1-3; \$5.91, vol. 4).

The four edited volumes of essays that make up *Transitions from Authoritarianism/Prospects for Democracy* are the product of an ambitious cross-national research project sponsored by the Latin American Program of the Woodrow Wilson Center of the Smithsonian Institution in 1979. The collection encompasses 13 country studies (5 in Europe, 8 in Latin America); 7 theoretical essays exploring the military, economic, international, and methodological aspects involved in the study of authoritarian regime transitions; and a concluding reflection on the entire enterprise.

Because of its scope and theoretical depth, this is the best examination of the subject to date and as such constitutes a major addition to both the literature on modern authoritarianism and that of comparative politics in general. Since space constraints preclude the type of detailed review that a work such as this deserves, attention here will concentrate on highlighting key points and the general theoretical argument that underlies the whole project rather than on the case studies and individual essays. The reader is nevertheless advised to take the time to dwell on the country studies and conceptual chapters at length, as each offers a rich and varied panorama of the factors involved in freeing national political landscapes from authoritarian domination. Suffice at this point to note that all of the volumes are uniformly well written (although in some cases a bit dated), and several essays (such as those of M. A. Garretón on Chile and A. Przeworski on problems involved in the study of transitions) are simply brilliant.

Transitions from Authoritarianism completes a trilogy of modern studies on the dynamics involved in authoritarian regime emergence, maintenance, and demise that includes landmark works by Linz and Stepan (1978), O'Donnell (1973, 1982), Collier (1979), Malloy (1977), and Stepan (1970, 1979), a voluminous periodical literature, and dozens of monographical works of a related nature. In addition, in assessing the prospects for democratization in the postauthoritarian context, the participants in this project follow lines of inquiry previously developed by the likes of Rostow, Dahl, and Gramsci, to name just a few prominent others. As a result, the collection not only serves to complete the trilogy of studies on modern authoritarianism, it also develops the logical basis for future studies of democratic consolidation in countries emerging from authoritarian rule. It is therefore a landmark work in the full sense of the word, as it bridges the gap between two previously distinct literatures and opens the way for an entirely new field of study.

That some of those mentioned served as guiding forces of the Wilson Center Project should come as no surprise, as it represented that natural extension of ongoing work. Moreover, by combining forces and expanding the scope of inquiry across time, regions, and disciplines, the editors hoped to sharpen and

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expand the focus of discussion about the evolutionary dynamics of modern authoritarianism beyond the tired structuralist-behavioralist debates. Rather than the more "static" pictures normally associated with the study of stable political regimes, here the concern focuses on the dynamic interplay of structural and superstructural variables in the transition from authoritarianism, and in particular on the types of strategic interaction between various sociopolitical agents that lead to successful transitions. The highly fluid, reversible, and thus inherently uncertain, nature of this process requires that structural conditions be treated as general parameters or constraints rather than as determinants of action, within which events such as strategic decisions, actor miscalculations, and unanticipated sectoral responses all respond to, and then combine to form, a wide range of intervening factors in the evolution of specific transitional "games." As O'Donnell and Schmitter point out in volume 4, "unexpected events (fortuna), insufficient information, plasticity, and even indefiniteness of political identities, as well as the talents of specific individuals (virtu) are frequently decisive in determining outcomes. This is not to say that macrostructural factors are not still 'there' . . . but even those mediations are looser, and their impacts more indeterminate, than in normal conditions" (p. 5). Hence an underlying thread throughout this work is the acceptance of the type of behavioralist propositions often associated with Juan Linz, in which individual decisions and ongoing strategic interaction do matter even if generally constrained by objective structural conditions. The very fact that several countries sharing roughly similar "macro" traits (such as dependent capitalism) evolved different transitional "paths" (to use a term utilized by Stepan in volume 3) attests to the validity of such an approach. Structural conditions are important, to be sure, but analysis of their impact must be overlapped with conjunctural and other superstructural factors that broaden the range of indeterminacy with regards to possible outcomes. This perspective serves as a common starting point for all of the essays in the collection, and is most evident in volume 3, in which the chapters' focus sequentially narrows from international variables to the two most important actors in these transitions, military and business elites.

Another common point of agreement is that the transition away from authoritarianism is most often an internal process, in terms both of its initiation and its subsequent development. As Whitehead and Stepan show in volume 3, a wide variety of external factors (ideological, economic, military, etc.) and actors (government and nongovernment agencies, international organizations, private interests, etc.) all play a role in facilitating different types of transition, but in most cases they are not the determinate factors. Only in West Germany was democracy directly imposed by an external agent (in the form of the Allied forces of occupation), and external events such as defeat in war at best serve as precipitating events in the collapse of authoritarianism (as the examples of France and Italy in 1943-44, Greece in 1974, Portugal in 1974, and Argentina in 1982 suggest). In each case, internal tensions within the authoritarian coalition and rising domestic opposition had already manifested themselves to the extent that external factors merely served as catalysts for change. The point is that while certainly important, more often than not external actors and events may contribute but cannot of themselves bring down an authoritarian regime; that is the province of domestic factors and agents.

Another key issue is the differentiation between authoritarian regime liberalization and democratization. As Przeworski notes, regime change begins the moment elements of the dominant coalition break rank and seek external sources of support among previously excluded sectors of society. Liberalization refers to a gradual "softening" of authoritarian rule, characterized by the incremental extension of civil liberties and individual and sectoral rights of political voice and participation. However, regime change does not always entail liberalization and can in fact merely imply movement to other, often harsher forms of authoritarianism. This process, and the possibilities for liberalization, depend on the strategic dynamics within and between the regime and opposition. In particular, they depend on the interplay between authoritarian hardliners (*duros*) and softliners (*blandos*) on the one hand, and their evolving relations with moderate and maximalist sectors of the opposition on the other. Where *duros* or maximalists prevail, the chances of liberalization are slim.

Where *blandos* or moderates outweigh extremists on each side, the prospects for liberalization improve. It should be underscored that actors do not remain locked in any single position and can in fact move from one to another depending on circumstances. The point is that for a variety of reasons (economic success or failure, achievement of security or political objectives, quest for legitimacy or expanded bases of support) the dominant coalition begins to erode, and authoritarian actors eventually look to civil society for alternative bases of support. Depending on the character and objectives of those who respond to this "opening," authoritarian liberalization may or may not occur.

Democratization refers to the expansion of civil society, reflected in the posing of previously excluded group demands upon the authoritarian incumbents, in the reforging of political identities, and in the horizontal expansion of social networks comprised of both traditional and newly aggregated collective agents. This "resurrection of civil society" (to use O'Donnell's phrase) may or may not parallel authoritarian liberalization, and in fact can either follow or precede it. The latter situation can be considered a "bottom-up" process of transition, and is most often evident in cases of authoritarian collapse, as Italy, Portugal, and, most recently, Argentina suggest. On the other hand, where democratization follows or parallels liberalization, this can be considered a "top-down" process of transition in which authoritarian power is incrementally relinquished or devolved rather than surrendered or lost. Post-Franco Spain and Brazil provide the most often cited examples of this type of transition.

There is a major irony involved in the move toward democratization, because the need to mollify authoritarian (particularly hardliner) concerns about the postauthoritarian context leads the democratic opposition to move beyond mere moderation and accept limits on the extent to which democratization can occur. More specifically, this involves limiting democratization to the procedural or formal level (party competition, open elections, etc.), and avoiding rapid movement toward more substantive democratization at the societal and economic levels. Moreover, it also often involves ethical compromises when addressing the "legacy" problem caused by military viola-

tions of civil rights and other transgressions committed during their tenure in power. This in turn also involves the extent of militarization of the authoritarian regime, as Rouquié notes in volume 3. Only in Greece and Argentina were any officers brought to trial for crimes committed during their rule, and in each case prosecution was limited to the leaders of the regime itself, not their subordinates. In fact, *de facto* amnesty has been granted the military qua institution in Argentina, Brazil, and Uruguay, three countries in which human rights violations were most extensive, in order to lessen the possibility of a military-led authoritarian regression. As Garretón underscores, this could well be the price paid for freedom in Chile in the future. The basic issue involved here is that of extending certain sectoral guarantees to actors that would otherwise be disposed to accept the continuation of authoritarianism, in an effort to ensure the transfer of power and prevent an authoritarian regression in the near future. The result may be simply the substitution of a "soft" dictatorship (*dictablanda*) with a "hard" or "limited" democracy (*democracia*) that places serious constraints on sectoral participation and efforts at substantive change. That this poses fundamental problems for democratic consolidation should be obvious.

Another important influence on the character of the transition undertaken is the degree of institutionalization of the authoritarian regime. Long-lived, well-institutionalized regimes such as those in Mexico, Spain, and Brazil leave organizational legacies that shape the content of the transition process in a way that is not evident in less institutionalized authoritarian regimes. Ultimately, though, the transition from authoritarianism hinges on the actions of two social actors that constitute the major "swing" votes in such processes: the bourgeoisie, particularly its transnationally connected upper fractions, and the military hierarchy. Only when these actors decide, for reasons of expediency, self-preservation, or altruism, to support efforts to liberalize, does the path towards democratization become feasible. Until that point, the prognosis for a successful transition to genuinely open forms of government remains uncertain at best. It is here that the issue of political "pacts" becomes important, since it is "horizontal" elite agreements that serve as the

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vehicles through which mutual sectoral guarantees are tendered and the terms of the ensuing political competition are fixed. In fact, evidence suggests that "pacted" democracies such as those of Spain, Italy, and Venezuela are the most viable democratic forms to emerge in the postwar era, given the levels of social mobilization and organizational complexity that now characterize many late twentieth-century societies. However, subsequent democratic consolidation may well depend on the degree to which these initial elite pacts are vertically "deepened" by incorporating agents of previously excluded subordinate groups, something that rests on material, as well as nonmaterial, conditions of a conjunctural and structural nature (such as foreign debt or ideological conflict).

In summarizing the project's findings, O'Donnell and Schmitter use the analogy of a multilayered chess game to characterize these types of transition. Though a bit overdrawn, the analogy well captures the multiple and intertwined levels of analysis, varied types of strategic interaction, and myriad uncertainties that are integral components of such processes. In all cases, the outcome is very uncertain almost to the end of the "game," is extremely susceptible to reversal, and hence is totally contingent on the sequential outcome of conflicts lacking clearly established rules. However, one area that is not covered in their analysis is the role of learning processes in these transitions. After all, even chess players learn from past experience in order to improve their chances in the future. Thus it would have been worthwhile considering the role of learning processes in both civil society and regime incumbents in the move away from authoritarianism. The waves of liberalization-democratization that swept southern Europe in the 1970s were followed by a similar wave in South America in the 1980s. Did not regime incumbents as well as opposition groups in the latter look to the European experience for lessons on how to go about negotiating with each other? The steady references to "social pacts" on the one hand and to the avoidance of "another Nuremberg" on the other, suggest that this might in fact have been so. Did not the timing of the Brazilian *abertura* in 1974 have some relation to what was occurring at that time in southern Europe? Certainly Pinochet in Chile has taken heed of the recent

events in neighboring countries, as have his opponents (in fact, some have suggested that Pinochet has abandoned the original Brazil-inspired model of gradual liberalization in favor of a Francoist vision of long-term personalist rule, something which may or may not give comfort to his opponents). In any event, this is one issue that remained absent from the collection, which, given its other attributes, is a small but surprising omission.

In summary, and recognizing the very superficial treatment awarded it here, this is a collection of essays that will serve as major elements in the study of comparative politics for years to come. Rich in detail, subtle in analysis, nuanced in presentation, and extensive in both its theoretical grounding and comparative scope, this is a must read for any serious student of the discipline.

PAUL G. BUCHANAN

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Asian Political Institutionalization. Edited by Robert A. Scalapino, Seizaburo Sato, and Jusuf Wanandi (Berkeley: Institute of East Asian Studies, University of California, 1986. x, 312p. \$20.00).

This is the second of a three-volume study of the major issues confronting the peoples of Asia. The editors initiated the project in 1983 with three workshops focusing on economic development, political institutionalization, and security problems. After the customary revision and a second meeting in 1984, the conference papers were published in the Research Papers and Policy Studies series of the Institute of East Asian Studies at Berkeley.

The book opens with an introduction by Scalapino, who defines political institutionalization as "the process whereby a political structure is made operational in accordance with stipulated rules and procedures, enabling regularized, hence predictable, patterns of political behavior." Under ideal circumstances, institutionalization discourages personalized rule and "makes orderly, evolutionary change possible" (p. 1). It is clear that institutionalization, as defined here, promotes political stability and development. In this regard, Scalapino's views closely parallel those of Samuel Huntington.

The U.S. editor's conceptual framework is largely in accord with the theoretical assumptions of the authors of the 13 chapters in this book. Their primary emphasis, however, is not on formulating theoretical expositions. They describe, explain, analyze, and forecast regarding the party system, the bureaucracy, the military, and the leadership style in the country or area of their expertise. C. Chung traces, with a critical overtone, the development of "monocracy" in North Korea, and D. Pike dwells extensively on the failings of the existing party structure in Vietnam. Scalapino presents a comparative analysis of "Asian Marxist-Leninist Systems" in North Korea, China, and Vietnam and foresees the rise of technocrats and decentralization. Sato's interpretive essay on Japan's party system covers the last century and warns that institutionalization does not mean adoption of Western-type democracy. S. Han analyzes the "bureaucratic/authoritarian system" of South Korea and hopes for an eventual liberalization, albeit within a basically authoritarian context. D. K. Emerson emphasizes the role of the state in Southeast Asia and singles out adaptability as the most useful variable in discussing institutionalization. T. Shiraiishi notes the divergent patterns of the praetorian military in Thailand, Burma, and Indonesia. A positive appraisal of Soeharto's efforts for evolutionary political development in Indonesia, the "New Order," is offered by Wanandi. L. B. Hiok's focus is on the People's Action Party in Singapore, while Z. H. Ahmad explains the reasons for evolutionary stability in Malaysia. The succession of military coups in Thailand since 1932 is described by C. Samudavanija who optimistically notes the stabilizing influence of "social institutions" such as monarchy, religion, and so on. C. G. Hernandez contrasts, in a tone tinged with despair, Marcos's rule with the tradition of civilian supremacy in the Philippines. In the last chapter, M. Weiner examines the contrasting patterns of institution building in India, Pakistan, and Bangladesh.

The preceding one-sentence summaries of these chapters fail to do justice to the wealth of information and insights that only area specialists can offer. A student of Asian politics or international affairs will greatly benefit by reading them. On the other hand, if one were to look for an extensive theoretical exposition on political institutionalization in

Asia, this book is of limited value despite occasional intriguing and even provocative passages found in the chapters by Scalapino and Emerson, among others. Does the concept of institutionalization coalesce with, or is it a current version of, "modernization" and "development" of the 1960s? Is institutionalization desirable for all political structures? The title of this book whets one's intellectual appetite, to leave it only partially satisfied.

HAN-KYO KIM

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The South African Quagmire: In Search of a Peaceful Path to Democratic Pluralism.

Edited by S. Prakash Sethi (Cambridge: MA: Ballinger, 1987. xvi, 512p. \$29.25).

Power-Sharing in South Africa. By Arend Lijphart. (Berkeley: Institute of International Studies, 1985. x, 179p. \$10.00).

Chronic political crisis in South Africa produces a steady stream of prescriptive Western publications that argue the imperative and feasibility of a democratic solution to that country's racial tragedy. This literature offers negotiational and constitutional formulas for the construction of a peaceable and prosperous policy. It is sometimes strikingly presumptuous, as for example the much-publicized manifesto, *South Africa: The Solution*, in which Leon Louw and Frances Kendall argue for the adoption of a South African version of the Swiss canton system. It reflects a penchant for problem solving by outside observers concerned that escalating violence and repression may lead to even greater tragedy. Often suffused with wishful thinking, it tends to underestimate the magnitude of political and racial polarization in South Africa and imposes formulaic rationality on the volatile reality of social cleavage, coercive rule, and frustrated aspirations.

The two volumes under review feature a shared commitment to values of democratic pluralism. But they are not prescriptive manifestos. Prakash Sethi of Baruch College's Center of Management has edited a collection of 37 mostly short (5-to-10-page) essays that stress the centrality of economic forces, notably business, to the dynamic of social and political reform. In his overview chapter, Sethi

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sets forth his own perspective, arguing that foreign divestment, disinvestment, and trade sanctions can only serve to disable, not empower black South Africans. A series of general essays by figures like Senator Richard Lugar, Congressman Howard Wolpe, and Archbishop Desmond Tutu, interesting because of the authors' political prominence, is followed by a plethora of pieces arguing the pros and cons of economic sanctions. Unfortunately, as Sethi notes, spokespersons for the African National Congress, labor unions, and other black movements did not contribute to the volume.

Some of the *South African Quagmire* essays bring new insights to bear, for example the contrasting assessments concerning compliance by U.S. companies with the Sullivan Principles. Cumulatively, the essays are informative and surprisingly free from repetition. But most, while well-reasoned and clearly written, are too brief to be substantively or analytically rich.

The *Quagmire* volume is nowhere more weighted than in the chapters dealing with education. The government position is cogently presented by Minister of [Black] Education and Development Gerrit Viljoen. It is not matched by a contribution from educational leaders or analysts associated with or knowledgeable about the many black-led educational organizations establishing educational agendas, running scholarship programs, conducting bridging courses, organizing correspondence education, and otherwise challenging the existing order in a sector that is key to black advancement.

Handicapping the volume's utility for ongoing discussion of Western policy and South Africa's future is the fact that divestment, disinvestment, and sanctions measures taken during the past several years have sharply undercut the proinvestment option. It is now probably too late to test the hypothesis that more socially engaged investment would effectively promote social justice.

Arend Lijphart's *Power-Sharing in South Africa* takes as its point of departure that the "gravity and urgency of the South African problem" that have given rise to "a host of proposed solutions" would be best addressed through consociational politics. He rejects majoritarianism, as well as undemocratic or partition proposals as undesirable, spelling out

his reasons. He then defines and refines the concept of consociational democracy, taking pains to clarify what is and is not required to make such a decentralized system function and hold together a highly polarized society. He systematically addresses criticisms of consociational theory and argues that despite the violence and coercion in South Africa it is not unrealistic to advocate such a system. This is so, he states, because South Africa's "segmented" leadership will come to realize that "competitive behavior" is leading to conflict and disastrous consequences that reason dictates they avoid.

Power-Sharing sets forth consociational democracy as the only "possible" democratic solution for South Africa, while acknowledging that the "longer it is delayed, the more polarized the political atmosphere will become, the smaller the chances of setting up a successful consociation will be, and the more likely a violent confrontation will be." The importance of the Lijphart study does not depend upon the receding prospects of such implementation, however. It does not depend upon the triumph of consociational reason in South Africa. The heart of the monograph (chaps. 3-5) presents a definitive and clear definition of what is and is not essential to consociation. It stands as a theoretical and prescriptive statement of what a consociational approach to the resolution of communal conflict and polarization must entail. It will remain an invaluable reference for all those interested in consociation, whatever happens in South Africa.

JOHN A. MARCUM

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Understanding Political Development. Edited by Myron Weiner and Samuel P. Huntington (Boston: Little, Brown, 1987. 514p. \$15.25, paper).

Political scientists have queried what happened to the mainstream developmental theory that so influenced comparative politics during the 1960s and early 1970s. A response is contained in this anthology of essays, which assesses work on developmental theory over the past two decades. This volume is organized into four sections.

A first part focuses on the third world, with

a general essay on development by Samuel Huntington and regional overviews by Myron Weiner (on Asia, Africa, and the Middle East) and by Jorge Domínguez (on Latin America).

The second part comprises essays on political participation (emphasizing territorial groupings, ethnicity, and religion over the industrial working class and the peasantry) by Joan M. Nelson; agrarian politics (looking at peasants as rational actors) by Robert H. Bates; ethnonationalism (arguing that ethnicity patterns provide a means of understanding the development of nations) by Walker Connor; religion and East Asia (suggesting a return to culture and political behavior in contrast to a materialist and class view of politics) by Winston Davis; and social-psychological approaches to political development (questioning traditional emphasis on individual attitudes in contrast to corporate and collectivist traditions) by Ali Banuazizi.

Part 3 focuses on the once forgotten concept of state with essays by Peter Evans on foreign capital and its impact on the state in the third world; Eric Nordlinger on state autonomy; and Joel S. Migdal on the strong-weak state dichotomy. The very inclusion of these essays implies recognition that the state is a core concept in the study of politics and that it is time to move on beyond the belief that political system supplants the state, as once advocated by the Comparative Politics Committee of the Social Science Research Council (SSRC).

The final section contains an overview essay by Gabriel Almond, who asserts an ideological defense of past themes and mainstream political science, together with criticism of theorists who emphasize dependency, the state, and a class conception of politics.

This anthology of original essays is useful in several respects. First, it represents a long-awaited reassessment of ideas and literature that have long dominated the mainstream of political science. Second, it incorporates (largely in critical fashion) some of the dependency ideas that challenged the perspectives cast by Almond, James Coleman, and Lucian Pye two decades ago, ideas that were subsequently assimilated into mainstream political science. Third, it includes attention to the question of state. Thus the views of both older and newer scholars are linked in one volume. While there is reference to some of the views that have transcended the dependency school

of thought, the volume is limited by its failure to deal seriously with some new thinking, for example, theories of mode of production and internationalization of capital that have influenced other disciplines but have yet to impinge on political science, probably because comparative politics has not fully incorporated political economy approaches and alternative radical and Marxist perspectives.

Resistance to some of the new views is evident in the initial and concluding essays, by Huntington and Almond respectively. Huntington is rather conciliatory, however, as he shows how liberal, dependency, and Marxist theories all focus on poverty and injustice. He stresses the relevance of various cultures in an explanation of developmental patterns and calls for closer links between comparative politics scholars and area specialists. Whereas Huntington acknowledges that the model of the developed Western society—wealthy, equitable, democratic, stable, autonomous—may not be useful to other societies, Almond is explicit about his Western preferences.

Almond carefully reviews and defends the contributions of two decades ago, including the influence of Talcott Parsons, the work of Coleman, Pye, and himself, and the SSRC series on political development. He rebuts the attack of dependency writers against mainstream comparative politics by erroneously characterizing all dependency writing as Marxist in its emphasis on class struggle and Leninist in its attention to imperialism; then he shows—by ignoring or not being aware of the plethora of criticism independent Marxists and Leninists directed against dependency theory over the past 15 years—that dependency theory was not very good Marxism-Leninism. Almond rests his critique on the dependency approach as “a propaganda fragment of an ideology, a polemic against mainstream development theory” (p. 454). Indeed the dependency approach served as an alternative to the old mainstream ideas, but it should not be forgotten that it also was an alternative to orthodox Marxist-Leninist theory as espoused by the traditional pro-Soviet Communist parties throughout Latin America and the third world. For all its failings, it stimulated new thinking and ideas and a healthy debate among third-world intellectuals and practitioners alike, and Almond ignores the substantial theoretical and empirical contributions that

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followed in the wake of the dependency tradition. Instead he concentrates on North American scholarship about dependency, decrying the ideological assumptions and the oversimplification of prominent writings such as Peter Evans's study of Brazil. He goes on to attack the "metaphysical overtones" of the "neostatist" movement led by Evans, Theda Skocpol, and others, arguing that his own pluralist and structural-functional movements had already demystified and operationalized the concept of state. Finally, he concludes that mainstream comparative politics is not in crisis.

Almond's forceful presentation serves as a staunch defense of the mainstream. While it may convince some to follow in this tradition, it certainly will provoke others to seek new alternatives.

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Creating Opportunities for Change: Approaches to Managing Development Programs. By Louise G. White (Boulder: Lynne Rienner, 1987. xvii, 283p. \$32.00).

Listen to the People: Participant-Observation Evaluation in Development Projects. By Lawrence F. Salmen (New York: Oxford University Press, 1987. x, 149p. \$24.95).

Both books aim to help policymakers and public administrators improve the contribution that development programs or projects can make to the economic, social, and even political well-being of third-world people. Both were written with the support of international donor agencies, the U.S. Agency for International Development and the World Bank, respectively. Their similarities end there. The first, in the tradition of public administration, addresses management problems "from above," while the other offers us a view "from below." It adapts the anthropological method of participant observation to inform managers how to meet better the needs and wants of the publics they are expected to serve.

The first will be of more interest within the discipline of political science because of its subject matter and the literature it draws on. Good use is made of the work of many political scientists, some fairly well known in

the profession, such as Graham Allison, Robert Bates, Milton Esman, Elinor Ostrom, David Leonard, John Montgomery, and Aaron Wildavsky. I counted over 40 others whom I know, and there are probably more. Louise White brings in also the work of many other social scientists whom political scientists read—Argyris, Arrow, Buchanan, Downs, Hirschman, Lindblom, March, Selznick, and Simon, among others; her analysis is not only grounded in concerns of interest to political scientists, but she integrates them with concepts and conclusions from cognate disciplines. The book indeed provides a thorough review and critique of the current literatures that contribute to the multidisciplinary subject of development administration. It is a well-written statement of the state of the art, systematic enough to qualify as "science."

White makes a substantial contribution through her delineation and analysis of six approaches for improving development administration. The book offers something like a factor analysis of the literature, parsing it into six reasonably distinct and enlightening schools of thought about how to improve the management of development. At first I resisted her classification because I found the "vector labels" (my term, not hers) not conceptually clear (as usual with factor analyses) and because the categories had some lingering commonalities. But ultimately I was won over by the typology because the differences delineated are significant.

Each approach is discussed in its own chapter, including the relevant literature and also the criticisms and limitations associated with that approach. While White may have some favorites among the six, she presents each even-handedly and constructively, showing how the approach can be made most useful and complementary to the others. Each has some similarities or parallels with others, but enough substantial distinctions are detailed to make this taxonomy one to be reckoned with for years to come.

Space limitations do not permit a discussion of each of the approaches. The one that most intrigued me was "management by anarchy," inspired in part by Charles Lindblom's work on "muddling through" 25 years ago but advanced furthest by James March and associates in the last 10 years. I was struck by the possibilities this approach offers for linking

with the work emerging from many disciplines on "chaos" (see James Gleick, *Chaos: Making a New Science* [1987]). "Wicked problems," for example, as identified by Rittel and Webber ("Dilemmas in a General Theory of Planning," *Policy Sciences* 4 [1973]:2) should be more amenable to analysis and action if understood in terms of the dynamics of "branching." The qualitative mathematics of "fractals" offer an alternative to the "linear" concepts and methodologies so dominant but inappropriate for most problems of management today. But each of the other approaches as mapped out by White also offers some new intellectual and experimental possibilities, so that public administration, relatively dormant theoretically for many years, could become more exciting as a field.

It is in its own way quite a breakthrough, for an anthropologist, Lawrence Salmen, to become so engaged in the task of improving bureaucratic approaches to working with the poor and for the World Bank to fund such a grass roots endeavor as *Listen to the People*. Political scientists who study micropolitics in the third world will find it of interest for its systematic exposition of the methodology of participant observation. Others may be interested in its documentation of the many ways "politics" intrude in the planning and implementation of development projects. Salmen lived in urban slum and resettlement areas in Ecuador and Bolivia to test and develop the methodology for World Bank use. He shows how donor agency ignorance of political factions, histories of past confrontation between government and local groups, patron-client systems in the barrio, electoral competition, local jealousies, and so on, impeded sensible project design and successful project implementation. Enough benefit was demonstrated from this investment in ethnographic data gathering (about .1% of total project cost) that the methodology was tested in several other countries and will now be used more widely by the World Bank, at least for urban development projects.

NORMAN UPHOFF

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Planning and Federalism: Australian and Canadian Experience. By Kenneth Wiltshire (St. Lucia: University of Queensland Press, 1986. x, 333p. \$42.50).

This is not a book for beginners. As the author states at the outset, it requires "complete familiarity with the history and institutions of Australian, and to a lesser extent Canadian, federalism" (p. 4). It also requires some familiarity—and a lot of patience—with the manifold theories and models of public administration and planning. Thus it is a book for specialists; but this is not to say that it would not address and add to an important aspect of the study of federalism in general.

Wiltshire's central message is simple and clear: planning and federalism are essentially incompatible. The division of powers in a federation is "a fundamental hindrance to the attainment of public sector planning on a national basis" (p. 108). This cannot be otherwise because planning requires rationality and predictability, while federalism means fragmentation and uncertainty (p. 259). Since the division of powers cannot be eradicated, the incompatibility of planning and federalism can at best be "mitigated" through a number of reforms aiming at the reallocation of functions and responsibilities among the different levels of government and at the improvement of the "linkages" that in modern federal systems have been placed in between these divided jurisdictions: intergovernmental, and especially financial, relations.

The design of the book is equally clear and straightforward. Chapters 1-4 deal with the concepts of planning and federalism, with the planning experience in Australia (or lack thereof), and with a comparison of the federal systems in Australia and Canada. Here Wiltshire provides useful descriptions of the historical genesis of both federations, and of their major institutions. In chapter 5 he examines more specifically the division of powers in both countries, which eventually leads him to a lengthy theoretical discussion of functional reallocation. Chapters 6-8 then deal with intergovernmental agreements, the basic dilemmas of federal finance, and the specific problems of conditional funding. The "Conclusion" is really a misnomer, because this is where Wiltshire develops extensive thoughts about federal

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reforms that would improve planning capacities.

The entire study is not so much a comparative analysis as a well documented critique and conceptualization of planning in federal systems. Because of their constitutional similarities, Australia and Canada supply the empirical background. Nevertheless the author provides plenty of data based on government documentation, literary review and a substantive number of interviews. Here the specialist will find useful information on patterns of intergovernmentalism, fiscal transfers, and conditional funding in both countries, although the material is presented country by country rather than aggregated into a comparative analysis. What amounts to rather difficult analytical reading, interspersed with frequent conceptual excursions, is facilitated by helpful summaries at the end of sections and chapters.

This conceptualization is the most problematic aspect of the book. Delving into a rather dated discussion on "coordinate" federalism, "duality," and the "existence of a balance of power" at the outset (pp. 70-84), Wiltshire later has to present as a result of the interviews he conducted what in reality has been standard wisdom for quite a while: that due to "growing interdependence" federations are "no longer coordinate" but have become cooperative and intergovernmental, or what he awkwardly calls "organic" (pp. 127-28). Far

from being coincidental, the choice of terminology helps to clarify the underlying rationale of the entire book. As "planning means coordination" and "coordination means hierarchy" (p. 31), federalism is incompatible with planning precisely because of its "lack of [hierarchical] coordination" (p. 84). This is where the student of federalism, among others, has to take exception. Only from a very narrow view of planning and public administration is it possible to argue that coordination means hierarchy. In the context of federalism and democracy, however, this is a contradiction in terms.

If there is a lack of coordination in the Australian and Canadian federations, it is more likely a lack of nonhierarchical and cooperative coordination caused by the hierarchical planning bias of government bureaucrats trained in the centralized Westminster tradition. Not surprisingly, Wiltshire's competent analysis of the intergovernmental linkages in modern federal systems leads him to the self-evident observation that the "classical" and "dual" theories of federalism are dated. Perhaps it is fair to say that in a modern world of fragmentation and interdependence the "classical" theory of linear and hierarchical planning is just as dated.

THOMAS O. HUEGLIN

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INTERNATIONAL RELATIONS

Terrorism, U.S. Strategy, and Reagan Policies. By Marc A. Celmer (New York: Greenwood, 1987. x, 132p. \$29.95).

The now vast literature on political terrorism abounds with proposals their authors think governments should adopt in order to prevent terrorist events from taking place and for defeating the aims of their perpetrators. It also includes many commentaries on the various measures governments have already used to achieve these goals; usually these are accompanied by assessments of their effectiveness. Compared to these efforts, the body

of writing whose focus is not on antiterrorist policy but on the mechanical or bureaucratic aspect of the problem is very limited. The book under consideration here seeks to fill this gap by reviewing the various organizational, civilian, and military arrangements and rearrangements the U.S. government has experienced as recent presidents have sought to combat terrorism.

Given its contents, an appropriate subtitle for this volume might be *How an Elephant Fights a Mouse*. As related by the author, the technique goes something like this: After the elephant is bitten, it emits a loud trumpeting

sound. This is followed by a long pause as the gigantic animal contemplates further reaction. The elephant glances toward other members of the herd who were also bitten. After a time, the elephant's brain sends a series of messages, known as EO's and NSDs, to the rest of its body. Tusks are raised and lowered, the elephant's legs stomp the mud until it is satisfied that the mouse has been sufficiently impressed to cease its attack. There is another pause until the mouse bites again. This time the trumpeting becomes louder, but the behavior that follows looks remarkably like the earlier reaction. The mouse, though injured, lives on. Apparently the elephant has even grown fond of the little creature. Why else would it lately have given the mouse a small portion of cheese?

Moving from the realm of parable to a more prosaic level, it seems clear that the organizational response to the problem posed by political terrorism has been event-driven. Beginning with the Nixon administration's reaction to the Munich and Lod Airport massacres in 1972, a succession of U.S. presidents, with some congressional participation, have created policy-making and bureaucratic frameworks within which to mount antiterrorist campaigns. Each new administration has modified the arrangements created by its predecessor. Also, there has been a rapid rate of turnover in the personnel given the task of organizing the response. For the most part these changes have been stimulated by presidential dissatisfaction, itself brought on by renewed terrorist attacks on U.S. citizens and facilities.

This is the story told in the book. The volume is marred by its author's unrelenting use of bureaucratic jargon, replete with the usual alphabet soup of agency names. A more thorough editorial effort would have helped.

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The Vatican, Islam, and the Middle East.

By Kail C. Ellis (Syracuse, NY: Syracuse University Press, 1987. 364p. \$39.95).

This volume consists of essays presented at a conference on the subject of the Vatican, Islam, and the Middle East at Villanova

University. Conference collections can present certain difficulties. The generality of the title allows for the coverage of a wide variety of themes yet conveys a certain lack of focus. Father Kail C. Ellis's introduction categorizes and provides a conceptual framework for the conference papers. He makes it clear there are certain things this book will not do: (1) This collection will not focus on the Holy See only as a classical sovereign state model. Although the Vatican may be viewed as a transnational actor in the traditional political science sense (see J. Bryan Hehir's article, 109-24), it is principally by virtue of its moral authority that it plays a significant role in the Middle East today. (2) The reader should not expect new insights into the Vatican's foreign policy-making processes. They remain a mystery. Ellis's broad claim that this volume offers the reader a multifaceted, coherent approach to the issues of Christian-Muslim relations and the Vatican's approach to the Middle East masks a disparity of topics. Stylistically, the chapters range from comparative theosophy, traditional political analysis, and historical, conceptual, and philosophical analysis to case studies of Christian-Muslim relations. Finally, the quality of the chapters is uneven. All tend to focus more on description and categorization than analysis or significance. The most original is James A. Bill and John Alden Williams's comparison of similarities in the doctrinal, structural, and sociopolitical characteristics of Shi'i Islam and Roman Catholicism. Especially valuable is the authors' comparison of the Catholic and Shi'i models of politics and the state. Bill and Williams observe certain similarities between populist Islam as seen among Shi'i communities and liberation theology as practiced by some Roman Catholic clergy, especially in Latin America. Hehir examines the role of the Vatican as a transnational actor operating at three separate levels of analysis: center-periphery, periphery-center, and periphery-periphery. This is a convenient taxonomy for analyzing the role of the Vatican as a political actor, not a nation yet having its own interests in the Middle East. Some articles are weak in conceptual clarity or depth. Joseph L. Ryan's article is a review of contacts between the Holy See and Jordan from 1953 to the present. He defers treatment of the wider political context—as well as analysis of the significance of

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each contact—to the political and diplomatic historian. Yoakim Moubarac seeks to apply the concept of minority to Lebanon but his differentiation among minorities, unionists, and prototype communities (those who created Lebanon) lacks in precision as well as *raison d'être*. Moubarac's thesis that the Maronites' political plan sought to create an egalitarian and modern Islamic-Christian society to serve as a prototype for the nation-states of the Arab East may be influenced by his own background as a spokesman for the Maronite church. Moubarac's article is a philosophical and conceptual treatise lacking in internal analytical coherence. In the chapter on Pakistan, Joseph Cardinal combines the style of an historical essay with inspirational suggestions to Christians for survival in an overwhelmingly Muslim state.

The intent of the conference (to examine the new Catholic posture toward Islam) makes this collection useful to the general reader and student, if not to the researcher. The Catholic church's emphasis on Christian-Muslim cooperation and dialogue derives from the statement, *Nostra Aetate*, issued by the Second Vatican Council (1965). This declaration stressed the common beliefs of Muslims and Christians and sought to discredit a stereotyped view of Islam as a crusading religion of power and ignorance. It is as a religiopolitical institution that the Catholic church enters into dialogue with other religions, as a "witness of faith in the world" (p. 2) and as a transnational actor.

The relationship of Catholicism and Islam has attracted little attention in the literature. One exception is George Irani's recent book, *The Papacy and the Middle East* (1986). Yet no other area of the world has been the focal point of such prolonged Vatican interest. The Vatican maintains diplomatic relations with several Arab and Islamic states and exerts moral authority on issues that vitally affect Christians and Muslims.

Four aspects of the progress of Catholic-Islamic cooperation are examined in this volume: (1) Islam and Western Christianity, (2) Vatican diplomacy and the Middle East, (3) the Vatican and Muslim-Christian relations in the Middle East, and (4) resurgent Islam and Christian-Muslim relations. Although the quality and originality of the chapters vary considerably, those by George Emile Irani and

Fred J. Khouri provide useful background and information on two of the most intractable problems of the volatile Middle East: the Israeli-Palestinian conflict and Jerusalem. According to Irani, the Holy See has been sympathetic to the plight of the Palestinians from the beginning of the Israeli-Palestine dispute. This position was motivated by the Vatican's concern for the fate of Catholics in Palestine and the humanitarian needs of the Palestinian refugees. Khouri traces the Vatican's concern for Jerusalem from the British mandate period to the post-1967 phase. During this time, the intent of Vatican policy shifted from internationalization (*corpus separatum*) to one of guarantees for freedom of worship, respect for the Holy Places, and preservation of the Christian presence in the Holy Land. Khouri warns against ignoring the status of Jerusalem, since no lasting solution to the Middle East conflict can be reached without an acceptable agreement on East Jerusalem.

The last two sections of this book consist of case studies. Individual authors discuss the status of small, marginal Christian communities in Pakistan and India—the first overwhelmed in a predominantly Muslim state, the second in a Hindu-dominated but religiously pluralistic state. Others examine the reverse of that situation: the status of the quasi-insurgent minority Muslim community in the Catholic Philippines. Thus, geographically as well as analytically, the topics explored by the various conferees ranged widely.

Perhaps the greatest contribution of this collection is its emphasis on the role of the Vatican in promoting mutual understanding between Muslims and Christians. In recent times, the Iranian revolution and the Shi'ite movement in Lebanon have contributed to distorted and stereotypical images of Islam. The essays in this volume serve to present Islam in its proper perspective as a religion of piety, patience, charity, and human concern that claims over 800 million adherents. Ellis also points out that Islam is not just a religion confined to distant lands but a U.S. religion. By the year 2015, it may become the second largest religion in the United States (p. 21). The insights and dynamics presented in this book underscore the importance of establishing a sound basis for Christian-Muslim relations. As the editor states, such mutual understanding can be achieved through the common theological

heritage of one God and the common Abrahamic tradition of Judaism, Christianity, and Islam.

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War and Intervention in Lebanon: The Israeli-Syrian Deterrence Dialogue. By Yair Evron (Baltimore: Johns Hopkins University Press, 1987. x, 246p. \$39.50).

This book adds, in a captivating way, one more piece to the puzzle of the Lebanon war of 1982. It reinterprets the politics of Israel and Syria in Lebanon between 1975 and the present through the prism of mutual deterrence and is a scholarly testimonial to the often-repeated notion that Israeli-Syrian relations, though hostile, are based on a pragmatic, almost mathematical, set of operational guidelines.

The material covered in yet another volume on the Lebanon issue is not new. However, Evron makes an original contribution to the role of regional politics in Lebanon.

The perspective used by Evron strips the complex Israeli-Syrian relationship to its core. This is both an advantage and a liability. On the one hand, one can clearly see the evolution of a mutual deterrence game played by two local hegemony on the Lebanese scene, replete with definitions of interests, tests of wills, escalations and deescalations, although, as the author reminds us, the success of deterrence "is usually much more difficult to identify and prove than its failure" (p. 176). Yet one cannot but realize that if this "deterrence regime" has succeeded, it is in part because the presidency of Syria's Assad has endured the test of time. In this sense, the deterrence equation is one tying Israel and Assad rather than Israel and Syria.

On the other hand, one cannot but wonder whether it is possible to reduce the multifaceted protracted social conflict raging in Lebanon since 1975 to what is essentially an Israeli-Syrian dyad. To what extent, we must ask, is war and peace between Israel and Syria purely the factor of a deterrence equation rather than the outcome of different competing domestic, regional, and international images of security and national identity? This is particularly true when both powers are entangled

in a country with no authority of its own. This was made clear during the clashes over Zahle (which Evron euphemistically refers to as the "Zahle encounter"), which led to Israel's downing of Syrian helicopters, and Syria's introduction of surface-to-air missile batteries into Lebanon's Bekaa valley. Then, the records show, Lebanon's Bashir Gemayel was intent on provoking a clash between his Lebanese forces and Syrian troops, thereby forcing Israel to come to his support. In this case it became incumbent upon Israel to respond militarily.

In 1978 a different but equally important chain of events had led to the previous major Israeli incursion in Lebanon. That incursion was primarily in response to a Palestinian guerrilla raid, which had resulted in 35 Israeli deaths, most of them civilian. Ironically, the chain of events that led to the "Litani Operation" was triggered *because* of the deterrence regime. In south Lebanon the PLO had become a source of worry to Israel because "Syrian forces were not allowed into the south by the Israeli deterrence threshold; the Lebanese government was unable to extend its authority into the south, and the local Shi'i population was incapable of resisting PLO movements there" (p. 71).

On Israeli and Syrian deterrence, Evron states that "it is difficult to prove whether the threat to escalate the conflict has been the basis of Israel's successful deterrence of a limited Syrian initiative" (pp. 197-98). It is certainly no coincidence, however, that Lebanon's border with Israel became an active guerrilla zone, and that the incidence of cross-border clashes rose sharply at the same time as the development of the deterrence regime. One can therefore suggest that as the cost of direct conflict between Syria and Israel outweighed its benefits, the nature of the conflict shifted to a different level. In Lebanon, Syria and Israel could confront and deter one another through their allies while minimizing the risks of outright war. Incidentally, that regime is still operational.

It is a little unfortunate that the editors were rather inconsistent with the nomenclature. Rather than being transliterated from Arabic into English, Arab names are often written as they sound in Hebrew, and even then, printed inconsistently. Thus, Camille Chamoun also appears as *Kamil*, Haddad is alternatively

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written *Hadad*, and Lieutenant al-Khatib is referred to as *Hatib*.

Despite this minor objection, Yair Evron's *War and Intervention in Lebanon* is a welcome addition to the conflict management and deterrence literature.

EDWARD E. AZAR

University of Maryland, College Park

Normative Politics and the Community of Nations. By Haskell Fain (Philadelphia: Temple University Press, 1987. xii, 244p. \$32.95).

The aim of this book is to sketch a new theory of international ethics, one that differs from existing theories in several ways. Unlike political realism, Fain's theory does not arbitrarily elevate national claims above those of the international community. Nor does it challenge the moral legitimacy of national sovereignty in contrast to the view that boundaries count for little in determining international moral obligations. Fain is willing, as realists and globalists often are not, to talk about the rights and duties of states. But while most theories of international obligation derive such rights and duties either from consent ("contractarianism") or from the constitutive practices that define and establish the community of nations ("legal conceptualism"), Fain rejects these derivations as spurious. The true source of international obligation, he argues, is the existence of certain "morally necessary tasks" that can be performed only at the international level. The nations of the world comprise a moral community because—and only because—their cooperation is required in order to realize these vitally important goals.

Fain devotes a good deal of effort to undermining contractarianism, which he thinks is defective because it fails to recognize that the goals of a community are prior to the rules and rights they legitimize. The right to property, for example, is something that must itself be morally justified in relation to the good of the community. Political communities are not founded upon the inviolability of contract but upon the duty to perform morally important tasks. Indeed, the very idea of a morally binding social contract presupposes the idea of morally imperative purposes or tasks.

Fain's attraction to a goal-oriented, or teleological, approach to ethics explains his skepticism regarding legal conceptualism as well as his rejection of contractarianism. Legal conceptualism is the view that rights and duties are derived from antecedent rules and that there can be no political community apart from the legal rules that constitute it. Fain at times seems to favor this view, since he argues that contracts "presuppose that one is not in a state of nature with regard to those with whom one wishes to contract" (p. 89). But he does not take this to mean that contracts presuppose rules and procedures establishing the institution of contract. Fain thinks such reasoning is mistaken. Morally speaking, it is not rules and procedures that undergird political communities and their institutions, but purposes. Morally justified powers are derived not from rules, which are instrumental and secondary, but from the primary moral purposes that provide the root justification of all legal and political arrangements.

Fain's own view, then, is that political communities are constituted by their "proper tasks" (p. 199). One example will suffice to suggest the sort of difficulty with which this theory must deal. "A chess club, which is governed by a steering committee," Fain writes, "is a political community that is constituted by 'the task' of seeing to it that its members have the opportunity to play chess, and if the steering committee uses the money of its members for any other purposes, it is acting *ultra vires*" (p. 199). But to act *ultra vires* is to act in a manner beyond the powers authorized by law. The legal conceptualist will therefore object that we can't determine whether a steering committee is acting *ultra vires* until we know the charter or constitution of the association. In deciding whether a government is acting within its authority one consults these rules, not what some undefined body of opinion deems to be its proper tasks. Furthermore, as everyone knows, associations often change their goals.

Fain's central thesis is that if there are morally essential tasks that can be pursued only through international cooperation, there exists a global political community whose reality and *raison d'être* derive from the moral obligation to perform these tasks. What are these tasks? According to Fain, the international community is constituted by such tasks as

eliminating war, achieving a "more satisfactory" distribution of the earth's resources, and preserving these resources for our descendants (pp. 184, 204-5). The general problem, of course, is to determine which tasks are moral tasks, tasks that are constitutive of the community. If the moral authority of law is accounted for by its "communal desirability," how does one determine what is desirable for the community? Fain acknowledges the difficulty: "I do not have a general theory that could explain, in a rigorous way, exactly which of the possible tasks of the state could be moral tasks. . . . I do not believe there can be such a theory, for it seems to be history, rather than theory, that determines which tasks are morally important for a given political community" (p. 201).

As a moralist, Fain repeats some common complaints about the present international order. I am not convinced that he has succeeded, as a theorist, in providing a solid foundation for these complaints.

TERRY NARDIN

University of Wisconsin, Milwaukee

Nuclear Deterrence, Morality, and Realism.

By John Finnis, Joseph M. Boyle, Jr., and Germain Grisez (New York: Oxford University Press, 1987. ix, 429p. \$39.95.)

Is it proper for the governments of the United States, Great Britain, and France to threaten millions of Soviet citizens with nuclear incineration in order to dissuade the Soviet government from aggression that might otherwise occur? These three Catholic writers argue thoughtfully, relentlessly, and persuasively that nuclear deterrence is not right, that it is flatly immoral because it egregiously offends the "common morality" of Western civilization.

British lawyer Finnis, U.S. moral philosopher Boyle, and U.S. theologian Grisez base their condemnation on nuclear deterrence's clear violation of this civilization's absolute prohibition of the deliberate killing of innocents. As the authors show, nuclear deterrence involves the continuously expressed

threat to wipe out the cities of the USSR, a conditional intention to kill millions of Soviet civilians if their government "makes it necessary." And this murderous intention is backed up with appropriations, deployment of the vast arsenal of nuclear tools, elaborate planning and preparations, indoctrination and training of thousands of military personnel who share the intention, and some predelegation of authority to enable subordinates to unleash the devastating nuclear strikes; so nuclear deterrence cannot be excused as the chief of state's personal bluff, a threat that might be without sin because he or she might not really mean it and might not actually execute it "on the day."

The authors reject consequentialist arguments that defend deterrence as a necessary, so-far effective, and therefore morally justifiable means for defending respectable Western values in the current emergency situation brought on by the menacing behavior of the repugnant Soviet regime. To the consequentialist suggestion that nuclear deterrence is preferable to prompt nuclear disarmament that might be followed by Soviet destruction or domination of the West, the authors elaborately explain how and why "the consequences of deterring and disarming are unpredictable, the probabilities of their occurrence are unknowable, and so the net utilities are incalculable" (p. 243). Consequentialism is revealed as an incoherent methodology. The notes following these middle chapters are a gold mine of serious grappling with the best points that consequentialists have been able to make.

The authors do not shrink from the possibility that renouncing nuclear deterrence might result in Soviet domination. In fact, they probably exaggerate the awful things Moscow would be likely and able to do to the West if one of these days the Kremlin's bosses awoke to find the West's nuclear forces had been abandoned, especially if the West had concurrently increased its conventional military forces, developed various forms of alternative defense and civilian resistance capabilities, and sharpened its economic and diplomatic instruments. But even if renunciation and abolition meant Soviet domination, the authors are convinced that nuclear deterrence must be condemned and abandoned as a morally prohibited means, however good the ends and unappealing the possible consequences. Civilized

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people are expected to suffer evil rather than to inflict or even to threaten or intend it.

Much of their discussion of the philosophical and theological origins of their central principle absolutely forbidding the killing of innocents could support a more thoroughgoing pacifism rather than their continued acceptance of just war doctrines that sound dubious alongside unabashed assertions about the sanctity of human life, the uncertain purposes of even "good" wars, and Christian faith in God's unknowable plans (part 4). But if these two chapters provoke question marks in the margin, the essay as a whole is an excellent, sophisticated contribution, perhaps the best that has been set out among the many recent books on this difficult but crucial subject.

JAMES A. STEGENGA

Purdue University

Refugees in International Politics. By Leon Gordenker (New York: Columbia University Press, 1987. 227p. \$30.00).

Leon Gordenker laments the steady increase in forced migration but finds intrinsic value in new forms of international cooperation to address refugee problems. His study briefly charts the growth of intergovernmental and private refugee institutions in the twentieth century before classifying the various causes of and responses to refugee emergencies. Initially major international wars were the primary cause, but since 1950 civil war, human rights violations, technological advances, and the consolidation of national power in former colonies have generated the major outflows.

Extraordinarily Machiavellian regimes have deliberately expelled selected groups of their citizens. Countries receiving forced migrants may pursue their own political ends by settling refugees on the border in bases calculated to destabilize an unfriendly neighbor. Permanent intergovernmental refugee institutions have replaced the first temporary agencies, and private humanitarian groups have become increasingly skilled. Despite improved international collaboration, Gordenker recognizes that existing institutions are so inadequate that further cooperation is urgently needed.

Gordenker examines overlapping concepts such as *refugee* and *migrant*, which have ac-

quired confusing, politically dictated meanings with legal consequences. Not all migrants forced to leave their homelands are recognized as internationally protected refugees. The United States, for example, distinguishes economic refugees and individuals fleeing civil war from those entitled to asylum as a result of political persecution. Mass exoduses resulting from natural disasters create a distinct category with equally pressing needs for emergency relief. United Nations and nongovernmental agencies initially granted a narrow mandate have progressively expanded their programs to assist otherwise unprotected groups when distinct needs arose.

The title reference to "international politics" does not accurately reflect the work's primary focus on transnational organization. Gordenker favors the humanitarian spirit motivating cooperative relief efforts and does not fully explore the national political interests and ideological differences affecting refugee situations. Without adequate explanation the text notes that the Soviet bloc's nonparticipation in international refugee organizations and fails to mention the unique tradition of political asylum among Latin American governments. The useful description of how political factors led African states to enlarge the legally recognized class of refugees is appropriately contrasted with the more narrow concept advanced to serve Western interests.

Gordenker correctly shows that political considerations have resulted in minimal legal protection for refugees. Nevertheless, the standards that have been codified deserve more systematic and less summary treatment. Richard Lillich's *The Human Rights of Aliens in Contemporary International Law* is a particularly valuable reference that is not cited.

The work presents numerous specific examples of refugee problems involving a variety of governments from different regions, which provide valuable illustration for the author's general classifications and observations. The study could have better shown the complex interactions among intergovernmental, national, and private relief agencies with one or more in-depth studies. Repeated brief references to the UN high commissioner for refugees create a fragmented, slow-to-develop picture of that all-important agency. Generalizations revealing the author's commonsense understanding of international affairs occasionally read like

statements of the obvious (e.g., "Resettlement inevitably involves costs"). Concluding chapters too often unnecessarily repeat illustrations and analyses from earlier portions of the work. Humanitarians, concerned with suffering refugees are occasionally faulted for addressing symptoms without remedying the underlying cause. Aside from a brief discussion of "early warning" mechanisms, Gordenker finds little prospect that refugee problems can be prevented.

Gordenker relies primarily on official documents and regrettably makes few direct references to his interviews with refugees, public and private relief officials, and four of the UN high commissioners for refugees. His normative, general overview offers minimal empirical analysis of data on refugee populations or relief programs. Recognizing the need for original research, the author does offer specific recommendations for future studies employing quantitative methods. The book concludes by suggesting that the transnational activity described constitutes an emerging international regime, suggesting an approach that might have been employed to give a somewhat greater analytical focus to his work. Gordenker's broad overview and general description provides a useful introduction to a subject of growing importance as well as helpful recommendations for future research.

HOWARD TOLLEY, JR.

University of Cincinnati

New Directions in the Study of Foreign Policy.

Edited by Charles F. Hermann, Charles W. Kegley, Jr., and James N. Rosenau (Boston: Allen & Unwin, 1987. xiii, 538p. \$60.00, cloth; \$19.95, paper).

This study contains a critical evaluation of the field of comparative foreign policy, which Rosenau originated in 1966. It is the outcome of a conference at Columbus, Ohio in May 1985, sponsored by the three editors' institutions: the Mershon Center and the universities of South Carolina and Southern California. Twenty-two papers have been revised by contributing scholars and carefully edited to present innovative proposals for research. There are no contributions from outside the United States.

A strength of this study is its criticism of the field. The major problem is that comparative foreign policy lacks a cohesive theory. In 20 years it has produced many hypotheses about isolated phenomena but no comprehensive body of thought or overall explanation. Nor is there any clear definition of content. It intersects both international relations and domestic decision-making behavior. It is thus a bridge between complex disciplines.

The editors are optimistic that a theory may emerge, and they give encouragement here to some proposals. Bruce Moon proposes basing comparative foreign policy in political economy, that is, understanding the economic determinants of behavior. Rosenau's reply to the challenge of general theory retreats to a single-country theory, which he illustrates with the USSR, ingeniously resolving the unique and the general by calling for a dialogue of the cultural historians and the international theorists.

The most comprehensive response is advanced by Maria Papadakis and Harvey Starr. Their model incorporates the three basic elements of process, output, and behavior: First the process of decision making is done by the actor (the state, decision makers, and structures), operating within the opportunity provided by the environment and constrained by it and by the actor's perception of the environment. Secondly, the actor's choice determines the output (decision). Thirdly, the actor's choice further determines outcomes (actual behavior). These then affect the environment, divided into six major levels of rising complexity—four domestic (individual, role, governmental, and societal) and two international (international relations and international system).

The other essays deal with one part of the field. In the area of process, Charles Powell, James Dyson, and Helen Purkitt urge the necessity of learning how policymakers understand a problem. Dwain Mefford proposes the use of artificial intelligence and shows that analogical thinking is pervasive and basic to decision making, as in the 1968 Soviet intervention in Czechoslovakia. In social organization Kegley proposes the study of regimes, Stephen Walker examines the use of role theory, and Paul Anderson studies social process of policy-making in three U.S. crises. Charles and Margaret Hermann join Joe Hagan

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to identify three types of decision-making bodies.

In the area of outcomes, William Dixon demonstrates a lag sequential approach to US-USSR behavior between 1966 and 1978. Gregory Raymond proposes evaluating consequences defined by goals. David Bobrow and Steve Chan urge study of anomalous "successes," such as three Asian states. Unfortunately, success is weakly defined by selectively quoting some pessimistic expectations of the 1950s. Benjamin Most and Randolph Siverston show that the intuitive belief that arms races and alliances are interchangeable does not apply between 1870 and 1914. Focusing on the interaction of states, Neil Richardson urges study of dyadic interaction and shows how it works in promoting conflict to divert from domestic difficulties. Russell Leng shows how behavior in militarized disputes corresponds with the structure.

Three approaches to international influences on the environment are advanced. Papadakis and Starr use behavior of small states. Eugene Wittkopf and Mark DeHaven test the influence of Soviet behavior on U.S. presidential popularity. Margaret Karus and Karen Mingst show that all states are influenced by international organizations.

Among the domestic influences of the environment, we have new ideas from Hagan on how to study the effect of opposition, from John Vasquez on the interplay of accommodation and power politics in the outbreak of war, and from Martin Sampson on the importance of cultural influences illustrated by France and Japan. I am struck that so little is known of the actual processes of policy-making in these two countries. Although our knowledge of facts of international relations has grown greatly, we still lack some of the most critical data. Beyond that we need confidence that data is not arbitrarily classified and that the interrelationships of such a huge and complex field are correctly understood.

JAMES W. GOULD

Scripps College

Ideology and U.S. Foreign Policy. By Michael H. Hunt (New Haven: Yale University Press, 1987, xiv, 237p. \$22.50).

Michael Hunt, a distinguished specialist in the history of U.S. relations with China, ventures beyond the bounds of his earlier writing to study how certain traditional ideas and prejudices have shaped the United States' behavior abroad. It is a work of history and theory together: a historical tracing of the recurrence of three characteristic U.S. habits of thought and also a theoretical discourse on the degree to which beliefs that are seldom consciously examined have shaped our foreign relations. The book begins with theory in an introductory chapter entitled "Coming to Terms with Ideology," then turns to history. Three themes of U.S. thinking—visions of national greatness, notions of racial superiority, and distrust of revolutions—are traced in three chapters through the seventeenth, eighteenth, and nineteenth centuries. A fifth chapter pursues these themes through the history of the twentieth century, then a final chapter titled "The Contemporary Dilemma" turns again to the theoretical problem of how U.S. citizens can be made aware of, and so be freed from, a "foreign policy ideology" that too often replaces thought and results in irrational behavior.

As readers of Hunt's earlier works would expect, the historical writing is superb. His prose is lucid and graceful, and he knows how to carry a narrative and an argument forward together. He warns his reader that he is experimenting, moving for the first time beyond the strict, source-tied writing of specific history to the exhilaration of summarizing decades in a page. Still, he is too well trained and experienced to lose his caution, and his generalizations are constructed carefully and substantiated as well as the broad scope of his inquiry will permit. Surely this volume will find a wide and appreciative audience among students of diplomatic history.

As a work of theory it will have its critics as well as its admirers. It will be applauded as an ambitious and resourceful attempt to expose the impact of popular prejudices and cherished assumptions on foreign policy thinking, but it will be faulted on at least two basic points.

First, do the three habits of thought Hunt identifies and traces really amount to U.S. ideology of foreign affairs? The reader senses

that the whole of U.S. ideology is bound to be larger than the sum of the parts presented here. If so, what themes characteristic of U.S. thought have been omitted? Hunt, a long-time student of U.S.-Chinese relations, not surprisingly centers on U.S. citizens' sense of racial superiority. But it seems a student of U.S.-European relations would be inclined to feature their distrust of monarchies ("crowned ruffians," Tom Paine called them) as well as moral skepticism of balance-of-power politics, and a student of U.S.-Latin American relations would seem likely to center on hostility to the Church of Rome and condescension to what Jefferson called "those priest-ridden peoples." Rather than scanning for themes in the vast record of three centuries of experience, mightn't it have been a safer method to track certain persistent metaphors in U.S. discourse on foreign affairs? There is, for example, the metaphor of the covenant, ably explored in Robert N. Bellah's *Broken Covenant*. The metaphor of the United States as Jerusalem, as opposed to Old World Babylon, is studied in Ernest Lee Tuveson's *Redeemer Nation*. The metaphor of the U.S. experiment is discussed in Daniel Boorstin's *Genius of American Politics*.

Second, is ideology too broad and inexact a term for this collection of traditional assumptions? Hunt's definition is very loose: "an interrelated set of convictions or assumptions that reduces the complexities of a particular slice of reality to easily comprehensible terms and suggests appropriate ways of dealing with that reality." This definition is posited in the book's opening paragraph, labeled "commonsensical," and never tested or defended. I recommend William H. McNeill's notion of "public myth" ("The Care and Repair of Public Myth," *Foreign Affairs* 61 [1982]: 1-13) as a kindred but more focused concept that avoids the debate over the meaning of ideology and clarifies the psychological importance of these beliefs to the popular mind. McNeill's words on myth making and myth breaking speak directly to the problems Hunt raises in his concluding chapter.

Hunt modestly says that his aim is only to make a start at a neglected problem, that this is a first cut and an invitation to others to try their hand. It is a promising beginning. He has gathered a large and varied array of telling items and linked them in an insightful and in-

structive historical essay. His successors in this inquiry will want to refine his instrument of analysis, however. McNeill's essay is probably the place to start.

STEPHEN D. WRAGE

Georgetown University

International Aviation and the Politics of Regime Change. By Christer Jönsson (New York: St. Martin's, 1987. vi, 185p. \$35.00).

This book is misleadingly titled; the emphasis should be on the politics of regime change, since the author's intent is to evaluate various explanations of regime establishment, change, and maintenance. International aviation is the issue area on which he focuses. Along with economic, structural, and situational models that approximate those previously used by Keohane and Nye in the evaluation of ocean and monetary regimes (*Power and Interdependence: World Politics in Transition* [1977]), he introduces a model similar to a complex interdependence model, which he calls a process model. Like Keohane and Nye, he moves from the most parsimonious models to the more complex ones in his investigation.

The three phases that Jönsson studies are the establishment of the sovereignty-oriented air regime in 1919, the granting of some basic international freedoms in international air transport with the Chicago-Bermuda agreements at the end of World War II, and the unsuccessful challenge by the United States in the late 1970s to make international aviation considerably more open. Although the United States was successful in having pricing rules relaxed, Jönsson asserts that the United States' restricted success marked a change in the regime rather than a change of the regime. Consequently, he describes the U.S. challenge as a failure.

Jönsson's overall conclusion regarding the use of the four models is that because they each ask somewhat different questions and provide answers that at best represent only partial explanations, the models can complement each other. However, by generally showing the structural and economic models to be inconsistent and the situational one to be an awkward blend of complexity with a simplifying

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assumption of rational actors, he strongly encourages the increased investigation of regimes using the process model. With his process model, he sees regime dynamics as "outcomes of multilevel bargaining processes where the existence or absence of transitional networks is of special significance" (p. 153). It goes beyond both a state-centered and domestic politics approach in order to investigate the critical organization links for information flow in a bargaining process that seeks a formula or compromise. Bargaining theory and inter-organizational theory intertwine in a complex way to explain how significant agreements emerge in regime decision making. By meshing bargaining and organizational theory in more case studies, Jönsson suggests that political scientists will be better able to generalize about the decision-making process in regime dynamics.

In response to Jönsson's suggestion that the four models be used to complement each other, the following question arises: Do partial explanations to different theoretical questions truly complement each other, compete with each other, present groundwork for synthesis, or simply illustrate the barrenness of theory in regimes?

Like Keohane and Nye, Jönsson tests models against a regime, but unlike them, he is only concerned with one regime. Although he is certainly not the only regimes analyst to focus on one issue area, his concepts of regime establishment, change, and maintenance would have been better developed with comparison across two or more issue areas—for example, with similar instances in Keohane and Nye's case studies.

Jönsson has written very clearly, with excellent transitions between his many models and eras. He has contributed to the regime literature with his description of the evolution of the air regime, and his work fits neatly in the international regime literature that has been so abundant in the last decade. Perhaps its very neatness is the biggest problem, though. He apparently assumes he can rush through his analyses of structural and economic models because they have already been generally attacked in the regime literature. His use of the situational model struck me as similarly half-hearted, although better developed. Overall, I think he could have made a much more significant contribution by concentrating only on the

process model and emphasizing the theoretical ramifications of such a shift to complexity for explaining regime dynamics.

STEVEN E. ELSE

United States Air Force Academy

War in International Society. By Evan Luard (New Haven: Yale University Press, 1987. 468p. \$32.50).

The Lessons of Recent Wars in the Third World, Vol. 1. Edited by Robert E. Harkavy and Stephanie G. Neuman (Lexington, MA: Lexington Books, 1985. ix, 304p. \$35.00).

In a field dominated by quantitative analysis, it is unusual to find a large book from a university press eschewing quantitative analysis for old-fashioned intuition. It is even more unusual to find a book exemplifying the very practices that proponents of the quantitative method have criticized—definitions that are not precise, criteria that are not rigid, and generalizations that are too vague to be either verified or refuted. Because it offers so many examples of what the quantitative school criticizes, Evan Luard's *War in International Society* is unlikely to make inroads on its dominance.

Although providing some trenchant criticism of other definitions of war, Luard then offers one remarkable for its elasticity—an "encounter" involving "substantial" fighting over a "significant" period (p. 7). By including such diverse events as the Syrian leveling of several square blocks of their own city of Hama in 1982 and a 1981 border skirmish between Ecuador and Peru that killed nine, Luard has produced a very long list. The Correlates of War project identified 224 wars (interstate, extrasystemic, and intrastate) from 1816 to 1980; by contrast, that portion of Luard's list for the same period (and his overall list is much longer) covers 388.

Working from this list, Luard makes observations of frequently surprising vagueness. We are told that governments have not "normally" desired war for its own sake (p. 181) and that "in many cases" public opinion has proved more warlike than governments themselves have been (p. 224). The reader may grant that

a false precision is misleading. Even so, the author should acknowledge that when unambiguous quantitative measures are available they help the reader assess the author's conclusions. Luard's assertion that "we live today not in a more peaceful international society but one that is particularly warlike" (p. 395) appears to contradict conclusions of other scholars (for example, the chapter by Starr and Most in the other book here under review), yet because he does not lay out the quantitative basis for his assertion, we cannot be sure. Only occasionally does Luard offer even percentages and then only as part of a running account, never in tables.

By choosing to begin in 1400, Luard has produced a list of staggering size. The relevance of much of this material to contemporary concerns will strike many readers as minimal. A reader totally unaware of history may find it useful to learn that states once went to war for dynastic concerns but do so no longer; readers who have grasped this point may question the value of learning the details of the particular dynastic wars.

His large number of cases leads Luard into a trap. Understandably, he divides the wars into periods. Less understandably, he gives each of these periods names—thus 1789 to 1917 becomes the "Age of Nationalism" while the period from 1917 to the present becomes the "Age of Ideology." He then allows these labels to influence his analysis. Wars fought since 1917 are perforce motivated by ideology. Thus, according to Luard, when Germany and Italy achieved unification through war, they fought wars of nationalism; when Vietnam and Korea sought unity through wars, they fought wars of ideology. In 1848 Hungarians fought a war of national liberation against the Austrians; in 1956 they fought an ideological war against the Soviet Union. Even the squabbling among rival warlords in Lebanon in 1958 is interpreted as a war between Communists and anti-Communists (p. 120). Although differences between wars pointed up by Luard's categorization do exist, they are often not as great as the similarities that his categorization conceals.

In part this conceptual rigidity follows from Luard's explicit argument from analogy. He posits an international society, then asserts that such a society behaves just as other societies do, imparting values to its members.

"The most important single thing society teaches its members," he asserts, "is what they should want; whether to value achievement or contentment, wealth or power, social success or artistic attainment, strength of arm or peace of mind" (p. 385). Having started with this premise, it is not surprising that he concludes "there is usually, as we have seen throughout this study, a considerable homogeneity in belief and behavior among all states within the same society" (p. 390). The current vogue of labels such as "trading states," "warrior states," and "crazy states" suggests that many scholars think otherwise.

Readers of encyclopedias have complained since the form was invented that while they may contain useful information for people who know little, they always get the details wrong in their own areas of expertise. Luard's book has the strengths and weaknesses of an encyclopedia—much information is assembled in handy form for the nonexpert, but it is unsatisfying to the expert or student desiring to become expert. Luard's own choice of the label *study* for his book suggests a past era, the solitary scholar alone in the library with pen and paper.

By contrast, Harkavy and Neuman have produced a very contemporary book, an assembly of papers generated by a conference of experts. Their effort is a superior one. The thematic essays all manage to address the subject of the book, saying something original while drawing on contemporary scholarly literature. The case studies illuminate the general theme. While no one would pretend that these studies are the last word on the particular conflicts, they are by and large comprehensive and complete.

Harkavy lays out the problems of comparative analysis, with ample reference to the literature. Starr and Most follow with a 20-page summary of war in the contemporary world that most readers will find more useful than the 450 pages of the Luard book. The remaining two introductory essays, by Luvaas on "lessons" in historical perspective and Dupuy on quantitative predictors of outcomes of wars, are more tangential but worthwhile in their own right.

The eight cases studies vary in quality, from a fragmentary look at the Falklands War (1982) to thorough accounts of the China-Vietnam (1979) and Israel-Syria (1982) con-

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flicts. Chapters on Ethiopia-Somalia (1977-78) and Morocco-Polisario (1975 on), Soviet Union-Afghanistan (1979 on), and various conflicts in Central America (1972-84) provides as much information as the nonregional specialist might care to absorb. Only the chapter on Iran-Iraq suffers from being seriously out of date.

The work coheres well enough that useful lessons can be drawn, some obvious, some not. As Neuman points out in her concluding chapter, preponderance in numbers or equipment does not ensure victory: strategy and tactics count for much. Equally important is mastery of logistics—these wars have been expensive. Partly for this reason, conflicts have been increasingly internationalized. But Western participation has declined, while involvement of other third-world states has increased. Further analysis is promised in a second volume.

DAVID W. ZIEGLER

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The Soviet Study of International Relations.

By Allen Lynch (Cambridge: Cambridge University Press, 1987. xii, 197p. \$34.50).

Western, particularly U.S., international relations scholars tend to ignore the works of their Soviet counterparts. They have assumed a slavish Soviet adherence to the Marxist-Leninist "vertical, class-oriented paradigm" in which horizontal relationships (as those between nation-states) are seen as derivative from the vertical interclass relationship.

The Soviet Study of International Relations reveals this assumption as untrue. Its author, Allen Lynch, is the deputy director of studies at the Institute for East-West Security Studies in New York. Lynch tests a hypothesis that Soviet international relations theory (characterized by one Soviet scholar as "creative Leninism") has moved substantially in the direction of theory emanating from the West (*convergence* is a term that the Soviets reject). Lynch's book is, in effect, an extension upon William Zimmerman's treatment of trends in the Khrushchev and early Brezhnev periods

(*Soviet Perspectives on International Relations, 1956-1967*, [1971]). Zimmerman found that Soviet scholars (indebted to the groundbreaking Stalin era works of Yevgeny S. Varga) were coming to recognize the primacy of states over classes as key elements of international relations, the primacy of the political sphere ("superstructure") over the economic (the "base") in interstate relations, and the significance of the structure of international relations ("system").

Lynch confirms that characteristics and tendencies revealed by Zimmerman have been sustained in the ensuing years. Lynch's book may be considered the definitive assessment of the state of the Soviet international relations discipline as of the early Gorbachev era.

Among many trends and patterns that Lynch reveals in his review of recent and contemporary Soviet international relations literature are

1. acceptance of the primacy of politics over economics as the essential stuff of international relations and a focus upon states as the most central elements; increasing perceptual differentiation between domestic and international political phenomena, particularly in the assumed centrality of class struggle to the former in contrast to the latter

2. the development and refinement of Soviet global systems theory; some corollary recognition of the insights of Western theorists such as Morton Kaplan, Charles McClelland, and Stanley Hoffmann.

3. "deutopianization" of Soviet international relations perspectives, owing to recognition (in implicit repudiation of Leninist orthodoxy) of the indeterminacy of outcomes, catastrophic potential of nuclear war, and "underlying stability" of the West

4. a preoccupation with systemic bipolar configurations; that is, the "socialist" and "imperialist" camps as critical subsystems and particularly their two leading actors, the Soviet Union and United States; recognition of considerable United States advantage, despite the passing of U.S. "hegemony."

5. recognition of tendencies toward systemic multipolarity—understandably, an emphasis upon "contradictions" (particularly those involving U.S. relationships with Western Europe and Japan) within the "imperialist" bloc more than upon differences among "socialist" states (except the People's Republic

of China, which the Soviets continue to characterize as "socialist," but have come to recognize as supplanting the United States as the principal external threat); admission, however, of the stability of the "imperialist" bloc, despite contemporary "crises" of capitalism.

6. Attention to "scientific-technical revolution," with a sense of uncertainty as to its outcomes vis-à-vis the Soviet position in world affairs.

In deference to ideology, Soviet theorists criticize the "realist" perspective in Western international relations scholarship. Lynch contends, however, that in its conceptual separation of "superstructure" from "base" (and perhaps in the consignment, by Soviet scholars, of international organization to the periphery of international relations), Soviet theory has incorporated much of the spirit of "political realism."

But normative elements remain as salient features of Soviet theory. This is indicated in the hortatory language in which it is articulated and in Soviet *appraisals* of the present system as fundamentally illegitimate. As to this world order, Soviet theory grants to Soviet citizens the prerogative (even obligation) to explore "ways of bringing about its demise, so long as this does not threaten the hard won achievements of socialism" (p. 147). Soviet international relations theory, divested of orthodoxy, has moved in the direction of theory in the West. But in this normative sense, it remains distinctly Leninist, and full convergence is most unlikely in the foreseeable future.

Readers with a broad understanding of Leninism, Soviet politics, and contemporary Western theories of international relations will find Lynch's synopsis of this Soviet literature to be both valuable and comprehensible. His extensive bibliography will be useful to those who have mastered the Russian language and have access to Soviet scholarly works. Lynch alludes to an increasing autonomy in the Soviet study of international relations. He means by this growing recognition of it as a field of study distinct from the other behavioral sciences. This field has not been separated from regime or party. Thus its theorists, more than their Western counterparts, instruct decision makers as to world perspective and the development of policy. Western counterparts of these Soviet decision

makers should, therefore, find particular value in Lynch's book.

J. DAVID GILLESPIE

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Africa and the Middle East Conflict. By Arye Oded (Boulder: Lynne Rienner, 1987. xii, 244p. \$29.50).

Ayre Oded provides a broad and well-researched survey of African relations with Israel and the Arab world since the 1973 war. It relies primarily on extensive use of newspaper and magazine sources, including many African and Arab publications. Although the preface refers to interviews and meetings in Africa there is no direct reference to these materials in the book.

The thesis is that since 1973 Arab states have achieved impressive gains in cultivating Africa through economic aid, diplomacy, use of Islam, and other instruments. Persistent problems plague African-Arab relations, including the legacy of the slave trade, African disillusionment with the nature and extent of Arab economic aid, fears of Islamic fundamentalism, and struggles over leadership in the Organization of African Unity and other entities. However, since 1973 only Zaire, Liberia, Cameroon, and the Ivory Coast have restored full diplomatic relations with Israel, in addition to Lesotho, Swaziland, and Malawi, which never broke relations. In spite of the Camp David accords, Arab pressures and assistance have maintained the facade of Arab-African solidarity on Arab-Israeli issues.

While the brunt of the book examines Arab-African relations, two chapters deal with Israel and Africa since 1973. These are particularly disappointing given the author's access to official Israeli sources. Three superficial pages discuss Israeli-South African concerns about alleged clandestine contacts. There is no mention of the Israeli rescue efforts on behalf of Ethiopian Jews and the impacts these had elsewhere in Africa. We are told that Israel maintains extensive commercial and technical assistance ties with states with whom it has no diplomatic relations (Ghana and Kenya) but are given little insight into how those ties operate and why.

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The material on Arab efforts to influence Africa is useful but highly uneven. An important chapter discusses official Islamic organizations and their activities but is bereft of any sense of how Islam in black Africa operates or of references to the literature. The maze of promised and actual Arab multilateral and bilateral aid to Africa is adequately sketched. Two chapters on African-Arab relations rely unduly on scattered editorials and letters to the editor from the African press and lack context and substance. It may be that more systematic data such as in-depth interviews and public opinion polls is lacking, but what is presented is too prone to selective bias by the author.

The conclusion is that since 1973 "the balance of African-Middle Eastern relations has definitely tilted in favor of the Arabs" (p. 222). Israel has managed to maintain working relations with a handful of states but it has been unable to convince more than a few states to resume diplomatic relations. This is attributed largely to the pressure of possible Arab reprisals, although the author notes that the four states that have restored relations since 1982 have not experienced significant reprisals.

What is lacking in this informative but limited study is an analysis of African, Arab, and Israeli interests in one another as perceived, implemented, and altered by decision makers, and where relevant, public opinions. Have the Arabs convinced most Africans that it is in their interests to support Arab views on the Arab-Israeli conflict in exchange for some economic aid and reciprocal support for African views on South Africa? Do African elites and publics share a sense of solidarity with the Palestinian cause or Arabs with that of black South Africans? What does Israel have to offer Africa, and vice-versa? This book is a useful effort to compile meaningful responses.

AARON SEGAL

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British and American Approaches to Intelligence. Edited by K. G. Robertson (New York: St. Martin's, 1987. x, 281p. \$39.95).

The title of this often stimulating collection of essays is somewhat misleading. The clear

emphasis is on British experience in modern intelligence policy and concepts. Ten of the 11 essays are by British authors who have either served in the Foreign Office or associated with British universities and publishing houses. While a few examples of U.S. intelligence policy are cited, there is no real effort made to integrate U.S. ideas into comparative framework for analysis. The unfortunate title aside, the collection as a whole contains some worthwhile material.

The stated theme of the collection is three-fold: the nature of intelligence, the development of British intelligence, and the relationship between intelligence and democracy. The essays that are rather loosely grouped under these headings do not begin to adequately address the broad topics suggested, but they do make a number of fresh contributions.

John Lockhart served 20 years with the British Foreign Office (1945-65). His "Intelligence: A British View" is written with an understated wit and common sense that most Englishmen would like to think is the hallmark of that island race. His concluding "Observations on Intelligence" is a sort of Murphy's Law of aphorisms on what can go wrong with intelligence operations in a democratic country; they should be memorized by everyone in the intelligence services from top to bottom.

"The best and most interesting essay is that of Wesley Wark, "Intelligence Prediction and Strategic Surprise: Reflections on the British Experience of the 1930's." Essentially, this essay is a distillation of his longer work, *The Ultimate Enemy*. His thesis is simple but rich in implications. Most of the literature on intelligence failures in World War II have followed the "communications model" pioneered by Wohlstetter's study of Pearl Harbor. Her argument was that the inability to separate background noise from true signals tends to bias intelligence analysis in favor of preconceived ideas. But as Wark points out, this usually assumes the existence of a set of convictions about potential enemies. The threat of Nazi Germany was unique. It was the first of the totalitarian states to pose a military threat to liberal democracies. The background noises were traditional assumptions about state behavior and the real signals were the unique ideological patterns of the Nazi state. While much of the thesis itself is not entirely new, Wark does successfully place it in the context

of intelligence analysis in a way that makes intelligence more than a policy issue, and helps focus attention on intelligence as a philosophical question as well.

The weakest part of the collection is "Intelligence and Democratic Values." The general subject speaks of its own importance, but the specific problems are far less obvious. Given the nature of the subject it might have been better to omit this section altogether than to deal with the questions so inadequately. The essay by Eunan O'Hallpin is a fine study of how British intelligence was financed from the turn of the century through World War II, but it doesn't seem to fit here. Michael Supperstone has contributed a good essay on the law relating to security in Great Britain, but doesn't pretend to exhaust the issue. The final essay by K. G. Robertson attempts to compare British and U.S. attitudes toward society in intelligence, but while he casts his conceptual net far enough to encompass most of the pertinent questions, the short essay format allows his subject to escape through the numerous holes. Taken as a whole, the reader wishing to explore intelligence and democratic values will have to look elsewhere.

The weaknesses of this collection are not due so much to the quality of the essays contained therein. It is rather a weakness born of collecting a series of conference papers into a published volume. The imposition of order on case studies and otherwise eclectic material is likely to be arbitrary at best and superficial at worst. This collection generally fits that pattern. Having said that, however, it must also be said that the essays themselves are generally of high quality and several are quite stimulating. Students of intelligence operations will find a few new morsels to chew over and newcomers will find some genuine food for thought.

SIDNEY A. PEARSON, JR.

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The Origins of Alliances. By Stephen M. Walt (Ithaca: Cornell University Press, 1987. x, 321p. \$32.50).

Notwithstanding the amount of attention alliances have received in the literature of international relations, it is fair to say that our comprehension of them is still imperfect. One

unresolved problem can be found in our fragmentary understanding of the manner in which nations choose particular alliance partners. This book is devoted to that problem.

In order to understand how alliance partners are chosen Walt articulates three sets of hypotheses. The first of these revolves around the question of whether nations choose alliance partners to balance against power or jump on the bandwagon with it. The second relates to the extent to which ideological similarity between governments influences specific alliance choices. Finally, Walt seeks to uncover whether foreign aid and political penetration cause alignment choices. The main value of the three chapters is not their originality, for, as the exhaustive notes tell us, most the substantive material has been presented by others before. Rather the value is to be found in the enviable manner in which Walt pulls together a vast amount of literature and summarizes the essential arguments into a relatively few hypotheses.

To evaluate the accuracy of the hypotheses, Walt then presents two chapters reporting the intricate twists and turns of military-diplomatic events in the Middle East between the Baghdad Pact and the Camp David agreements.

The main conclusions that follow from the comparison of the hypotheses with the historical narrative may be readily summarized. First, in general, nations choose to enter alliances for the purpose of balance against a nation that is threatening. Put differently, there is little evidence of the bandwagon and less that nations simply react to power by itself; the element of threat usually needs to be present to activate balancing alliance activity. Secondly, Walt argues that the overall pattern of alliance activity in the Middle East "offers only modest support for the hypothesis that states with similar domestic systems are more likely to ally with one another" (p. 214). Finally, with respect to the influence of economic and political penetration, Walt finds that these instruments are not likely to be effective in themselves but that when the interests of nations converge such things as aid and arms transfers may reinforce existing interests and reduce the friction of the arrangement.

The final chapter casts the three sets of findings against a general discussion of alliances and a consideration of the relationship be-

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tween the U.S. and Soviet alliance systems as they now function. Although the broad discussion of alliances in this chapter is not closely tied to the set of theoretical questions that served as the initial point of departure for the study, the policy recommendations offered in the final pages are nonetheless satisfying because they follow from a general set of propositions much more than is normally the case in policy analysis.

In sum, the book provides an interesting discussion of three possible factors in alliance formation; an excellent brief summary of post-World War II Middle Eastern diplomacy; and a thoughtful, theoretically driven set of policy recommendations.

That said, it must be admitted that the study is not without its problems. While there are some minor points of difficulty (such as often-mistaken discussions of rational choice theories of alliance formation), the major problem of the work is its failure to maintain the high theoretical level that guides its initial purpose. Instead of explicitly stating a set of Waltzian assumptions about the necessity of alliances in the international system and then allowing these assumptions to interact across considerations of national power, ideology, and penetration, the author turns to a consideration of the three individual factors in the formation of alliances without a well-formed consideration of how the constraints and imperatives of the international system interact with the three identified factors. In addition, the analysis of the case data is carried out within a research design that simply will not permit a sufficiently rigorous framework for the purpose. For example, is it reasonable to expect that the range of difference in power, ideology, and penetration in the Middle East over a relatively short period is sufficient to adequately probe the hypotheses? While Walt asserts that he is using the technique of focused comparison, the absence of other distinct cases to add to the variation in the independent variables detracts from the "tests." The adoption of a more rigorous approach to the coherence of the theory and domain of the research design might have led to a discussion of the role of national autonomy and policy preferences in alliance choices. As it is, these factors are only implicitly considered.

Admirable as some aspects of this book are, its difficulties should remind us of how far we

have to go in designing research to address the problems that drive inquiry in international politics. It is, of course, the case that fully satisfactory research designs are difficult to devise because of the difficulty of establishing statistical controls on the small data sets usually encountered in the field, but this realization should lead us to specify our theories more closely.

RANDOLPH M. SIVERSON

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Egypt and the Politics of U.S. Economic Aid.
By Marvin G. Weinbaum (Boulder: Westview, 1986. xi, 192p. \$23.00).

Egypt was once called the gift of the Nile. Today things have changed considerably. Since the construction of the High Dam at Aswan at the border between Egypt and the Sudan a generation ago, the river that for centuries brought nourishment to Egypt's parched soil has lost much of its life-giving quality. The very waterway that once afforded unimpeded passage across the entire length of Egypt to innumerable dhows and other rivercraft is today all but choked over much of its course by an inundation of tenacious weeds.

Where Egypt was once the breadbasket of the Middle East, today it is dependent upon economic help in vast amounts from overseas. This help is needed not only for economic development but to sustain the people of the Nile valley with the necessities of daily living. Since the mid-1970s the United States has supplanted the Soviet Union as Egypt's principal benefactor. Today, Egypt has achieved the distinction of being second only to Israel as the main recipient of U.S. economic and military aid.

Marvin Weinbaum's small book, *Egypt and the Politics of U.S. Economic Aid*, sets out to examine how both Egypt and the United States have gained from the vast expenditure of funds. While Weinbaum puts the best possible gloss on his account, the story is not a happy one, regardless of whether the perspective starts from Washington, D.C. or from Cairo.

Weinbaum shows in considerable detail the many ways in which the billions of dollars expended on economic and military aid have not only repeatedly fallen short of their declared

objectives but failed, in the bargain, to elevate the United States in the esteem of the Egyptians. Even worse, the gift giver is increasingly blamed by the Egyptian populace for the aid program's inability to lift Egypt out of its chronic despondency. Nonetheless Weinbaum seeks to console the U.S. reader with the judgement that U.S. assistance has, for all its shortcomings, served to sustain Egypt in the difficulties it has endured at the hands of its fellow Arab states ever since the Egyptian-Israeli peace treaty was signed in 1979.

In his concluding sections, Weinbaum suggests that the U.S. might obtain better Egyptian compliance with the entire aid program and thereby make it more effective if the United States adopted a sterner stand whenever Israel is held by Arab states to cause them offense. Curiously, Weinbaum never

considers the benefit that the U.S. brought to Egypt itself by bringing the peace treaty to completion and thereby releasing Egyptian military forces to confront their potential enemies on their African frontiers: Libya, Sudan, and Ethiopia.

So far, both Egypt and the United States have muddled through. But the era now upon us, where each U.S. budget dollar is being scrutinized closely for its program effectiveness, means that the time is fast approaching when the U.S. assistance program in Egypt will be examined more closely than before. It is a pity that Weinbaum does not provide much guidance for that upcoming undertaking.

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POLITICAL ECONOMY

Multinationals, the State, and Control of the Nigerian Economy. By Thomas J. Biersteker (Princeton: Princeton University Press, 1987. xvii, 344p. \$45.00, cloth; \$12.50 paper).

In the field of the comparative political economy of development, no question is more compelling and elusive than that of control. The issue of who really controls the economies of developing countries—their own states, local bourgeoisies, or the multinational enterprises that invest directly in them—lies at the core of the ongoing debate between various neoclassical and radical (dependency or neo-Marxist) schools of theory. Few studies have examined the issue as seriously, imaginatively, and productively as has this impressive new book by Thomas Biersteker.

Multinationals, the State, and Control of the Nigerian Economy is one of the first works on the political economy of development to penetrate the process by which third-world states make and implement policies on foreign investment. Thus whereas most previous works have been able to draw only weak inferences about the (changing) nature and balance

of power relationships, Biersteker derives a number of well-supported conclusions from a rich and original array of data sources—in-depth interviews with 157 state officials, multinational managers, indigenous businessmen, and other key actors or observers; government documents and statistics; academic and policy papers; and an elaborately coded set of data on Nigerian corporations.

Such close study of the policy process is the only way of determining anything conclusive about the structure of power. That this behaviorist method yields compelling but qualified support for some predictions of dependency theory is one of the many refreshing and provocative features of this work.

The book examines the origins and determinants of Nigeria's two indigenization decrees (1972 and 1977) and then assesses their consequences for control of the Nigerian economy at three levels: individual firms, economic sectors, and the Nigerian economy as a whole. The author begins "by identifying the actors with effective channels of influence in each issue area, and then attempt[ing] to determine their principal objectives, concerns, and disputes" (p. 290). He proceeds to disaggregate

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the major corporate actors (the state and foreign and local capital), looking for contending interests and motives within each.

The analysis of the two indigenization decrees shows how each key group pursued its interests and pressed, influenced, or checked the others and thus how each decree represented a compromise of some sort—the product of a distinctly *political*, rather than rational, process. The state can mediate, as it did for the formulation of the first decree, or it can initiate and lead, as it did for the second; but no single corporate actor can simply impose its will. Also examined is the process of policy implementation, both the actual functioning of state institutions and the evasive, “counter-offensive” behavior of multinational enterprises. The potentially contradictory nature of the formulation and implementation phases of the policy process is an important methodological lesson of the study.

Theoretically, the book contributes an original, dispassionate, and remarkably clarifying dissection of six contending perspectives, which demonstrates inadequacies in every one. Thus, the author’s approach is deliberately eclectic, seeking to evaluate and draw from the contending theories. Admirably, Biersteker resists the temptation to reach for sweeping generalizations. Having no theoretical axe to grind, he recognizes that the empirical world is messy, contradictory, and full of nuance and that caution is in order when generalizing from a single, distinctive case.

The key general finding is the complex one that “there is no single theoretical explanation for the sources of programs of this sort (e.g., capital does not always capture the state and the state does not always have the capacity for autonomy)” (p. 290). There will also be keen interest in his conclusion that the shift of bargaining power from multinational enterprises to host countries is neither stable nor inexorable but part of a mutual process of learning, adaptation, and struggle for control.

With respect to the issue of control, Biersteker concludes that there has been virtually no change yet at the level of the firm, but he does uncover (and is the first to do so) some potentially historic developments. There has begun to be some significant indigenization of personnel at the middle and senior ranks. Due not simply to the decrees but to the active managerial intervention of the central bank,

Nigeria has taken control of its own financial sector. Most interesting, perhaps, the book reveals some movement of the traditionally comprador, commercial Nigerian bourgeoisie into productive activity, as the indigenization decrees have opened a possible niche for local capital in low-technology manufacturing.

The book is not without its flaws and limits. Although there is a note of caution (p. 246), the author has not done enough to warn the reader (and perhaps himself) of the questionable basis for drawing conclusions from his data set of 1,230 firms (only about half of the 2,380 incorporated in Nigeria at the end of 1980). I would have preferred to see more treatment of the movement of Nigerian capital not just into manufacturing but possibly even more striking, into agricultural production in recent years. More attention should have been paid to the most nagging (albeit most inscrutable) element in the whole problem of economic control and public policy in Nigeria—corruption. There could also have been a more sustained analysis of the role of bureaucratic leadership, which gets some fascinating but abbreviated attention, and of the fate of implementation after the transition back to civilian, democratic (and more corrupt) rule in 1979.

However, no one study can do everything. To be sure, this book’s creativity of design, balance of perspective, rigor of method, depth of evidence, and wealth of richly documented findings mark it as one of the most important contributions to the field in many years.

LARRY DIAMOND

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Rivalry: In Business, Science, among Nations.
By Reuven Brenner (New York: Cambridge University Press, 1987. xi, 244p. \$27.95).

The world is in the midst of dizzying changes in both product and process technologies. Computers that two decades ago measured their speed in dozens of operations per second now carry out more than 20 thousand arithmetic functions in that same time. Automobiles that once took 80 months or more to move from the design board to production now appear in less than half that time.

New medical technologies that used to take a dozen years to develop now move to their final testing stages in a matter of months.

These changes in technology—the ways that things are done—are reshaping the world economic arena. Japan has become the world's preeminent economic power by harnessing its traditional predilection for new technologies to the cart of economic growth. In Europe, Germany's economic growth is based on its continuing ability to transform technological developments into useful products at a startling rate.

Why does such a harnessing of technological development to economic growth occur in some societies and not in others? A spate of analysts have addressed this question by focusing on the role of the state and by examining the policies that government pursues in promoting economic change and growth. The Japanese miracle, for example, is often explained in terms of its government's fabled industrial policies; China's growth over the past decade is described in terms of its government's Open Door policies of the late 1970s.

But just how important is government's role in pushing economic development and technological change? Reuven Brenner, in *Rivalry: In Business, Science, among Nations*, examines some of the theories that point to the increasing role of government in shaping the nature and pace of technological change.

Brenner takes exception to many of the conventional wisdoms that underlie much of the current analysis. Ultimately, his arguments rest on one important proposition: it is not government, he asserts, that is primarily responsible for the process by which new technologies are acquired and implemented. The proper focus, he argues, ought to be the building blocks that constitute the core of the modern economic system—the individual enterprise.

Brenner begins by questioning the concepts of *market* and asking what it is that enterprises set out to do. Classical economic theorists often argued that an enterprise was simply an organization for "profit maximization." Closer to reality, argues Brenner, is a definition that views an enterprise as a collection of individuals who are motivated (or inhibited) in their search for and acquisition of new technologies.

His early chapters outline some of the players in the technology acquisition process.

Echoing J. B. Say at the end of the eighteenth century, Brenner focuses on the role of enterprise leadership—especially leaders who are "entrepreneurial." An entrepreneur is the "individual who bets on new ideas and implements them" (p. 45). The key variables here are judgment, vision, and insight into people. Entrepreneurial managers focus on innovation and the individuals who might push an enterprise toward innovation rather than financial profit-and-loss criteria.

In a rapidly changing world, Brenner argues, the key to the competitive struggle lies in the enterprise's ability to match customers with services. As customers' needs change, innovative firms with entrepreneurial leadership "leapfrog" into new product and process technologies. Competition between firms, he states, is not so much concerned with a struggle for comparative advantage in pricing as it is with the battle to meet new needs.

Large firms and cartels represent a special danger to the course of change. The danger arises not so much from the oft-observed tendency of cartels to control price, according to Brenner. The real threat from monopolies and cartels, he argues, is that they often inhibit the process of technological change.

How, then, is the wealth of nations maintained or restored? The key, Brenner concludes, lies not simply in the formulation of government "industrial policy" or in the maintenance of a system of "free trade." More important, he argues, is the ability of a society to maintain the creativity of those who produce material goods and new technologies—those within the enterprise and the entrepreneurs behind the enterprise.

Unfortunately, this book is a tough read. Quite often the arguments bog down in difficult language, awkward syntax, and hyperbolic generalization. The editing seems incomplete, and I wish that the author had written at least one more draft.

Nonetheless, Brenner's is an important argument, and he elaborates the argument with some interesting case studies. Moreover, the argument is much more than a simple paean to unfettered laissez-faire capitalism. Command and market economies alike, this argument concludes, must all do more than simply devise attractive macrolevel policies. The real trick is to motivate the actors who are the key players in the game of economic change—the

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discrete units that make up the microlevels of any society.

ROY F. GROW

Carleton College

Peasants against the State: The Politics of Market Control in Bugisu, Uganda, 1900-1983. By Stephen G. Bunker (Urbana: University of Illinois Press, 1987. 296p. \$27.50).

The study of the contemporary African political economy is in ferment. From the rather simplistic expectation in the 1950s and 1960s that the Western liberal-democratic model would guide and explain African politics to the fascination of the late 1960s and 1970s with the seemingly militant single-party mobilization to the attempts to explain all African political processes as extensions of world dependency or class conflict models, African political specialists have tended to overreact to events in the capital and over-apply political-economic models generated in other areas, under other conditions. The lack of fit between the worlds that these models predicted and evolving events, along with the rather rapid about-faces taken in choosing the models, have been clues that something was seriously wrong. Even the attention paid to the "soft state" concept by current analysts leaves some significant ambiguities: How can it be sensible to talk of a "soft state" and overcentralization at the same time? Are the brutal dictatorships which arise from time to time examples of "soft states," reactions to them, or aberrations? How is order maintained in the "soft state," and what sorts of relationships exist among major political actors in them? How are they related to Africa's contemporary economic problems: Are they a cause, a result, or part of a solution?

As a result of the poverty of most theorizing done hitherto on the African political economy, the very appropriateness of conventional social scientific concepts commonly applied to Africa needs to be questioned. *State, representation, peasants, ethnicity, bureaucracy, administration*, and other terms have historically evoked a host of images and implications out of a largely Western experience, which current (and selected earlier) research (Donald Rothchild, Nelson Kasfir, Goran

Hyden, Polly Hill, Sara Berry, and Audrey Smock) increasingly shows simply does not fit into the reality of contemporary Africa.

In a finely researched, well-written and theoretically sophisticated field study, Stephen G. Bunker earns a place in this selected list as he contributes substantially to our understanding of these concepts and how they must be applied to Africa.

Bunker's focus is on smallholder, coffee-producer organizations formed to protect their economic interests in the richest coffee-growing area (Bugisu) of Uganda. In a detailed history of the period 1900-83, Bunker reviews the interactions between the Bagisu (people of Bugisu) and central authorities, the British, the Uganda independent government, and the postcoup regime of Idi Amin. In the first two cases he shows how the smallholder producers were able to utilize their control over production to mediate, negotiate, and modify central policies and programs to better fit Bagisu interests. The independent base of the freeholders along with the state's need for their product meant they had leverage, which they were quick and able to use to protect their interests. Only in the ruthless random violence of the Amin regime, where the base of the state was essentially no longer production but brigandage, did the Bagisu lack leverage.

Bunker's study is instructive in several regards. The rather simple dynamics of dependency theory are strongly challenged by his study, as smallholder producers were clearly *not* "captured" and "molded" by "international capital" but used their capacity to organize and control the production process to protect their economic, social, and political interests. Similarly, the Ugandan state (like the British colonial government) was not able to dominate the Bagisu, but had to negotiate with them and develop exchange relationships where profits and development flowed up-country in return for a steady and substantial flow of the commodity. A critically important insight, furthermore, is that this moderate, limited state was perhaps an optimal state for all concerned. This is because the drive of the state to extract ever additional capital from agriculture is not controlled by any dynamics internal to the central state. If unconfined, it will squeeze the smallholder until he ceases production, and the state will collapse economically. The "strong" state becomes the

impotent state, as recent history in Ghana and Tanzania suggest. Thus a state forced to negotiate with its citizens may be, in the long run, a more powerful entity than one seeking dominance.

The important role played by political intermediaries such as local administrators, party and cooperative officials, and members of parliament, is also demonstrated here. These officials act as the "balancing function" between state and small producer. They must retain the confidence of both to maintain their economically privileged status, and to do so they must extract enough wealth from the producers to sustain the state but strike a good enough deal for the producers to keep them growing cash crops. The high priority on dominating this political space played by militant regimes such as those of Nyerere, Nkrumah, and Toure can thus be seen to have damaged or destroyed a critical resource for the state.

Bunker thus shows us that smallholders are not passive subjects unable to affect the world they work and live in; dependency dynamics and class analysis do not, at least in this case, explain domestic political-economic relationships in Africa in any simple fashion; "strong" or militant states are in effect "soft" states (ineffective) in their ability to actually reach such goals as economic development; the unconstrained coercive power of the state is a blunt, ineffective, and ultimately destructive tool of social organization; nominally centralized governments are dependent on independent and decentralized social and political structures for management and organization of key sections of the economy; and states that allow (or accept) existence of political space for their citizens to negotiate "win-win" arrangements are more effective development agents than those which demand control and conformity. It is through detailed, careful, theoretically sophisticated but microbased field studies such as this one that our understanding of African politics and the appropriate concepts to use with it and our ability to contribute to resolving current problems will improve.

JAMES S. WUNSCH

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Futures for the Welfare State. Edited by Norman Furniss (Bloomington: Indiana University Press, 1986. vi, 444p. \$22.50).

In the midst of continuing political and intellectual controversy concerning the viability of established welfare systems under recurrent conditions of international economic instability, this volume of original essays stands out as a thoughtful and informative contribution. The various authors address important issues of interest to comparative scholars and U.S. specialists alike. Their focuses and analytical methods differ widely, but they affirm in common the historical and contemporary significance of welfare institutions and provisions as ameliorative features of modern capitalism.

The book consists of 13 chapters, approximately half of which were originally presented as papers at a conference held at Indiana University in 1983. A total of 21 U.S. and European scholars collaborated to produce the final product.

Substantively, the various essays address three broad sets of topics, each of which corresponds to a major section of the volume. Those in part 1 deal with general conceptual and methodological questions pertaining to the analysis of the welfare state; essays in part 2 assess empirical trends in a number of advanced welfare countries, with particular emphasis on Scandinavia; and chapters in part 3 focus on the U.S. experience. Unifying the essays is a shared belief among the contributors that the established welfare systems of Western Europe and North America are the product of diverse political, economic, and social factors and that, within limits, their futures remain open to conscious political choice among key policy actors.

This thesis argument is forcibly advanced by Timothy Tilton in an opening chapter explicating key research problems pertaining to the welfare state. On the basis of a critical assessment of alternative theories of welfare enactment and retrenchment, Tilton concludes that industrialization, capitalism, and other ostensibly deterministic factors do not provide a sufficient explanation for welfare state development and performance. A more convincing explanation must begin, instead, with recognition of the institutional capacity of the state for autonomous action. Even more important, he adds (affirming the view of

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Walter Korpi and others), is the nature of the prevailing balance of political forces in a given society. Thus, whether or not slower rates of economic growth and increased budgetary deficits will result in a reduction of welfare provisions depends on the composition and values of a nation's governing coalition.

Tilton's emphasis on the centrality of politics is underscored in a number of other essays, notably those by Douglas Ashford, John Logue, and Uriel Rosenthal. In a penetrating if sometimes murky analysis, Ashford concentrates on the role of high-level civil servants in the historical development of the welfare state in Britain and France. He argues, in the British case, that the prior existence of a centralized and bureaucratically efficient system of cabinet government resulted in the emergence of considerable institutional fragmentation and policy ineffectiveness, whereas in the case of the French a more adaptive and innovative national civil service achieved a more responsive array of policy instruments and programs. Ashford's larger point—which is eminently worthy of more extensive empirical analysis than is possible in a single chapter—is that contrasting institutional choices have resulted in significant national differences in policy cohesion and provision among the Western democracies.

Logue's chapter, in the second part of the book, concentrates on more contemporary facets of welfare state development. His focus is Denmark and Sweden, where the very success of the welfare state, he contends, has generated serious social and political problems. In the former instance, "the solidaristic values of the generation that built the welfare state are giving way to more egotistical values in the generation that grew up with it" (p. 268). As evidence he cites (without providing empirical data) an increased tendency on the part of citizens to utilize welfare services while simultaneously seeking to minimize the personal cost of such services through various legal means of tax avoidance. Politically, the formidable size of the public sector in Scandinavia, and ideological constraints on the part of both the Social Democrats and the non-socialist parties, are key factors that seriously restrict public efforts to undertake significant retrenchment measures in response to economic and fiscal crises. Logue's best hope for the welfare state is "general constraint" among its beneficiaries—to be achieved, if necessary, through government manipulation of in-

dividual incentives via changes in the tax system for example.

A third chapter worthy of note is Uriel Rosenthal's spirited effort in the final section of the volume to promote greater conceptual clarity about the relation between welfare society and the welfare state. Any attempt to theorize about the contemporary and future role of the welfare state, he argues, must build on this distinction while simultaneously taking into consideration configurative psychological and historical factors.

Furniss echoes Rosenthal's emphasis on the dichotomy between the state and civil society in a concluding summary essay. Noting that the relationship between society and the state in advanced capitalist democracies is "essentially adversarial and unstable" (p. 389), Furniss restates the view of Tilton, Ashford, and others that the welfare state emerged as a result of purposeful political action. To the extent that the modern welfare state has succeeded in regulating conflict among competing social forces even under conditions of recurrent economic crises, apocalyptic predictions about its imminent demise are at best highly premature.

Other essays stand out because of their methodological or empirical rigor. Among them are chapters by Jerald Hage on techniques for delineating periods of welfare state development, Don Schwerin's argument that large public sectors do not necessarily undermine domestic economic efficiency, and an incisive analysis by Larry Griffin and Kevin Leicht of the "politicization" of welfare expenditures in the United States through the budgetary process. Also useful for reference purposes is a fairly comprehensive bibliographical essay on central themes of mainstream research on welfare-related principles and issues.

No brief assessment can adequately summarize and critique as diverse and complex a collection as this. As a whole, the volume is an impressive achievement, flawed only by minor irritants such as the omission of an index and occasional editorial lapses and printer's errors. The sum of the essays is a reasoned, conceptually innovative, and empirically rich overview of contrasting patterns of welfare state development and future potentials.

M. DONALD HANCOCK

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The State and the City. By Ted Robert Gurr and Desmond S. King (Chicago: University of Chicago Press, 1987. ix, 242p. \$35.00, cloth; \$14.95, paper).

Invisible to much of the profession, under the general rubric *political economy* intense ideological battles are being waged in the study of urban politics. A range of analysts with varying perspectives, including, but by no means limited to, neo-Marxists, neo-Weberians, neoconservatives, and pluralists, have turned to the study of urban political, economic, and social phenomena as a battleground for their diverse theories.

Gurr and King join the fray, taking what they call a "state-centered" approach to the study of change in local government. They argue that national and regional "states" have distinct interests in cities (the "local state"), and that the growth and decline of cities is as much the result of the incentives, policies, and actions of these "states" as they are of the social and economic trends stressed in other theoretical perspectives.

The book consists of two distinct sections, of widely varying quality. The first two chapters plus the conclusion serve as an excellent window into the ideological debates now raging. Using their state-centered perspective, Gurr and King review and criticize a variety of alternative theoretical perspectives, attempting to integrate them into their state-centered approach. For example, dismissing rational choice perspectives, which emphasize the bureaucratic desire for budget maximization or the politician's need for reelection, Gurr and King argue that such models emphasize "short-term" objectives, while the state-centered approach they advocate emphasizes long-term concerns for the pursuit of programs and the maintenance of patterns of power and authority. Their analysis stresses the "programmatic commitments" bureaucrats and politicians have to social and political objectives that transcend the short-term interests emphasized in rational choice models.

Similarly, the authors reject the emphasis on economic forces that underlies Marxist theories. Their state-centered approach emphasizes the interest of the state in reproducing and legitimizing *itself*, not the capitalist system. Gurr and King also reject the "economic determinism" they argue underlies

Paul Peterson's relatively mainstream analysis in *City Limits*. While economic forces clearly affect the ability, capacity, and legitimacy of the "local state," the authors explore the limits imposed by the interests of the "national state."

In general, these chapters are provocative and serve as a valuable introduction into an area of intense scholarly conflict. Unfortunately, the other chapters of the book are much weaker. In chapters 3-5 the authors present an analysis of urban politics in the United States and Great Britain using their state-centered perspective. A truly comparative analysis is not presented. While the authors recognize that their comparative "approach" emphasizes differences rather than "equivalences" (p. 184), I found two radically different analyses and the points of direct comparison too few and far between.

A more serious problem is that the analysis of the U.S. situation is weak and unconvincing. In chapter 4, the authors make a fundamental error in their analysis of intergovernmental aid. Underlying their argument is the singular importance they attach to the programmatic interests of federal *bureaucrats* in allocating aid to various cities. While this argument makes sense from their state-centered perspective, it totally misses the complexity of the intergovernmental system in the United States, a system in which *politicians* in Congress play a major role in setting formulas for the allocation and distribution of federal aid.

Moreover the empirical analysis is unconvincing. Both the bivariate correlation analysis and the multivariate "dynamic" analysis produce weak results, and the enterprise is marred by high collinearity in the independent variables. While the authors call the empirical support for their state-centered model "modest" (p. 148), this is a generous assessment at best.

In the concluding chapter, the authors return to their strengths, discussing future developments in urban growth. The concern for identifying characteristic patterns of urban growth is common to much of the urban political economic analysis now being undertaken. Following in this tradition, Gurr and King identify four possible types of cities they believe are emerging: old industrial cities, new industrial cities, administration-service cities, and welfare cities. They summarize the economic, political, social, and technical forces

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producing these patterns of growth and identify the likely future for each type of city. The arguments presented in this chapter are important for understanding urban development and for understanding much of the genre of present urban political economic research.

In the late 1950s and early 1960s intense ideological conflicts between pluralists (spurred on by Dahl's work in New Haven) and elitists (following Hunter's work in Atlanta) eventually spilled over from the study of urban politics to the discipline at large. Whether or not the present debate will transcend its current boundaries remains to be seen. I believe one reason the debate might not cross over is that the level of technical analysis brought to bear is often not up to professional standards. In this regard, *The State and the City* acts as a perfect window into ongoing research on urban political economy: intense and sophisticated ideological and theoretical analysis coupled with weak empirical work.

MARK SCHNEIDER

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Governing the Economy. The Politics of State Intervention in Britain and France. By Peter Hall (New York: Oxford University Press, 1986. viii, 341p. \$36.00, cloth; \$14.95, paper).

In this cowinner of the 1987 Woodrow Wilson Foundation Award, Peter Hall offers an impressive diagnosis of the "British disease" and an explanation of how the French avoided it. In the process he proposes a style of policy analysis that focuses on the nature of institutions on both sides of the state-society divide. His emphasis on the interdependence of society and polity is reminiscent of systems theory, although he rejects structural functionalism for its assumptions regarding universal functions and system maintenance. Hall has pithy and perceptive comments on the neglect of institutions in several current styles of policy analysis. For example, on public choice he notes that "the notion of simple rationality is relatively meaningless as a description of motivation un-

til it is given more content by a specification of the organizational conditions that identify the direction in which rational action is aimed and the means it is likely to apply" (p. 13). On state-centered theories that postulate state autonomy, he warns that "institutional networks affecting state action extend well into society, in such a way as to expose the state again to societal influences" (p. 17).

In Hall's judgment, Britain's relatively slow growth rates in the postwar period are not attributable primarily to government restrictions on the market but rather to inefficiencies grounded in the structure of the market itself. These inefficiencies, in turn, are the result of a historical experience of imperialism and early industrialization. The city's orientation toward overseas lending made it a powerful proponent of deflation. The British economy suffered from the absence of large investment banks of the sort that emerged to support late industrialization in Germany and survived to guide industrial modernization in that country. Britain's trade unions and employers associations appeared to be robust but ultimately proved incapable of imposing discipline on their members. The government found itself unable to reduce inefficiencies in the market through corporatist agreements, even on those rare occasions, as in the late 1960s, when it was inclined to do so. Moreover, state agencies were embedded in a "labyrinth of institutional relations" which inhibited their desire for change (p. 68). The powerful Bank of England, like the city, worried most about inflation, while the treasury, perceiving its task to be control of public expenditures, had little interest or expertise in industrial policy.

Following the path that Andrew Shonfield blazed over 20 years ago, Hall looks to French planning as an example of effective state intervention. Yet, more than Shonfield, he is aware that French planners were less important for the specific targets they proposed (which the government often ignored) than for their successful efforts at spreading the gospel of economic growth through modernization to businessmen, politicians, and bureaucrats. Again Hall explains state intervention in France essentially in terms of institutions. The comparative weakness of trade unions, coupled with the discredited status of employers associations after their affiliation with Vichy, offered unique opportunities for the planners.

In contrast to Britain, where state aid to industry for things like regional development had little effect on efficiency, French government officials used their resources to strengthen sectors—and specific firms—with growth potential.

Throughout the chapters on British and French economic policy, Hall offers useful insights into the complex interrelationships among historical experience, institutions, and ideas. And yet his book is unlikely to end the debate over industrial policy. His admiration for French *étatisme*, which runs through the chapters on British policy, is not fully supported by his own account of the French experience. He is aware that the plan had limited influence over policy even in its heyday, then was reduced to irrelevance or to a simple statement of the government's spending priorities after the oil crisis of 1974. He notes that the "national champions" policy of the 1960s produced gigantic failures like Plan Calcul, white elephants like the Concorde, and expensive public charges like the steel industry. The French economic miracle was indeed impressive, but was it primarily the work of state intervention? It coincided with rapid growth in most Western economies and was no more impressive than its Italian counterpart, which, some would argue, occurred as much despite as because of state intervention.

Were he writing today, Hall might be less pessimistic about the prospects for Thatcher's strategy of modernization through the market. He writes, "if the diagnosis of Britain's long-term economic decline offered in chapter one is correct, the Thatcher experiment does not augur well" (p. 132). In view of Britain's growth rate of over 5% in 1987—among the highest in the Western world—one might question Hall's pessimism but not necessarily his analysis. The changing structure of the British work force will make it difficult for unions to recover their strength of the 1960s and 1970s. As for the status quo mentality of civil servants and businessmen, is it not possible that the Thatcher government could be playing the same resocializing role Hall attributes to the Commission du Plan in France?

Although one may quibble with the author over particular interpretations, there is little doubt that this is an important book, one that demonstrates that policy can be understood only in the context of institutions and their

historical development. It is a book that merits careful study and emulation.

JOHN S. AMBLER

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Neighborhood Justice in Capitalist Society. By Richard Hofrichter (Westport, CT: Greenwood, 1987. xodiii, 192p. \$35.00).

"Neighborhood justice" refers to a national judicial reform movement which in the last 15 years has established over 350 community mediation programs throughout the United States. Similar to the juvenile court and small claims court reforms initiated at the beginning of this century, the neighborhood justice movement is characteristic of judicial efforts to institute informal procedures, such as mediation, for resolving family and neighborhood disputes. Unlike earlier informal procedure reforms, however, neighborhood dispute resolution programs are the product of federal planning and nationally based institutions. In this book Hofrichter explores the politics of state-planned dispute resolution programs that appear as decentralized community entities.

This book offers a sophisticated analysis of the role of law in maintaining social order and responding to competing demands on the state. Hofrichter does not replicate the traditional approach to analyzing reform, an approach that often seeks to measure the success of failure of reform. Instead he is concerned with understanding the organization and politics of social regulation under capitalism. Neighborhood mediation is explained from an analysis of the capitalist state and hegemony. Building upon contemporary Marxist theories of the state, Hofrichter theorizes the conditions that produce neighborhood dispute resolution.

Hofrichter's thesis is that informalism is a new form of crisis management. There are several preliminary steps in his analysis and key among them is his theory of the informal state. Hofrichter describes a change in the form of state power and examines manifestations of it at the community level. He argues quite persuasively that "the community itself, its institutions and physical space become implicated in production and reproduction" of state power (p. 70). A new form of power, informalism, leads to an expansion of the state;

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but it is not necessarily a strategy for obscuring state power. Although he argues that the informal state "appears as [a] nonstate, employing ideologies and practices from everyday life" (p. 71), Hofrichter also maintains that the flexibility of informalism is compatible with an "increasingly unpredictable social environment which results from capital expansion itself" (p. 44). He further situates the role of communities in a larger context of redefining state power by discussing a variety of examples of the informal state, such as community mental health and worker participation schemes.

The nature of order in a liberal society is not treated as merely an internal problem for legal institutions, but in Hofrichter's study the subject matter of neighborhood dispute resolution—minor criminal and civil disputes between people who know one another—"reflect the tensions of life as lived in capitalist society" (p. 10). Drawing on the work of Ira Katznelson and others who link citizens and workers to political interests, Hofrichter maintains that the neighborhood dispute resolution center is "a response to disorganize community challenges by demobilizing and absorbing demands at the individual level" (p. 22).

The book is a significant contribution to research on law and social control. The analysis resists a tendency in political science and sociology to reduce law to an instrument of state power and thereby view law solely as a social control mechanism. Hofrichter does not adopt a determinist theory of state power but instead explores the role of legitimacy and its pursuit in producing *indeterminacy*. The informal state, he argues, embodies "ideologies that integrate people into the social order" and "oppose instrumental accumulation rationalities." Furthermore, he argues that there is a "contradiction between the rationalization or instrumental reason necessary for capital expansion and the cultural forms of everyday life which generate different social rhythms and oppose technocratic logics" (p. 42).

With a theoretical grounding in Marxist crisis theory, Hofrichter poses the following question for his empirical study of mediation: How does the informal state, which "coordinates and plans the infrastructure of communities through new, undemocratic administrative institutions, expect to manage community life without jeopardizing its legitimacy?" (pp. 76–77). Although his theory has

already provided an answer to this question, he does illustrate how the search for legitimacy produces internal contradictions for neighborhood dispute resolution. Hofrichter gathered data from observations of mediation sessions and from transcripts of the hearings. He found that mediation is organized around sets of structured rules that guide disputants toward particular kinds of agreements. Through his analysis of the hearing process he shows that mediators, who are lay citizens trained in mediation techniques of conflict resolution, are not passive participants but authoritative interveners. From these findings Hofrichter concludes that the organization and dynamics of the neighborhood dispute resolution process produce a legitimacy problem for the informal state. What was to be informal and flexible is formal and inflexible: "The rules are hegemonic because they restrict and direct the organization of the experience in the situation, while giving the appearance of something else" (p. 129).

Hofrichter has blended together a rich theoretical perspective with an original analysis of mediation hearings. This work provides a challenging new approach to studies of state power and the politics of law. It also takes us beyond the conventional focus of policy implementation studies by demonstrating the dynamics of legal procedures and the complexities of crisis management.

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Congress: Structure and Policy. Edited by Mathew D. McCubbins and Terry Sullivan (Cambridge: Cambridge University Press, 1987. xii, 563p. \$49.50, cloth; \$16.95, paper).

Do policies make institutions, or do institutions make policies? The answer to this chicken-and-egg question turns upon one's research assumptions and objectives. The present collection, reflecting currently fashionable scholarly concerns that the editors term "new institutionalism," takes the latter point of departure.

The "modern era" of congressional research began when behavioralists, borrowing concepts and methods from sociology and anthro-

pology, began to study Congress as a complex human organization. They emphasized leaders and followers, subgroups and factions, lines of communication, and especially norms and folkways. The result was a descriptively rich portrait of the post-World War II Congress. This system was transformed, first by wide-ranging structural reform and then by agenda shifts that brought sharp debates over policy content and priorities. Not by coincidence, political scientists turned to economic models to address a new question—how Congress's structure dictates certain decision sequences and policy forms.

Signaling this paradigm shift, the McCubbins-Sullivan collection asks (in the series editors' words), "How do institutions evolve in response to individual incentives, strategies, and choices; and how do institutions affect the performance of political and economic systems?" (p. ix). Compare this with the questions raised by a recent collection of a more behavioral flavor: "How are decisions made within the political system? How and why do institutions change?" (Glenn R. Parker, ed., *Studies of Congress* [1985], xi).

Comparing the McCubbins-Sullivan and Parker volumes is instructive because they have many things in common (length, quality of articles, lucidity of editors' essays, and even some of the contributors). The two have comparable sections on elections and such topics as parties, committees, and leadership (though Parker gives more attention to leadership). It is in the final sections that the two books part company. Whereas Parker's voting studies are about factions, coalitions, and constituent influences, McCubbins and Sullivan are more intrigued by theoretical models of voting choice. The payoff of the newer approach comes in the final section, not duplicated in the other work—the impact of institutional arrangements upon public policies.

The early Mayhew-Fiorina formulation has much prominence, although the editors suggest (pp. 14-15) that important modifications must be introduced to overcome some of its shortcomings. Jacobson's informative, sensible essay brings things up to date and fills in the gaps. Nonetheless, the impression conveyed by this literature is that elected officials are free to bend congressional structures and bureaucratic functions to their reelection needs. In fact, Congress is not "perfectly designed to

serve the election needs of its members" (p. 73). Leaving aside members' nonelectoral goals, governmental structures respond to contextual factors and agendas that often vastly complicate office-holding tasks. If the 1970s didn't make this disjunction between individual incentives and institutional imperatives obvious, the 1980s have dramatically done so.

Members' complex motives come into play in the institutional pieces—for example, Fenno's comparative analysis of House committees. It is hard to overstate the importance of committees, but the autonomy of committees as decision units may be partly an artifact of the pre-1970s era. Committee boundaries are more easily overstepped in the Senate, but the House is following suit. At a time when more than a quarter of the average House committee's workload is shared by one or more other panels, we need to rethink our assumptions about committee operations.

The early sections of the book, mostly conventional in content, serve as foundation for the editors' ultimate concern: how legislative decision rules mandate consistent patterns of policy. There is a lucid introductory essay by Thomas Schwartz, along with important articles by Shepsle, Ferejohn, Arnold, Noll, Krehbiel, and others. The authors make a solid case for the utility of the public choice approach to legislative decision making; their volume will introduce students and general readers to this literature, which has achieved much currency in scholarly journals.

While making their case for this viewpoint, the editors are broad-gauged enough to appreciate the legacy of other approaches, and to admit the limits of the rational choice paradigm. As they note, the approach rests on an assumption of stability—not a bad assumption for our system. Yet in periods of rapid change or closely contested policies, legislative decisions may be unstable and frequently reversed.

Institutions themselves are malleable products of policy struggles. Those of us who watch Congress closely are forever reminded of the pliability of rules and structures, and the ingenious ways they can be bent or breached to serve the purposes of the moment. Bills may be passed without committee deliberation or even referral; jurisdictions are often trespassed; parliamentary rules are ignored or waived, even constitutional provisions circum-

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vented (for example, that revenue bills originate in the House). Any calculations based on an immutable set of procedural "givens" is bound to distort reality. Although institutional arrangements are inertial (p. 316), they are constantly in motion.

Policy choices and institutional arrangements influence one another. True, institutions are bequeathed each generation of policy-makers, quite apart from the policy agendas that may emerge. These institutional arrangements surely shape the way "problems" are interpreted and "solutions" chosen. But changing agendas push and stretch institutional structures, sometimes altering them profoundly even when outward forms remain intact. This is the lesson of Lowi and others: that policy type affects the way policy is handled.

McCubbins and Sullivan have produced an attractive showcase for the "new institutionalism." For the behavioralists' interpersonal equilibrium, however, has this new formalism substituted a stasis of institutional "rules"? Each approach has important, powerful things to tell us about the institution and its policies. Yet each leaves key questions unanswered.

ROGER H. DAVIDSON

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Exploring Long Cycles. Edited by George Modelski (Boulder: Lynne Rienner, 1987. x, 277p. \$30.00).

The appearance of a collection of papers by Modelski and students provides an opportunity to assess the project associated with his name and studying the cyclical rise and fall of the great powers since the sixteenth century. There wasn't much data in these articles, and since the original Modelski piece in 1978, there does not appear to have been much concrete empirical research on documenting the actual periodicities of these purported cycles, save for the work of William Thompson and Karen Rasler. Simply put, Thompson and Rasler deserve more space and attention in this volume and more credit in general for continually trying to gather data and test ideas about the existence of long waves or cycles. In a very real sense they have been carrying this tradition with their continued high-quality journal articles, while Modelski seems content

to repeat the initial observation of the original 1978 article as if there was no more research to be done.

The actual workings of cycles, let alone their very presence, needs research—real data about the world and not just the assumption that it exists. The recent work of Joshua Goldstein comes immediately to mind. In economics, where long statistical series are much more available, there is still great debate about the presence or absence of long swings, a skepticism that could shed some light and new thinking on the seemingly fixed Modelski scheme. Concerning data, there is a forthcoming book by Thompson and Modelski containing measures of sea power over these centuries, and it is anxiously awaited by all interested in the long wave, or cycle, phenomena.

There also needs to be more general reading in this tradition. There is simply too much self-reference among the annointed. Given the growing literatures in sociology and economics on long-wave phenomena, encountering other literatures would be most helpful. I think here specifically of the work of David Gordon, Edwards and Reich, G. Mensch, the Braudel scholars around Immanuel Wallerstein, Paul Kennedy, Andre Gunder Frank, and others. This would be particularly helpful because these authors have different dynamics in mind and the present system dynamics model favored by Modelski contains so much Parsonian gibberish that some new approaches would be welcome.

Following that point, something needs to be said about the prohegemonic sentiment that is involved in conceptualizing the dominance of a single state as a response to the global demand for "world leadership." This approach simply ignores the tremendous exercise of force in maintaining a hegemony and the differential economic benefits that flow to the hegemon. Modelski's assertion that "global war has played [the role] of the regular election process" (p. 126)—deciding, I suppose, the next hegemon—goes way beyond simply a benign view of the clearly crisis-prone nature of the international system. Given the presence of other ideas about war and the role of the hegemon it would certainly help the Modelski project to consider other implications of world dominance than simply that of leadership meeting a world demand for international order. There is a deep pathology to interna-

tional life, nicely identified by Kenneth Boulding, that is simply masked in this functionalism.

ALBERT BERGESEN

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Politics in the Semi-Periphery: Early Parliamentarism and Late Industrialisation in the Balkans and Latin America. By Nicos P. Mouzellis (London: Macmillan, 1986. xix, 284p. \$32.50).

This book analyzes the political trajectory of Argentina, Chile, Greece, and—secondarily—Bulgaria and Yugoslavia over the course of a century. It treats these countries, especially the first three, as a distinct unit, whose overarching similarity lies in the fact that they established parliamentary—albeit oligarchic—rule in the nineteenth century despite being in the at-best-incipiently industrialized semiperiphery of the world capitalist system.

The author notes that there have been three phases in their political evolution, each one of which is different from parallel developments in either the central or peripheral capitalist nations. The first, that of oligarchic parliamentarism, was characterized by a relatively strong state, a weak civil society, given the dearth of associational life (itself a product of the absence of *corps intermédiaries* under colonial rule), and a paucity of popular political participation and mobilization, as the masses were either excluded altogether or subjected to clientelistic arrangements. This phase was relatively stable, given the fact that the societies were mostly rural and agrarian.

During the second phase, roughly from the end of World War I to the 1950s, the polity is changed by an increase in middle class and popular mobilization, both partly the result of growing industrialization and urbanization. Unlike similar developments in the developed West, however, in the semiperiphery the popular sectors are not able to achieve the formation of autonomous political and union organizations, which would have led to their "integration." Rather, they are "incorporated-excluded" (not "integrated") into the polity by a more widespread recourse either to clientelism, which occurs in the less urbanized and in-

dustrialized of these semiperipheral countries, or to populism, which is a phenomenon of the more urbanized and developed ones. In either case, the broad masses are still subjected to extensive controls by elites.

In the third phase, the popular sectors further increase their mobilization and demands, thereby forcing the limits of the "incorporation-exclusion" institutions of social and political control. At this point, the military becomes an increasingly important political actor, and it finally opts for taking over the government in order to install an authoritarian regime that will once again restrict popular political participation.

While much of the book discusses the political histories of the chosen countries (especially Argentina, Chile, and Greece) in order to illustrate these phases, it also contains theoretical analysis of the concepts of populism and clientelism and of the shortcomings of various conceptual approaches, particularly the Marxist, for the analysis of states and politics. I thought these latter arguments, which comprise the bulk of chapter 4, were the best in the book although they are not particularly novel; Mouzellis calls for an integration of Marxist and Weberian approaches, generating a class as well as an organizational and state-focused perspective.

The author is to be commended for attempting to do a broad-ranging comparison of cases drawn from different areas. However, I did not find this book's analysis particularly convincing. Firstly, any good comparative analysis must begin by noting the universe of cases to which the question being posed applies (the question in this work refers to the characteristics of the political evolution since the late nineteenth century of semiperipheral countries with early parliamentary political institutions). And yet the roster of cases that correspond to this generic type includes a good many more examples, such as Ireland, Portugal, Spain, Colombia, Uruguay, Peru, Brazil, and others, which are hardly if at all mentioned in the book. The author would have to explain more carefully his selection of cases or include more of them, in order to make a stronger argument for the fact that such countries do have a uniquely similar pattern of political development. Similarly, the author should explain more explicitly what the differences are between institutions in the semiperiphery and

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those in central countries, in order to buttress his arguments regarding the former.

Secondly, I was not impressed by the historical and analytical treatment of the Chilean case. I simply do not think that this case fits all that well the pattern Mouzelis puts it into. He is somewhat wary of his own treatment of Chile, given the fact that he frequently notes that Chile may constitute a "partial exception" to his generalizations, particularly regarding populism and the politicization of the military. The citations to the Chilean literature are not well done, and it is surprising that despite a lengthy discussion of the events leading to the military coup of 1973, there are no references to Arturo Valenzuela's well-known book on the subject. Valenzuela explicitly deals with, and rejects, the thesis Mouzelis (and others, such as Henry Landsberger and Tim McDaniel, who are also not cited) advance, namely, that the Chilean coup resulted from an excessive degree of popular mobilization. Similarly, although some authors do make these arguments, it is nonetheless very debatable whether Chile made a transition out of an oligarchic parliamentary form of rule in the 1920s, whether its political system could be characterized as something less than a democracy, given what are in the text ill-specified "incorporative-excluding" institutions, and whether its military was as heavily involved in politics before 1970 as the text paints it. The Chilean case is, however, very important for the general argumentation of this book, since it is certainly one of the modal cases in Mouzelis's type of early parliamentarism in the semiperiphery. My impression is that the author was all too eager to force Chilean history into his categories; a more thorough understanding of it would probably have led him to modify them.

Finally, I missed seeing any references to the fact that the author is not alone in using the term *incorporation*. David Collier and Ruth Collier's work on labor employs the same expression, and Alfred Stepan's work on Peru uses the very similar term, *exclusionary corporatism*. A longer treatment of the specific characteristics of the "incorporative-excluding" institutions Mouzelis mentions, and of the differences—if any—between his conception of them and those of others would have been a welcome addition to this book.

And yet, despite these misgivings, this is a

provocative piece that may stimulate further comparative treatments and discussions of political development in these countries, which have been relatively neglected in the comparative politics literature.

J. SAMUEL VALENZUELA

University of Notre Dame

The Welfare State East and West. Edited by Richard Rose and Rei Shiratori (New York: Oxford University Press, 1986. 209p. \$29.95).

The study of advanced industrial societies has been invigorated in recent years by sustained comparative investigations of the modern welfare state. Inquiry into the origins and trajectories of social welfare policies and institutions has become one of the most lively and theoretically interesting areas in comparative studies and historical sociology. At the same time, theoretical advances in this field have been matched by a new awareness of the fragile and shifting socioeconomic foundations of the welfare state. We have begun to understand the social forces and historical structures upon which the welfare state is built just as those forces and structures have endangered and rendered problematic the public provision of social welfare.

Amidst these developments, this edited collection of empirical country studies provides an important and distinctive contribution to our understanding of the modern welfare state. This contribution involves both a broadening and a deepening of our focus. The volume broadens the research agenda by bringing Japan into the comparative framework; hence the attention to the welfare state, East and West. In many respects, Japan represents the first non-Western industrial society to emerge on the world stage. As such, its distinctive patterns of industrial capitalism and politics challenge our European and U.S. models of political economy and relationships of state and society.

The juxtaposition in this volume of the Japanese experience with those of the United States and European countries also serves to deepen the conceptions of social welfare, and this is the volume's most original contribution. The authors seek to probe not just differences

in the institutions and policy trajectories of social welfare, but also the larger social setting and cultural traditions that define national perceptions of welfare. In this sense, the editors are less concerned with presenting new theoretical explanations for welfare state development and more concerned with exploring national differences in what in fact constitutes social welfare. This leads Richard Rose to make a series of useful distinctions between public and private realms of welfare provision. When a "societywide approach to welfare" (p. 16) is adopted, the focus shifts to the role played by households and communities as well as that played by the state. Our theoretical attention must also shift to encompass cultural variations in the role of state, society, family, and the individual.

The strength of the volume is in the light it casts on national variations in the location and style of social welfare provision. The volume is less successful in advancing our understanding of the origins of these differences. The volume is helpful in revealing the divergent national experiences that need explanation; it is less helpful in advancing explanations of those differences. For those searching for concepts of the welfare state and for broad comparative sketches of alternative routes in welfare state development, this volume will be very illuminating. For those working in the explanatory trenches, the advice is to keep digging.

G. JOHN IKENBERRY

Princeton University

The Business of the Japanese State: Energy Markets in Comparative and Historical Perspective. By Richard J. Samuels (Ithaca: Cornell University Press, 1987. xii, 359p. \$45.00, cloth; \$14.95, paper).

This is a very good book on Japanese industrial policy, and on state intervention in markets generally. Samuels does everything right: he speaks to the key issues in the literature, systematically compares Japan to the relevant European examples, argues from solid historical research, and actually tests important hypotheses. The facts he presents simply demolish the hypothesis that the main engine of Japan's economic success was the "capitalist developmental state"; his interpretations call

into question the usefulness of several theories of state intervention, and even of the very concept of "state."

The book is a "critical-case study" of the energy sector in Japan, surely as good a test of "state leadership" as can be imagined. Its heart is a chapter each on the modern political history of the coal (back 200 years), electric, oil, and alternative and nuclear energy industries in Japan, preceded by a chapter on parallel developments in Europe. No doubt specialists could find weak points in these accounts, but they are well documented and impressive in scope and detail. The major finding is that Japan is a unique "case of extensive state jurisdiction without a competitive commercial presence in the energy market place" (p. 257). Unlike Europe, where "national champion" state-owned corporations have been so common in the energy field, Japanese interventions have consistently been market-confirming, mostly in the form of funds and guarantees against risk. Moreover, the state rarely anticipated or caused market transformations but typically reacted after the fact.

That Japanese industrial policy is generally market-confirming and avoids direct controls has not been ignored by its more perceptive analysts, such as Chalmers Johnson (*MITI and the Japanese Economic Miracle* [1982]), but the usual explanation has been the wisdom, power, and skill of the economic bureaucrats—indirect methods as their strategy of choice. Samuels demonstrates that it is not for want of trying that the state does not participate directly. The Ministry of International Trade and Industry and its predecessors again and again tried to gain real control over energy markets, including the creation of national champions, and always failed. In not one of the 70 cases of market transformation described here was the state able to prevail over private interests; nearly always, it served them.

Does this book then revive the old image of Japan run by big business? Yes and no. Certainly it emphasizes the power of business actors—not just individual firms but also the critical and understudied political roles of various groupings of firms and, especially, of the banks. But business power is limited, both by internal competition among firms and groupings and by the countervailing power of other social interests (including labor in the

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postwar years) and of the state itself, which, while not fully autonomous, is nobody's captive.

Samuel's view thus generally fits into recent interpretations of Japanese politics as "patterned pluralism" and so forth, but more than others he stresses pervasive conflict, in the form of endless negotiation and renegotiation as changes in the economic, social, political, and international environments alter the resources, constraints, and opportunities of each actor. Over time, a norm of "reciprocal consent" emerges, under which "the state often helps structure market forces, but public/private negotiations invariably structure state and market choices alike" (p. 2). The politics of reciprocal consent can be found in Europe as well, but it is especially characteristic of Japan.

The main question for the comparative analysis is, Why no state ownership in Japan? and of course why something does *not* occur is difficult to explain. Samuel's research design focuses not on a single independent variable (e.g., What is the impact of industrial policy?) but on the dependent variable, market transformation (What causes changes in relations among producers and consumers?). In principle, he thus can balance state intervention against other causal factors: the strategies of private actors, outside forces, timing, and luck. In practice, Samuel proceeds mainly from the political science literature, evaluating six hypotheses that try to predict the extent and shape of state intervention in markets. These have to do with market structure, governmental centralization, developmental timing, openness to the international economy, and the nature of ruling coalitions. The results of testing these variables (non-quantitatively) among the national systems and of comparing Japanese subsectors and cases are not very promising for theory builders: each explanation is significant but sometimes in subtle or contradictory ways, and interaction often overwhelms the direct effects. Such hypotheses thus appear more useful as a checklist of factors for explicating particular cases than as a basis for a replicable theory. (Or perhaps a "process theory" rather than "variance theory" approach might work better.)

The hypothesis that works best is the breadth and stability of the ruling coalition. This conclusion rests on two sorts of evidence: market-disconfirming interventions in Europe

have often been associated with shifts in relatively narrow ruling coalitions; and stable rights of participation for many actors appear to be a necessary condition for the reciprocal consent norm, which inhibits control by one actor. It is striking, incidentally, that patterns much like reciprocal consent appear in quite different Japanese settings: the conflict leading to "consensus" in villages found by the anthropologist Robert C. Marshall (*Collective Decision Making in Rural Japan* [1984]); the intricate relationships among regional textile producers depicted by Ronald Dore (*Flexible Rigidities* [1986]); various accounts of company organization, the budgeting system, and even radical protest groups. Stability and inclusion are key factors in all these fragmented systems of institutionalized conflict. Dare one suggest that a cultural explanation—albeit more sophisticated than the usual "harmony" model—might also be considered?

The still broader context of this book is the recent literature on "the state," and the discussion—while quite interesting—tends to reinforce a suspicion that U.S. political scientists not weaned on Hegel usually find this concept more distracting than helpful. The problem is not just that the Japanese government is so penetrated by private interests; in two-thirds of Samuel's cases, divisions *within* the state affected outcomes significantly, indicating that agencies, groups, and subgovernments may be a more productive level of analysis. They have interests and strategies, as do the private actors, while the "state" apparently does not.

Given that industrial policy is the hot topic in the Japan field these days and that Japan is cited as the exemplar of strong state leadership of the economy in so many comparative studies, this book is bound to provoke widespread debate. Samuel's facts and arguments are sure to stand up very well.

JOHN CREIGHTON CAMPBELL

University of Michigan

Erratum

Jacobsohn, Gary J. *The Supreme Court and the Decline of Constitutional Aspiration*. Reviewed by Stanley Brubaker (September 1987, 997-98). Page 998, col. 2, line 27 should read "think, of the nobility of the constitution, but."

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ARTICLES

POLITICS, MARKETS, AND THE ORGANIZATION OF SCHOOLS

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We offer a comparative analysis of public and private schools, presenting data from a new national study—the Administrator and Teacher Survey—that expands on the pathbreaking *High School and Beyond* survey. We find that public and private schools are distinctively different in environment and organization. Most importantly, private schools are more likely to possess the characteristics widely believed to produce effectiveness. We argue throughout that the differences across the sectors are anchored in the logic of politics and markets. This argument derives from our belief that environmental context has pervasive consequences for the organization and operation of all schools and specifically that the key differences between public and private environments—and thus between public and private schools—derive from their characteristic methods of social control: the public schools are subordinates in a hierarchic system of democratic politics, whereas private schools are largely autonomous actors “controlled” by the market.

Virtually all public schools in the United States are governed by democratic institutions of the same basic form. This form is now taken for granted. There is a broad consensus that democratic control of the public schools is a good thing and that democratic control means control through local school boards, superintendents, central office bureaucracies, and corresponding apparatuses at the state and (increasingly) the federal levels. However heated the conflict over educational policy and practice, however intense the struggle for influence and resources, the “one best system” stands above it all (Tyack 1974).

In recent years, educational politics has centered on the quality of the public schools. Long-simmering discontent

about declining test scores, loose academic standards, and lax discipline—fueled by a series of national studies—has engendered a widespread reaction against the “rising tide of mediocrity” (National Commission on Excellence in Education 1983), and state legislatures around the country have responded with reforms ranging from stricter academic requirements to merit pay plans for teachers (Doyle and Hartle 1985). Throughout this period, the “one best system” has provided the institutional framework within which problems have been identified and policy responses chosen. It structures criticism and reform, but it is never their target.

Much the same is true within educational research, which has generally taken institutions as given. Studies of school effectiveness have focused directly on the

schools, asking about those aspects of organization and immediate environment that explain school performance. Taken as a whole, this work has promoted a loose consensus on factors that appear to enhance effectiveness, among them, clear school goals, rigorous academic requirements, an orderly climate, strong instructional leadership by the principal, teacher participation in decision making, cooperative principal-teacher relations, active parental involvement, and high expectations for student performance (Boyer 1983; Brookover et al. 1979; Goodlad 1984; Powell, Farrar, and Cohen 1985; Rutter et al. 1979; Sizer 1984).

This research has shaped the contours of public debate by suggesting traits good schools ought to have. But it is the institutional system itself, accepted by one and all, that tells us how these desirable features are to be transmitted to the schools: they are to be imposed from above. For many objectives—tougher academic requirements, say—reform simply calls for new legislative or district policy. Not coincidentally, these have been among the more popular reforms. Other objectives are less amenable to formal imposition—cooperative relations within the school, for example. But these tend to be regarded as matters of good management and training, and thus as reforms that can be delegated to the professional side of the control structure. Whether the means are formal or professional, then, the rationale of democratic control is to “make” schools more effective by imposing desirable traits on them (Campbell et al. 1985).

These reforms are likely to fail. To see why, it is useful to begin with a curious feature of the way schools are conventionally understood. Among those who study education, it is received doctrine that schools are open systems and thus products of their environments (Scott and Meyer 1984; Weick 1976). By this logic it should follow that the organization and performance of schools are largely ex-

plainable by the environments that surround them. Different types of environments should tend to produce different types of schools. When schools turn out to have undesirable characteristics, the logical culprit is the environment—not the schools.

Yet studies of school effectiveness have rarely taken the environment seriously. They tend to explain poor performance in terms of variables inside and immediately outside the school—and then they turn to our institutions of democratic control to make the necessary changes. Our institutions, however, are core components of the very environment that by open systems reasoning is likely to have caused the problems in the first place. These studies should be asking, What is the relationship between democratic control and the organization of schools? Might there be something inherent in these institutions that systematically promotes organizations of a type no one really wants?

We do not pretend to have all the answers. We do think, however, that institutions are fundamental to an understanding of schools. Here we try to make a plausible case for this view by developing a theoretical argument and presenting some new evidence from a recent survey.

Our basic argument is that the organization of schools is largely endogenous to the system of institutional control in which the schools are embedded. Different systems of institutional control should tend to produce schools with distinctive patterns of characteristics. While we will specify these patterns in some detail, our most general claim is simply that the hierarchy of democratic control, the “one best system,” puts its stamp on the organization of our public schools and that this stamp holds the key to school quality and school reform.

Institutional issues are often difficult to explore through empirical research. This is especially true in studying the “one best system.” How can we study institutional

Organization of Schools

effects if there is only one, all-encompassing institution? An instructive way to proceed, we believe, is to compare public schools to those schools that fall outside the hierarchy of democratic control: private schools.

That is what we do here. We explore the logic of institutional control in the two sectors and derive implications for schools. This line of reasoning suggests that schools should indeed look different across sectors and, most importantly, that democratic control should inhibit the emergence of "effective school" characteristics. Using data obtained from a representative sample of public and private high schools, we compare them on a range of characteristics commonly associated with effective academic performance. These results consistently suggest that institutions are important determinants of school organization—and that, in consequence, public schools are quite literally at a systematic disadvantage.

Politics, Markets, and Control

Public schools are controlled by democratic authority and administration. The specifics vary from district to district and state to state, but the basic framework is remarkably uniform throughout the country. The private sector might seem to lack any comparable uniformity. Most private schools are affiliated with a church; some are elite preparatory schools; some are military academies; and there are other types as well (Kraushaar 1972). But they all have two important institutional features in common: society does not control them directly through democratic politics, and society does control them—indirectly—through the marketplace. Rather than marvel at diversity, we find it useful to think about schools in terms of these alternative institutions of social control. As a shorthand, we will refer to them as politics and markets.

We want to provide a little background

on how these institutions operate and what they imply for schools. We will not yet relate them to specific aspects of school organization. This will be done later when we turn to a discussion of the survey data.

Constituents and Consumers

Popular myth lauds the role of local citizens and their elected school boards in governing the public schools. But the fact is that the schools are not locally controlled and are not supposed to be. The state and federal governments have legitimate roles to play in financing schools, setting standards, and otherwise imposing their own policies. This means that U.S. citizens everywhere, whether or not they have children in school and whether or not they live in the district or even the state, have a legitimate hand in governing each school (Campbell et al. 1985; Wirt and Kirst 1982).

The proper constituency of even a single public school is a huge and heterogeneous one whose interests are variously represented by formally prescribed agents—politicians and administrators—at all levels of government. Parents and students, therefore, are but a small part of the legitimate constituency of "their own" schools. The schools are not meant to be theirs to control and are literally not supposed to provide the kind of education they might want. Public education is shaped by larger social purposes as defined by larger constituencies.

Private schools determine their own goals, standards, and methods. These may reflect the values of owners or patrons, or perhaps a collective such as a diocese. But the market imposes a fundamental constraint. Private schools provide services in exchange for payment, and unless heavily subsidized from the outside, they must please their consumers—students and parents—if they are to prosper. Whatever the constituency of the private school, therefore, it will surely be

much smaller and more homogeneous than the democratic constituency of the public school, and students and parents will occupy a much more central position within it.

Exit and Voice

In the private marketplace, educational choice is founded on what has come to be called, following Hirschman (1970), the *exit* option. If parents and students do not like the services they are being provided, they can exit and find another school whose offerings are more congruent with their needs. This process of selection promotes a match between what educational consumers want and what their schools supply. Matching is reinforced by the population effects (Alchian 1950) of selection: schools that fail to satisfy a sufficiently large clientele will be weeded out (or, if subsidized, become an increasing burden).

Selection also forges a strong bond between consumer satisfaction and organizational well-being. This gives schools incentives to please their clientele, as well as to set up *voice* mechanisms—committees, associations—that build a capacity for responsiveness into organizational structure. These incentives, too, promote a match—but they are not necessary for success. A school might rigidly adhere to purist doctrine yet succeed because that is what enough consumers happen to want. Either way, the result tends to be the same: a match.

In the public sector, popular control is built around voice. Exit plays a minimal role. The public school is usually a local monopoly, in the sense that all children living in a given area are assigned to a particular school. This does not eliminate choice, since parents can take account of school quality in deciding where to live. But residential decisions involve many factors in addition to education, and once they are made, sunk costs are high.¹ Low

or declining quality need not keep parents from moving into an area, and it is even less likely to prompt existing residents to pick up and leave.

It might prompt them to consider a private school. But here they confront a major disincentive: public schools are free, private schools are not. Due to this cost differential, the perceived value of private schools must far outweigh that of public schools if they are to win students. To put it the other way round, public schools, because they are relatively inexpensive, can attract students without seeming to be particularly good at educating them.

Lacking a real exit option, many parents and students will choose a public school despite dissatisfaction with its goals, methods, or personnel. Having done so, they have a right to voice their preferences through the democratic control structure—but everyone else has the same rights, and many are well armed and organized. Voice cannot remedy the mismatch between what parents and students want and what schools provide. Conflict and disharmony are built into the system.

Autonomy and Control

In the private sector, the exit option not only promotes harmony and responsiveness, it also promotes school autonomy. This is true even for schools that are part of a hierarchy, as the Catholic schools are. The reason is that most of the technology and resources needed to please clients are inherently present at the bottom of the hierarchy—in the school—since educational services are based on personal relationships and interactions, on continual feedback, and on the knowledge, skills, and experience of teachers. The school is thus in the best position to know how to enhance its own organizational well-being. Hierarchical control, or any external imposition, tends to be inefficient and counterproductive.²

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Central direction is important when superiors have an agenda of their own that cannot be pursued simply by pleasing clients. In the private sector, imposition of such an agenda involves a trade-off: if schools are constrained in their efforts to please clients, dissatisfied clients can leave. In some hierarchies—notably, those associated with churches—superiors may consider this an acceptable price; they may prefer “pure” schools to growing, prosperous ones. But it is still a price, one that threatens organizational well-being—and one that in the limit can be fatal. Thus, even if there are higher-order values to be pursued, the exit option discourages tight external control in favor of school autonomy.

In the public sector the institutional forces work in the opposite direction. The *raison d'être* of democratic control is to impose higher-order values on schools and thus limit their autonomy. Exit is an obstacle to control: when the governance structure imposes a policy on parents and students who disagree, exit allows them to avoid compliance by “voting with their feet,” thus defeating the purpose of the policy. But public officials do not have to take exit as a given. They can simply pass laws restricting its availability. While private decision makers value autonomy because it helps them cope with problems of exit, public officials eliminate exit in order to facilitate their imposition of higher-order values.

The drive to restrict autonomy is built into the incentive structures of politicians and bureaucrats. Politicians seek political support by responding to various constituency groups, particularly those that are well organized and active. These include teachers' unions and associations of administrators, but also a vast array of groups representing more specialized interests—those of minorities, the handicapped, bilingual education, drivers' education, schools of education, book publishers, and accrediting and testing

organizations, among others. These groups typically have financial or occupational stakes in existing educational arrangements, and their policy positions reflect as much. They all want a share of the public's educational resources. They want to influence educational programs. They want to have a say in how the schools are organized and operated. And politicians are only too happy to oblige—this is the path to political popularity (Masters, Salisbury, and Eliot 1964; Iannaccone 1967; Peterson 1976; Wirt and Kirst 1982).

Bureaucrats play both sides of the governmental fence. Their power rests on the fact that bureaucracy is essential to direct democratic control. The imposition of higher-order values is hardly automatic, particularly given the built-in dissatisfaction of parents and students and the inevitable pressures from teachers and principals for autonomy. Control requires rules and regulations, monitoring, incentive structures, and other means of ensuring that those engaged in the educational process behave as they are supposed to behave. It requires bureaucracy—and bureaucrats.³

As public officials they have incentives to expand their budgets, programs, and administrative controls. These are the basics of bureaucratic well-being, and their pursuit is an integral part of the job. But bureaucrats also belong to important interest groups—of administrators, of professionals—that lobby government from the outside (ostensibly) as well. Although traditionally they have portrayed themselves as nonpolitical experts pursuing the greater good, they are in fact a powerful special interest—an interest dedicated to hierarchical control (Knott and Miller 1987; Tyack 1974).

The system, in short, is inherently destructive of autonomy. Politicians have the authority to shape the schools through public policy, and, precisely because they have this authority, they are consistently

under pressure from interest groups to exercise it. It is in their own best interests to impose choices on the schools. The same is true of bureaucrats, who have occupational and professional stakes in control: a world of autonomous schools would be a world without educational bureaucrats. Thus, while principals and teachers may praise the virtues of autonomy, the "one best system" is organized against it. Politicians, bureaucrats, and virtually the full spectrum of interest groups tend to see autonomy for what it is: a transfer of power and a threat to their interests.⁴

Purpose and Performance

Public schools are products of public policy. With a huge constituency, there is inevitably dissension over what constitutes "good" policy—and many of the contending groups have their own stakes in public education and are not simply struggling to provide us with "good" policy anyway. Even if they were, there is no guarantee they could implement it very effectively, for bureaucratic control is an inherently difficult and costly means of engineering educational outcomes.

Reform grows naturally out of all this. When important groups signal their dissatisfaction with what the schools are doing, politicians and bureaucrats spring into action. They respond to group demands by doing what they are institutionally empowered and motivated to do: they seek remedies through new policies and new controls. This is the characteristic way in which the public schools are "improved." And because administrative problems, value conflicts, and shifts in power alignments are endemic to the system, reform is a never-ending process.

Private schools operate in a wholly different institutional environment. Reform occurs when schools find it in their own best interests to make adaptive adjustments, when new schools enter the educa-

tional marketplace, and when unpopular schools fail. All are closely tied to the interests of parents and students.

Are private schools also likely to be better than public schools? In an important sense, the answer is *yes*. Parents and students who choose a private school are revealing their judgment of quality: the private school is not only better, it is better by an amount that exceeds the cost differential. Since there is clearly an objective basis for their judgment—they directly experience private education and are free to return to the public sector at any time—we have good reason to believe that private schools are in fact more effective at providing the types of educational services their clients care about.

But are they better at the important things schools ought to be doing? This question cannot be answered without substituting our value judgments for those of parents and students. A church school that attracts students on the basis of religious and moral training almost surely outperforms the local public school on this dimension. But this says nothing about their relative effectiveness in transmitting democratic values or an appreciation of cultural diversity. Performance is only desirable if the goals are desirable.

This, of course, is the justification for democratic control. In principle, our institutions are set up to articulate important social goals and to ensure that schools act effectively on them. If private schools do a better job of providing certain services or of pleasing parents and students, this does not mean that society must therefore prefer private to public education. Any evaluation has to depend on a more fundamental judgment about what the schools ought to be doing.

In objective terms the two institutional systems are simply very different, and they give rise to schools that reflect these differences—providing different services in different ways to please different constituencies.

Organization of Schools

Data and Method

High School and Beyond (HSB), first administered in 1980, is the most comprehensive survey of secondary schools to date. The original data base, pertaining to some 60 thousand students in more than one thousand public and private schools, provided a rich source of information about student achievement, attitudes, activities, and family background. This was the empirical foundation for Coleman, Hoffer, and Kilgore's (1982) *High School Achievement*, which set off shock waves in the educational community with its conclusion that private schools are academically more effective than public schools.⁵

High School and Beyond included certain information about the schools, but important aspects of organization and environment were not part of the study. To augment the data base, we helped design the Administrator and Teacher Survey (ATS), which went back to about five hundred HSB schools and administered questionnaires to the principal, a sample of 30 teachers, and selected staff members in each. Their responses tell us a good deal more about the schools as organizations—about their external relationships, their leadership, their structure and goals, their patterns of influence and interaction, and their educational practices. We put the ATS data to use in exploring how education is organized in the public and private sectors.⁶

Because private schools are so diverse, empirical work on student achievement has frequently clarified sectoral comparisons by distinguishing two relatively homogeneous types of private schools in the HSB sample—Catholic and elite—from all the rest. Catholic schools have played the central role in these analyses. They are the majority of private schools, and their students are very similar on socioeconomic and ethnic grounds to students in the public sector. The elite

schools are the handful of top private schools in the nation as judged by the proportion of seniors who were semifinalists in the National Merit Scholarship competition. The remaining schools, "other private," vary from tiny religious groupings to large college prep schools.⁷ We will maintain these distinctions—and despite their marked diversity, we will expect a uniformity across the three types. For by virtue of their shared institutional context, they should give evidence of something that approaches a common syndrome of organization.

In the analysis, we simply regress each organizational or environmental characteristic against dummy variables representing the three types of private schools.⁸ Specifically, if C is the characteristic in question, we estimate the following equation:

$$\begin{aligned} C = & B_0 + B_1 \text{ Catholic} \\ & + B_2 \text{ other private} + B_3 \text{ elite} \\ & + \text{error.} \end{aligned}$$

The constant, B_0 , measures the public school mean on C . *Catholic*, *other private*, and *elite* are dummy variables taking on the value 1 if the school is of that type, 0 otherwise. B_1 measures the difference between the Catholic school mean and the public school mean on characteristic C . B_2 and B_3 measure the same private-public comparison for the other private and elite categories.

These comparisons are made without the usual laundry list of statistical controls. It is an easy matter to include controls for school size, student background, and countless other factors, but we think it would be inappropriate and possibly very misleading to do so at this point. In estimating relationships among variables, the purpose of statistical controls is to remove covariation due to prior or exogenous influences. An institutional perspective on the organization of schools, how-

ever, suggests that all major variables are probably endogenous.

It may be, for instance, that private schools are more likely to exhibit certain characteristics—happy teachers, perhaps—because they tend to be smaller organizations than public schools. But it is no accident that private schools tend to be smaller, since small size is a major basis on which they appeal to students, parents, and teachers. Small size and happy teachers are integral parts of the same syndrome. Similarly, private schools may seem to have desirable organizational traits—more orderly climates, say—because their students come from families that care more about education. But these families have chosen their schools in the first place precisely because they like the way the schools are organized. So what causes what? Do motivated students make for a good school, or do good schools attract motivated students? The most reasonable view is that causality flows both ways and thus that both variables are endogenous.

Analogous arguments apply for virtually any variables of interest. To control for them as though they are exogenous is to remove from the sectoral comparisons—and to remove in a methodologically inappropriate way (via additive terms in recursive equations)—factors that are integrally woven into the very fabric of each system. For now, prudence argues for getting a clear look at how basic aspects of environment and organization differ across sectors. Investigation of the causal structure—and with it, informed thinking about statistical controls—can proceed most usefully once this sort of foundation has been laid.

The Findings

External Authorities

If the operation of politics and markets suggests anything, it is that the control

of schools should differ systematically across sectors. Public schools should find themselves operating in larger, more complex governing systems that tend to exert greater influence over school policy, educational practice, and personnel decisions. Private schools should tend to enjoy more autonomy with respect to their structure, goals, and operation. Of course, these tendencies are well documented when they derive from higher levels of government authority: public schools are part of state and federal hierarchies, integrated financially and programmatically, while private schools generally are not (Coleman, Hoffer, and Kilgore 1982). But what about immediate outside authorities? What kind of governing system operates at the local level to distinguish the sectors?

Table 1 presents summary figures on the extent to which the various schools are hierarchically subordinate to school boards or to outside administrative superiors (in the form of a superintendent or central office of some kind). Not surprisingly, virtually all public schools in the sample are governed by both; only two of nearly three hundred schools depart from this pattern. The private sector is far more diverse. Almost all private schools, regardless of type, have a school board of some sort, but often there is no accompanying administrative apparatus. Such an apparatus is quite rare among the elite schools, and nearly half of the "other privates" are similarly unencumbered. It is the Catholic schools that most resemble the publics in this regard, with some two-thirds of Catholic schools having both school boards and administrative superiors; even here, however, there is a good deal of hierarchic diversity by comparison to the public sector. Fully a quarter of the Catholic schools are overseen only by a school board, the Church's reputation for hierarchy notwithstanding.

Because administrative authorities are often absent from the environments of

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Table 1. Types of Outside Authorities (%)

Outside Authorities Present	Public ^b	Catholic ^c	Other Private	Elite Private
School board and administrators ^a	99.3 (287)	69.0 (20)	52.9 (9)	28.6 (2)
School board only	.0 (0)	24.1 (7)	41.2 (7)	57.1 (4)
Administrators only	.3 (1)	3.4 (1)	.0 (0)	.0 (0)
None	.3 (1)	3.4 (1)	5.9 (1)	14.3 (1)
Total	99.9 (289)	99.9 (29)	100 (17)	100 (7)

Note: Numbers in parentheses are the unweighted number of schools in each sector.

^aAdministrators include superintendent or central office.

^bExcludes several special public schools oversampled in ATS.

^cExcludes exclusively black Catholic schools oversampled in ATS.

private schools, many of these schools will operate relatively free from bureaucratic control. But what about the control exercised by the political and administrative authorities that public and private schools often have in common? In Table 2 control by school boards as perceived by principals is compared along five basic policy dimensions: curriculum, instructional methods, discipline, hiring, and firing. The results are striking in their consistency. On all five dimensions school boards in the public sector appear to have more influence over school policy than

they do in the private sector, regardless of the type of private school. The differences between public and private are consistently greater (and statistically significant) for personnel and disciplinary policy than for matters pertaining to educational practice and content.⁹ They amount on the average to between one and two points on a six-point influence scale. But in view of the uniformity of the overall pattern, there is reason to suspect that even the small estimated differences for curriculum and instruction are indicative of a greater role by school boards in the public sector

Table 2. The Influence of School Boards on School Policies

Areas of Influence	Catholic	Other Private	Elite Private
School board			
Curriculum	-.27 (1.33)	-.24 (1.52)	-.07 (.04)
Instruction	-.03 (.14)	-.003 (.02)	-.03 (.01)
Discipline	-.80 (4.02)	-.35 (2.24)	-1.16 (.56)
Hiring	-.98 (5.16)	-.80 (5.31)	-1.11 (.56)
Firing	-.76 (3.83)	-.41 (2.63)	-.86 (.42)
School board vs. principal			
Curriculum	-.40 (1.96)	-.26 (1.63)	.05 (.03)
Instruction	-.06 (.31)	.41 (2.50)	.17 (.08)
Discipline	-.72 (3.59)	-.28 (1.75)	-.92 (.44)
Hiring	-.97 (4.89)	-.42 (2.67)	-1.30 (.63)
Firing	-1.08 (5.65)	-.72 (4.72)	-1.23 (.62)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

Table 3. The Influence of Administration on School Policies

Areas of Influence	Catholic
Administration	
Curriculum	-1.51 (8.19)
Instruction	-1.01 (5.12)
Discipline	-1.23 (6.39)
Hiring	-1.82 (10.28)
Firing	-2.11 (12.65)
Administration vs. principal	
Curriculum	-1.57 (8.44)
Instruction	-1.23 (6.33)
Discipline	-1.11 (5.66)
Hiring	-1.56 (8.39)
Firing	-2.00 (11.57)

Note: Administration includes superintendent or central office. Table reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

generally. This is reinforced by the perceived influence of school boards relative to principals. As outlined in the lower half of Table 2, the sectoral differences are repeated and sometimes amplified. Relative to their school boards, private principals play a more autonomous role in setting and implementing policy—especially as it pertains to personnel and discipline—than public principals do.

Relative to their administrative superiors, private principals appear to be similarly autonomous. At least this is true of principals in Catholic schools, the only type of private school with enough administrative supervision to make a comparison with public schools valid and instructive. The figures, presented in Table 3, suggest an interesting conclusion: that the famed Catholic hierarchy (although see Greeley 1977) plays a comparatively small role in governing Catholic schools. On all five dimensions, the influence of administrative superiors is far less in Catholic than in public schools. These differences are again greatest in the area of

personnel policy, but here the other policy areas reflect substantial differences as well. When we explore the school's autonomy a bit further by comparing the principal's influence to that of administrative superiors, the same pattern emerges. Relative to administrators, Catholic principals enjoy more freedom than public principals in setting school policy.

To be sure, the differences in school autonomy that seem to distinguish the public and private sectors are based on simple measures of perceived influence and not on actual behavior. But the patterns these measures yield are quite uniform and entirely consistent with our expectations for external control. The authorities that are so ubiquitous in the democratic context of the public school are often simply absent from private school settings—and even when they are an acknowledged part of the private governing apparatus, they play less influential roles in the actual determination of school policy. Private schools, it would appear, have more control over their own destinies.

External Constraints: Choosing the Organization's Staff

Among the controls that any organization seeks to exercise over its operations, perhaps none is as important as control over its staff—in the case of a school, its teachers. To what extent does the school have flexibility in recruiting the kinds of teachers it wants and getting rid of those who do not live up to its standards? We have already seen that public schools are at a disadvantage in this regard, for external authorities have much more influence over hiring and firing in the public sector than they do in the private sector. The sectoral differences are not limited to the role of external authorities, however. They become still more dramatic when we

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consider two additional constraints on the choice of personnel: tenure and unions.

Tenure systems in public schools are special cases of the civil service systems that exist at all levels of government. Historically, these systems arose to prevent politicians from rewarding their supporters with public jobs. Reformers recognized that the widespread use of patronage was inconsistent with the kind of expertise, professionalism, and continuity so necessary to effective government, and—in a halting process that took decades to accomplish—they brought about the pervasive adoption of civil service systems built around objective qualifications and designed to protect employees judged to be qualified. Tenure is one of these protections (Peterson 1985).

Teacher unions (or “associations”), although initially resisted by politicians wedded to patronage, eventually found political allies of their own. Organized teachers could offer money, manpower, and votes to politicians. In state and local elections, where turnout is typically very low, these are attractive inducements indeed. As teacher unions thrived, they gained not only economic concessions but also contractual guarantees of job security and other limitations on responsibilities that reinforced the protections of the civil service system and introduced wholly new constraints into personnel decisions affecting the local school (Grimshaw 1979).

Although there is nothing to prevent unions from gaining a foothold in private schools nor to keep private schools from adopting tenure and other civil service-like protections, there is nothing comparable to government that drives them in that direction. Whether unions and tenure systems take hold in the private sector is determined much less by politics and much more by markets. Schools may choose to offer tenure and other protections as a means of attracting good teachers, particularly given that public schools

offer that benefit. But private schools may also decide, especially if the supply of teachers is high, that they can offer a very attractive set of benefits—such as good students, orderly atmosphere, and collegial decision making—without offering tenure at all. Similarly, as in any market setting, unions may or may not succeed in organizing teachers. But they cannot count on symbiotic relationships with the authorities, as public unions can, to help their cause.

The ATS data suggest that the public and private sectors are in fact enormously different in these respects. While 88% of public schools offer tenure, only a minority of the private schools do: 24% of the Catholics, 39% of the elites, and 17% of the “other privates.” Among the schools that do offer tenure, moreover, the proportion of teachers who have actually been awarded it reflects the same asymmetry: 80% of the eligibles in public schools have tenure, while the figure is some 10%–16% lower in the private sector. The differences in unionization are even more substantial. The vast majority of public schools are unionized—some 80%—almost all of them by either the National Education Association or the American Federation of Teachers. In the private sector, by contrast, teachers are rarely represented by unions. Only about 10% of the Catholic schools are unionized, and virtually none of the elites and “other privates” are.

To assess whether school control over personnel is perceptibly constrained by tenure, unions, and other proximate external authorities, we asked principals to evaluate an assortment of potential barriers to hiring excellent teachers and firing incompetent ones. On the hiring side, principals in the two sectors agreed on the severity of several obstacles, including applicant shortages and low pay. But public school principals were far more likely to complain about obstacles administrative in origin: “central office con-

Table 4. School Personnel Policy and Process

Personnel Constraints	Catholic	Other Private	Elite Private
Barriers to Hiring			
Too many transfers	-.57 (2.72)	-.56 (3.36)	-.43 (.20)
Central office control	-.51 (2.43)	-.25 (1.51)	-.47 (.22)
Barriers to Firing			
Complex procedures	-.59 (3.35)	-1.47 (10.51)	-1.26 (.69)
Tenure rules	-.93 (5.08)	-1.36 (9.39)	-1.55 (.82)
Hours involved in firing someone	-.85 (4.01)	-.75 (4.47)	-.90 (.41)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

trol" and "excessive transfers from other schools" (see Table 4).

The obstacles to dismissing teachers for poor performance differ similarly. In the public schools the procedures are far more complex, the tenure rules more constraining, and the preparation and documentation process roughly three times as long (Table 4). The complexity and formality of dismissal procedures is the highest barrier to firing cited by public school principals. For private school principals, of every type, the highest barrier is "a personal reluctance to fire." These responses provide a rather poignant statement of the differences between the sectors: while the public school principal is bound most by red tape, the private school principal is bound most by his or her conscience.

Principals do, of course, have other forms of control over their staffs. They can encourage undesirable staff to resign, retire, or transfer. They can offer good teachers special assignments or relieve them of onerous duties. They can recognize high performance with awards.¹⁰ But none of these practices differs systematically across the sectors. Public principals simply have less power than private principals to mold and manage their teaching staffs.

Even if public superintendents or central offices wanted to delegate such power

to the school—and, in general, there is no reason to think they have incentives to do this—many personnel decisions cannot in practice be delegated. Tenure protections are usually guaranteed through laws that are written by school boards or state legislatures, and these laws are then enforced by administrators. Union contracts are typically bargained at the district level, not at the school level, and are enforced from above. Tenure and unionization tend to settle the question of where and how the basic personnel decisions will be made in the public sector. They will be centralized. Schools in the private sector, largely free of such constraints, have far greater flexibility to choose their own members and chart their own paths.

Parents

In most respects, private schools would seem to have ideal parental environments. Parents, after all, have made a positive choice to send their children to a private school, presumably because they care about education and have a high appraisal of the school. And if at any time they change their views, they can simply exercise their exit option. This means that the school is likely to enjoy significant gains: they gain children whose family lives encourage education, and parents

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Table 5. Parental Relationships with Schools

Parental Role	Catholic	Other Private	Elite Private
Monitoring students	.90 (4.69)	.43 (2.82)	1.30 (.65)
Expectations of students	1.24 (6.85)	.91 (6.33)	2.62 (1.39)
Involvement in school	.74 (3.81)	.52 (3.39)	.64 (.32)
Cooperativeness	.43 (2.13)	.18 (1.14)	.47 (.22)
Freedom from constraint	.57 (2.81)	.22 (1.35)	.49 (.23)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

who not only will facilitate school objectives by monitoring homework and the like but will be informed and supportive when they take an active interest in school decision making. Parents who may cause problems on these scores are precisely the ones most likely to drop out of the school's environment voluntarily.

Public schools are not so fortunate. Many of their students come from families that put little or no emphasis on education; the students come to school with poor attitudes and orientations, and the parents do little to facilitate the school's efforts. Because exit is often not a viable option, many parents who do not support the school's goals, methods, or activities will remain in its environment nevertheless; and some—perhaps many—will use the democratic mechanisms at their disposal, as well as interactions with principal and staff, to express their dissent and press for change. Far from gaining sustenance from a supportive parental environment, the public school may often find itself dealing as best it can with conflict, disappointment, and apathy.

Not all public and private schools will neatly fit these molds, of course. But it seems clear that characteristics inherent in the two sectors—characteristics anchored in politics and markets—encourage the kinds of environmental differences outlined here. And results from the ATS study, detailed in Table 5, are consistent with this line of reasoning. Parents in the private sector, regardless of the type of

less constrained by the kinds of formal rules and norms that due to democratic governing structures impinge on the flexibility of public school principals.

school, are uniformly more supportive of their schools. They have higher expectations about their children's educational performance, they are more active in monitoring their children's behavior outside of school, and they are more deeply involved with the school as an organization. Private school principals, not surprisingly, also express greater satisfaction with their parental environments. Relationships with parents are more cooperative than in the public sector, and they are

The operation of politics and markets, then, appears to put public schools at a real disadvantage. Because of forces largely beyond the control of the individual school, parents in the public sector tend to be less supportive of the school's general educational efforts and more likely to promote organizational conflict—and, to make matters worse, the school has less flexibility in seeking solutions to these problems. By comparison, private schools have fewer such problems and yet more flexibility for dealing with them.

Between Environment and Organization: The Principal

The principal operates at the boundary of the organization and is, more than any other single person, responsible for negotiating successfully with the environment

Table 6. Characteristics of School Principals

Characteristics	Catholic	Other Private	Elite Private
Teaching experience	.43 (2.21)	.56 (3.59)	.58 (.28)
Motivations			
Policy control	.61 (3.04)	.46 (2.92)	.31 (.15)
Preference for administrative duties	-.33 (1.63)	-.49 (3.08)	-.14 (.07)
Career advancement	-.69 (3.52)	-.61 (3.91)	-.48 (.23)
Desire further advancement	-.76 (3.95)	-.71 (4.69)	-1.04 (.52)
Leadership as perceived by teachers	.41 (2.12)	.74 (4.84)	.64 (.32)
Instructional leadership	.66 (3.67)	1.28 (8.98)	.82 (.44)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

—responding to demands and pressures from parents, unions, administrators, and school boards, and dealing with external disruptions such as budget cuts, policy conflicts, and demographic changes. The principal may also hold a key to school effectiveness. Evidence increasingly suggests that educational excellence is promoted by a principal who articulates clear goals, holds high expectations of students and teachers, exercises strong instructional leadership, steers clear of administrative burdens, and effectively extracts resources from the environment (e.g., Blumberg and Greenfield 1980; Brookover et al. 1979; Goodlad 1984).

It is seldom stressed, however, that the school environment can have a lot to say about whether the principal is able to practice these precepts of effective leadership—or, for that matter, is even motivated to practice them. Effective leadership does not simply inhere in the individual filling the role; it is unavoidably contingent upon the demands, constraints, and resources that the principal must deal with. Depending on the nature and strength of these forces, even the “best” principal may have only a marginal effect on school performance. We must also remember that principals do not possess or lack leadership qualities by accident. Both environment and organization tend to ensure that there will be selec-

tive attraction to the job: certain schools will tend to attract certain kinds of principals. Similarly, principals will be socialized on the job, and internal and external factors will ensure that principals at distinctly different schools will be socialized differently. Thus, while it is one thing to point to certain qualities that appear conducive to effective leadership, it is quite another to suggest that principals are free to develop them.

It should not be surprising, then, to find differences between public and private school principals, both in terms of their own characteristics and in terms of their performance. Consider first what the ATS data (see Table 6) have to say about how they came to their jobs. Private school principals have quite a bit more teaching experience than their public counterparts—the gap is almost four years for principals in Catholic schools, and over five years for those in the elites and “other privates.”¹¹ This is consistent with the hierarchic organization of public sector education: its career ladder offers teachers early opportunities for moving into a host of subordinate administrative positions (such as assistant principalships), followed by subsequent opportunities for moving up in status and salary. As this implies, principals also come to their jobs with different motivations. Private principals are more likely to stress “control

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over school policies," while public principals place greater emphasis on "preference for administrative responsibilities," a "desire to further [their] career[s]," and an interest in advancing "to a higher administrative post."

The typical career orientations of principals in the two sectors thus appear to be quite different. Public principals tend to disembark from teaching relatively early, get on an administrative track, and take the job of principal to keep the train rolling. Private principals are scarcely on a track at all. They stay in teaching longer, and their view of the principalship focuses more on its relation to the school than on its relation to their movement up the educational hierarchy.

How the principal performs on the job is a function of many things, not just the values and experiences noted here. They would, however, appear to have a direct bearing on one aspect of performance consistently singled out in the effective schools literature: instructional leadership. Teachers in the ATS study were asked questions about the quality of the assistance they received in regard to instructional problems, and their responses indicate strong differences across the sectors. As judged by their own teachers, private principals are more effective in this important area of leadership than public principals are. Again, this may be due to a variety of factors. But the simple fact that the public principal has far less teaching experience (which itself has roots in his or her distinctive career orientation) is in itself likely to affect rapport with teachers, self-perception of instructional role, and other aspects of the job as they pertain to teaching. It is not surprising to find that instructional leadership is more effective in private schools.

Finally, the ATS teachers were asked to evaluate their principal with regard to a range of leadership-related qualities bearing on knowledge of school problems, communication with the staff, clarity and

strength of purpose, and willingness to innovate. Constructing a general index of leadership from these items, we find that by these criteria teachers rate private principals to be better all-around leaders than public principals. This result is more likely to reflect the operation of general environmental conditions than the earlier one on instructional leadership. Principals in the public sector are forced to operate in much more complex, conflictual circumstances in which educational success is more difficult to achieve regardless of the principal's (perhaps considerable) abilities and qualifications. If anything, however, it is plausible to suggest that the public principal's lack of teaching experience and a hierarchic career orientation probably contribute to these leadership problems.

While these findings only begin to scratch the surface, it does appear that public and private school principals are quite different in important respects. They have different backgrounds, different career orientations, and—whatever the true constraints on their performance might be—they are evaluated differently by their teachers: principals in private schools are more highly regarded as leaders.

The Organization: Goals and Policies

Given what we know of their environments, there is every reason to expect that public and private schools should adopt very different orientations toward the education of their students. Because public schools must take whoever walks in the door, they do not have the luxury of being able to select the kind of students best suited to organizational goals and structure—it is the latter that must do virtually all the adapting if a harmonious fit is to be achieved. In practice, this means that the pursuit of educational excellence must compete with much more basic needs—for literacy, for remedial training,

Table 7. School Structure

Characteristics	Catholic	Other Private	Elite Private
Goals			
Basic literacy	-1.59 (8.83)	-.83 (5.81)	-1.10 (.59)
Citizenship	-1.12 (6.06)	-1.04 (7.11)	-.96 (.50)
Good work habits	-.92 (4.68)	-.52 (3.37)	-.26 (.13)
Occupational skills	-.89 (4.60)	-.77 (5.01)	-.98 (.49)
Academic excellence	.10 (.48)	.41 (2.57)	.94 (.45)
Personal growth	.47 (2.33)	.12 (.78)	.69 (.33)
Human relations	.24 (1.19)	.11 (.71)	.34 (.16)
General graduation requirements			
English and history	.61 (3.09)	.51 (3.26)	.57 (.28)
Science and mathematics	.34 (1.73)	.88 (5.77)	1.78 (.88)
Foreign language	1.28 (7.88)	1.61 (12.47)	3.33 (1.96)
School-wide homework policy	.13 (.65)	.48 (3.06)	.90 (.44)
Goal clarity	.64 (3.32)	.80 (5.24)	.80 (.40)
Goal disagreement	-.35 (1.80)	-.85 (5.59)	-.55 (.27)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

for more slowly paced instruction. In addition, there is the hierarchic structure of democratic control to ensure that a range of actors and diverse, often-conflicting interests are brought to bear in decisions about what the public school ought to be pursuing and how. As in other areas of politics, the thrust is toward compromises and "solutions" (see also Powell, Farrar, and Cohen 1985) that reflect the lowest common denominator—and often a great deal of ambiguity and internal inconsistency as well. This is to be expected when an important function of the decision-making process is conflict resolution. The process is unavoidably a political exercise, not an analytical attempt at problem solving.

Private schools are largely unconstrained in comparison, both in the selection of students and in the determination of organizational goals. It is only reasonable to suggest that a given private school is likely to have clearer and more homogeneous goals than a given public school. Aggregate comparisons, however, are more uncertain because the private sector

is comprised of so many different types of schools; an elite school will emphasize academic excellence, but a religious or military school may have quite different priorities—although we would still expect them to be relatively clear and homogeneous compared to the publics.

Despite such uncertainties, the comparisons across sectors are quite uniform (see Table 7). In terms of general goals, public schools place significantly greater emphasis on basic literacy, citizenship, good work habits, and specific occupational skills, while private schools—regardless of type—are more oriented by academic excellence, and personal growth. For the most part, these sorts of differences are what we should expect in view of the more fundamental differences in student bodies and governing structures. Most obviously, public schools would ordinarily find it politically and organizationally very difficult to place high priority on academic excellence.

Whether these goals become reflected in school structure and performance depends on whether they are upheld by

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specific policies and clearly discerned by the staff. As Table 7 suggests, there are definite differences across sectors in these respects. To begin with, the private schools have more stringent minimum graduation requirements; their students, regardless of track, must take significantly more English and history, science and math, and foreign language than must public students in order to graduate. In science, math, and foreign language the differences range up to two years.

Private schools also have stricter homework policies. This is particularly true of the elites and "other privates," most of which establish schoolwide daily minimums per subject, strongly encourage homework, or, in cases where faculty are overzealous, set daily maximums per subject. In contrast, 90% of all public schools leave the amount of homework entirely up to teachers. Catholic schools fall in between these extremes.

These differences in goals and policies are accompanied by differences in their clarity and their acceptance by organization members—key factors in their translation into organizational action. Private teachers uniformly report school goals as clearer—and more clearly communicated by the principal—than public teachers report. In addition, there is less disagreement among the school priorities reported by teachers in private schools. In general, private schools tend to possess a clarity and homogeneity of educational purpose that does set them apart from public schools, at least on average. They place more emphasis on academic excellence, have stricter graduation requirements, and have tougher homework policies. And their staff members have clearer, more consistent conceptions of what their organizations are supposed to be achieving. These are, of course, stereotypical characteristics of "effective schools." They are also characteristics that, due to the differential operation of politics and markets, would seem extremely difficult for

public schools to develop in the same degree.

The Organization: People, Decisions, Operation

What should public and private schools look like on the inside? What might we expect in general about their structures, processes, and personnel? A widely accepted notion in organization theory is that environmental complexity is reflected in organizational complexity (Lawrence and Lorsch 1967). For rather obvious reasons, then, public schools should prove far more complex than their private counterparts; and existing studies indicate that this is actually the case (Scott and Meyer 1984). Moreover, their very complexity of structure and heterogeneity of goals suggest that public schools may often be "loosely coupled," characterized by relatively autonomous centers of activity and decision making. Private schools, on the other hand, would seem to approximate classical notions of organization. They have simpler, more stable, less threatening environments, and goals that are fewer in number, clearer, and more narrowly based—characteristics that facilitate the centralized direction of goal pursuit. This would seem to be consistent with the private schools' reputation for rigid curricula, traditional instructional methods, and strong principals.

A politics-and-markets perspective cannot hope to tell us everything we might want to know about organizational structure and process, but it does tend to point us in a different direction. The critical fact about the public school environment is not just that it is complex but that it literally imposes decisions about policy, structure, personnel, and procedure on the school. Thus, while the school may well adapt to environmental complexity by developing an internal complexity of its own, its range of choice is severely constrained—for a great many potential adaptive adjustments are simply ruled out

by environmental fiat. Conversely, the private school is not only blessed with a relatively simple environment but with a much broader range of organizational options in adapting to it.

Consider, in particular, the most crucial agent of organizational performance: the teacher. As we have seen, the public school principal is far less able than the private school principal to staff the organization according to his or her best judgment. The public principal may value expertise, enthusiasm, collegiality, communication skills, creativity, or any number of qualifications related to the school's goals but simply has less power to obtain teachers who possess them or eliminate ones who do not. This should tend to promote staff heterogeneity and conflict. Teachers may reject the principal's leadership, dissent from school goals and policies, get along poorly with their colleagues, or fail to perform acceptably in the classroom—but the principal must somehow learn to live with them. When these teachers are represented by unions, as they normally are, leadership difficulties are magnified and an important wedge is driven between the principal and the staff, a wedge that promotes formalized decision procedures, struggles for power, and jealousies over turf. "Professionalism" takes on new meaning—as a justification for placing decision power in the hands of teachers rather than the principal.

Private schools are not immune from personnel problems and struggles for power. But the principal, having much greater control over hiring and firing, can take steps to recruit the kinds of teachers he or she wants and weed out the rest. It also means that teachers have a strong inducement to live up to the principal's criteria on a continuing basis. By comparison to the public school counterpart, then, the private school principal is in a position to create a "team" of teachers whose values, skills, and willingness to

work together tend to mirror those qualifications the principal deems conducive to the pursuit of organizational goals. At the same time, the principal is in a position to make teacher professionalism work for, rather than against, him or her. Without real threat to his or her own authority or control, the principal can encourage teacher participation in decision making, extend teachers substantial autonomy within their own spheres of expertise, and promote a context of interaction, exchange of ideas, and mutual respect.

The data from the ATS study seem to provide strong support for this general line of reasoning. As outlined in Table 8, principals and teachers simply have higher opinions of one another in the private sector. Private principals consistently claim that a larger percentage of their schools' teachers are "excellent," suggesting that they are more confident in the abilities of their own staff members than public school principals are. Private sector teachers, in turn, have better relationships with their principals. They are consistently more likely to regard the latter as encouraging, supportive, and reinforcing; and, as we saw earlier, they have higher regard for their principals as effective organizational leaders.

Private school teachers also feel more involved and efficacious in important areas of school decision making that bear on their teaching. In particular, they feel more influential over schoolwide policies governing the curriculum, student behavior, teacher in-service programs, and the grouping of students of differing abilities. Regarding issues of special relevance to the classroom, they believe they have more control over text selection, course content, teaching techniques, disciplining students, and, in the Catholic schools, determining the amount of homework to be assigned. (The non-Catholic private teachers feel constrained by the schoolwide homework policies identified earlier.) Even on matters of hiring and firing,

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Table 8. Staff Relations

Characteristics	Catholic	Other Private	Elite Private
Percent excellent teachers	.40 (2.07)	.78 (5.05)	1.16 (.58)
Principal-teacher relations	.44 (2.29)	.90 (5.93)	.96 (.48)
Teacher influence and control			
Student behavior codes	.95 (5.15)	1.00 (6.84)	1.04 (.54)
In-service programs	.32 (1.64)	.77 (5.05)	.41 (.20)
Ability groupings	1.24 (7.20)	1.32 (9.69)	1.37 (.76)
Curriculum	1.01 (5.66)	1.22 (8.59)	1.22 (.66)
Text selection	.66 (3.46)	.78 (5.11)	.74 (.37)
Topics taught	.50 (2.62)	.94 (6.23)	.53 (.27)
Techniques	.70 (3.58)	.40 (2.61)	1.06 (.52)
Discipline	1.34 (7.17)	.46 (3.12)	.83 (.42)
Homework	.63 (3.19)	.33 (2.14)	-1.16 (.57)
Hiring	.54 (2.63)	.38 (2.38)	.93 (.44)
Firing	.55 (2.75)	.16 (1.04)	.21 (.10)
Teacher-teacher relations			
Curriculum coordination	.60 (3.11)	.67 (4.37)	.96 (.48)
Teaching improvement	.60 (3.21)	1.06 (7.21)	1.03 (.53)
Collegiality	.90 (5.34)	1.58 (11.82)	.82 (.47)
Success not beyond personal control	.79 (4.19)	.54 (3.59)	1.23 (.62)
Doing best not waste of time	1.12 (5.39)	.96 (5.85)	1.78 (.82)
Job satisfaction	.57 (3.37)	.54 (4.06)	1.01 (.58)
Teacher absenteeism	.58 (3.00)	.73 (4.74)	.49 (.24)
Lowest teacher salary	-.75 (3.92)	-.86 (5.72)	.39 (.19)
Highest teacher salary	-.76 (3.87)	-.57 (3.65)	.91 (.45)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

private teachers believe they are more influential—this, despite the almost complete absence of unions in their sector.

Relative harmony between private principals and private teachers is matched by relative harmony among the private teachers themselves. On a personal level, relationships are more collegial in the private sector. Stated in the plain terms of the survey, private teachers are more likely to believe that they "can count on most staff members to help out anywhere, anytime—even though it may not be part of their official assignment" and ultimately that "the school seems like a big family." On a professional level, private teachers give greater evidence of mutual involvement and support. They are more likely

to know what their colleagues are teaching, to coordinate the content of their courses, and to observe one another's classes. They also spend more time meeting together for the purpose of discussing curriculum and students.

It is no surprise, then, that private school teachers also feel more efficacious than public school teachers. Unlike their public counterparts, they do not believe their success is beyond their control, and they do not feel it is a waste of time to do their best. Overall, private school teachers are much more satisfied with their jobs. It is no wonder, then, that private school teachers have better attendance records nor that they tend to work for less money. Private school teachers are trad-

ing economic compensation and formal job security for superior working conditions, professional autonomy, and personal fulfillment. Public school teachers are doing precisely the opposite.

In short, private schools do tend to look more like "teams." By their own account, teachers have better relationships with principals, are more integrally involved in decision making, interact more frequently and productively with their colleagues, and feel more positively about their jobs and their organization. According to their principals, they are higher-quality teachers as well. As professionals, it appears they are given much greater reign in a private setting, which gives them opportunities to put their ideas and skills to use through a level of active involvement and a sharing of power that teachers in the public sector generally cannot expect. The key to explanation is anchored in a more fundamental feature of the sectors: private leaders have the freedom to choose their own professionals, public leaders do not.

Conclusion

The Administrator and Teacher Survey provides the first opportunity to document public-private differences by means of a large, representative sample of schools, and its findings dovetail nicely with major lines of argument in the education literature. If it is true, as Coleman, Hoffer, and Kilgore (1982) and Coleman and Hoffer (1987) have claimed, that private schools outperform public schools on academic grounds, and if the effective schools research is basically correct in the characteristics it tends to associate with effectiveness, then we should find that private schools disproportionately possess these characteristics.

That is just what we find. Private schools have simpler, less constraining environments of administrators, school

boards, and parents. They are more autonomous and strongly led. They have clearer goals and stricter requirements, and they put greater stress on academic excellence. Relations between principals and teachers and among teachers themselves are more harmonious, interactive, and focused on teaching. Teachers are more involved in policy decisions, have greater control over their work, and are more satisfied with their jobs.

We have tried to do more here than present findings and relate them to existing work, however. We have tried to develop an institutional perspective that suggests why schools should be expected to differ across the sectors. This perspective arises from our belief that institutional context has pervasive consequences for the organization and operation of all schools, consequences more far-reaching than most of the literature tends to suggest (Pfeffer and Salancik 1979).

Public schools are products of our democratic institutions. They are subordinates in a hierarchic system of control in which diverse constituency groups and public officials impose policies on local schools. It is no accident that public schools are lacking in autonomy, that principals have difficulty leading, and that school goals are heterogeneous, unclear, and undemanding. Nor is it an accident that weak principals and tenured, unionized teachers struggle for power. These sorts of characteristics constitute an organizational syndrome whose roots are deeply anchored in democratic control as we have come to know it.

Private schools are controlled by society too, but not through politics or bureaucracy. They make their own decisions about policy, organization, and personnel subject to market forces that signal how they can best pursue their own interests. Given their substantial autonomy—and given the incentives for autonomy that are built into the system—it is not surprising to find that principals are

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stronger leaders; that principals have greater control over hiring and firing; that principals and the teachers they choose have greater respect for, and interaction with, one another; and that teachers—without conflict or formal requirement—are more integrally involved in policy-making. These sorts of characteristics are bound up with one another, and they jointly arise from the institutional environment. Different institutions promote different organizational syndromes.

If this is essentially correct, the standard proposals for reforming public schools are misconceived. It is easy to say, for instance, that schools should have greater autonomy or that principals should be stronger leaders. But these sorts of reforms are incompatible with the "one best system" and cannot succeed. Politicians and bureaucrats have little incentive to move forcefully in these directions. Their careers are tied to their own control over the schools, and they are unavoidably responsive to well-organized interests that have stakes in the system's capacity to impose higher-order values on the local schools. Restricting autonomy is what democratic control is all about.

It is also about power, about who gets to have how much say in the control of schools. Reformist notions that the various actors should work together in the best interest of the schools are doomed by the institutions of democratic control, which guarantee conflict of interest, struggle for advantage, and resort to formally enforced "cooperation." Reforms calling for even the simplest changes—testing veteran teachers for minimum competence, say—will normally fail if they threaten established interests. Their bearing on school effectiveness has little to do with their political feasibility.

Reformers must reckon with the possibility that the measures they support, particularly those arising from the effective schools research, are often inconsistent with our current framework of demo-

cratic control. The public schools cannot be anything we might want them to be. They must take organizational forms compatible with their surrounding institutional environments. It may well be, then, that the key to school improvement is not school reform, but institutional reform—a shift away from direct democratic control.

This does not mean that the public schools must be freed from all democratic governance. But it is instructive that the private schools, which are products of an institutional system that decentralizes power to the producers and immediate consumers of educational services, tend to develop precisely the sorts of organizational characteristics reformers want the public schools to have. Some sort of voucher system, combining broad democratic guidance with a radical decentralization of resources and choice, is at least a reasonable alternative to direct control—one that might transform the public schools into different, more effective organizations, while still leaving them truly public.

Even if this or some other alternative is someday shown to have compelling features, however, democracy probably cannot get us from here to there. Any proposal to shift away from prevailing institutions is so threatening to established interests that it stands little chance of political victory. Because a shift in institutional control may be the one reform that makes all the others possible, the uncomfortable reality may simply be that all the others are not possible.

Notes

An earlier version of this paper was presented at the 1987 annual meeting of the American Political Science Association, Chicago. The first draft appeared as the Brookings Institution's Governmental Studies Discussion Paper No. 1.

1. The responsiveness of residential decisions to differences in the quality of public services, includ-

ing education, has been extensively investigated and generally found to be quite imperfect (Rose-Ackerman 1983).

2. This is not to say that intelligent guidance from the center—e.g., about important innovations in curriculum and methods—is unimportant, only that education appears to benefit from a balance of control and autonomy favoring the teachers and the school (e.g., Carnegie Forum on Education and the Economy 1986).

3. On the logic of control and its implications for bureaucratic forms of organization, see, e.g., Williamson 1975. For a review with applications to politics, see Moe 1984. On the historical development of the educational bureaucracy, see Tyack 1974 and Peterson 1985.

4. There are some interesting complications that we cannot dwell on here without getting too far afield. Mayors, for instance, sometimes attempt to avoid the risks and pressures of educational politics by minimizing their own roles and shifting authority to others (e.g., members of the school board). More generally, politicians sometimes find it advantageous to create agencies that are insulated in some measure from political influence, including their own influence. These maneuvers, however, never put an end to politics or political control—and the maneuvers themselves are reversible through subsequent political maneuvers. Through it all, the authority to impose higher-order values is still there, and those with access to it still have incentives to use it to get what they want.

5. This study has been pummeled from all angles. See, e.g., Bryk 1981; Goldberger and Cain 1982; Guthrie and Zusman 1981; Heyns and Hilton 1982; Murnane 1981. It seems to us, however, that the thrust of the Coleman-Hoffer-Kilgore argument has largely withstood these attacks. In more recent research by Coleman and Hoffer (1987) it has also been strongly reinforced.

6. When the ATS data are merged with those from HSB and its follow-ups (the details of which are available from the National Center for Education Statistics), they offer a unique foundation for exploring the connections among environment, organization, and student achievement. That is the purpose of our larger project, of which this paper is a part.

7. Studies of organization, as opposed to student achievement, have dealt almost entirely with public schools. Most of what is known about the organization of private schools is derived from case studies or studies of limited samples (Cibulka, O'Brien, and Zewe 1982; Erickson 1982; Greeley 1966; Peshkin 1986; Sanders 1981). Systematic national surveys are rare (Abramowitz and Stackhouse 1980; Kraushaar 1972).

8. The procedures employed in constructing indexes and measures of the variables are detailed in an appendix available from the authors.

9. Throughout this paper coefficients will be called "statistically significant," or simply "significant," if they satisfy a two-tailed t-test at a probability level of .05. The results for the elite schools will not, however, be evaluated in this fashion. The elite schools are not a sample, but a population—the schools with the most National Merit semi-finalists in 1978. As such, it is arguably inappropriate to make statistical inferences from the "sample" to the population. In any case, the number of elite schools is too small, especially after weighting, to produce t-scores in the necessary range of 2.0.

10. Private schools may also offer merit pay; however, only the "other private" sector makes significantly greater use of it. Catholic schools do not differ from public schools in providing merit pay.

11. Recall that the coefficients reported in the tables are based on standardized measures of the dependent variables. When converted back to their original metric, the coefficients for teaching experience are equivalent to the years reported in the text.

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THE POLITICAL ECONOMY OF STATE MEDICAID POLICY

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We develop and test a market-based model to explain variations in states' welfare medicine policy decisions. The empirical results support the model of state policy outputs, indicating that states' spending efforts for welfare medicine are most sensitive to the supply of services within their borders. We learn in addition that spending effort declines with demand for services, indicating that the states spending the highest proportions of total personal income for the program are those who need it most and can afford it least. Measures of political system development affect spending effort positively and significantly, suggesting that ideology, diversity of interests, and administrative professionalism increase states' welfare efforts.

We construct a supply and demand model of state policy-making and test its applicability to welfare medicine policies in the states. Through this exercise we seek to contribute to the growing body of empirical political economy research, which incorporates economic and political theories in models of policy and politics. As stated elsewhere (see, e.g., Chubb 1985; Plotnick and Winters 1985), neither wholly political nor wholly economic explanations are adequate for some policy outputs. Better explanations are gained by blending elements of each approach.

Policy-making environments may be likened to markets in which demand and supply influence policy provision. Political mechanisms are designed to link citizen demands for government services to policies. Demand is expressed most directly when citizens or groups convey preferences about governmental actions to policymakers. Supply in policy mar-

kets is set by the development of service delivery systems, ability to pay, and by normative and technical constraints.

Embodied in the neoclassical theory of markets is the assumption that consumers share information equally; that assumption does not hold in policy markets (see Moe 1984). Some citizens and groups are better equipped than others to monitor or participate actively in the governmental process and express their demands more clearly than others. Hence there is an information problem, creating an asymmetrical market.

As issues become more complex and abstract, the size of the attentive public decreases and the role of experts from the private and public sectors increases. The public's ability to express demands clearly is hampered by its poor and at times poorly targeted attention to issues. Issues enter the agenda and receive some public attention, government may or may not act, issues' perceived importance may fade,

and the public's attention shifts to other concerns (Downs 1972). The lack of widespread public attention to policy problems prejudices the policy market toward organized interests that are better equipped than the disorganized public to effect change. Even where elective officials and bureaucrats work to protect the public's interests, broadly conceived, organization is crucial in determining who gets what from government (Olson 1982, 1985).

The supply of policy outputs is determined in part by wealth. Public spending generally increases with the wealth of jurisdictions (Aronson and Hilley 1986, 1-10), leading some writers to conclude that policy possibilities are determined by economics (Dye 1966; Lewis-Beck 1977). However, group conflict and general political development also increase with wealth (Olson 1982). Consequently, political and economic development are complementary (Godwin and Shepard 1976; Hanson 1984; Plotnick and Winters 1985). As the potential for budgetary expansion grows, demand for governmental action increases. Supply of and demand for policy are aligned closely.

The states' welfare medicine policy decisions nicely illustrate policy market arrangements. A state's medical care policies are determined partly by countervailing powers of providers (supply) and recipients (demand). In turn, policy may affect supply and demand. Characteristics of political, social, and economic environments define the bounds within which decisions may be made and constrain policy outputs. A model of policy determination may be expressed in terms of

$$\text{demand} = f(\text{supply, policy, constraints}) \quad (1)$$

$$\text{supply} = f(\text{demand, policy, constraints}) \quad (2)$$

$$\text{policy} = f(\text{supply, demand, constraints}) \quad (3)$$

Individual politicians and legislatures are biased toward spending (Downs 1957; Hayes 1981; McCormick and Tollison

1981). Wealth, political participation, and political competition constrain the scope of policy and mediate legislative spending decisions. Supply, demand, and policy are interrelated, each simultaneously influencing the shape of the others.

We seek to make three major points through this exercise. First, we specify and test a model of policy determination. Second, we demonstrate the mutual influence of the environment on policy and of policy on the environment, which cannot be determined properly absent the use of simultaneous estimation techniques. Third, by measuring state policy in a manner that assesses the relative effort they devote to Medicaid spending, we cast state policy differences in a more revealing light than that cast by liberal-conservative or wealthy-poor distinctions. Analysis of the determinants of the real financial costs of a policy borne by a state's citizens calls for construction of a model more complex than that presented in the academic literature on policy determination.

Following this introduction we describe Medicaid, the policy to be analyzed. In the third and fourth sections, respectively, we develop the theoretical and measurement models and present results of the empirical tests. The fifth section consists of a discussion of our empirical findings in light of the theoretical model. Conclusions are drawn in the final section.

A Medicaid Primer

Medicaid, a state-federal program, is the primary vehicle for providing the poor or near-poor access to health services in the states. It is a means-tested social insurance program in which the state acts as insurer of last resort, paying both private and public sector medical

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vendors directly for services delivered to beneficiaries. It is paid for by the states and the national government. The national government share, which ranges from 50% to 77% of total state spending, is determined by a formula based on states' per capita incomes.

States administer Medicaid within federal guidelines. Receipt of federal funds is contingent upon state compliance with federal standards regulating the type and amount of services paid for under state programs, states' eligibility determination policies, and state provider payment policies. State-federal relations under Medicaid are typically strained; each wishes to control the program to the greatest extent possible while minimizing its financial burden.

Medicaid is expensive. Total state-federal spending for service delivery was \$40.7 billion in 1986. Medicaid spending, like that for health care generally, is often characterized as hard to control (Grannemann and Pauly 1983; Holahan and Cohen 1986). The program was created in 1966 with the stated goal of ensuring the indigent populations' access to mainstream medical services. Because responsibility for public welfare programs traditionally fell to the states, they took the lead in developing programs and sought to guarantee access, showing little concern for program costs. Congress had little information about potential costs when legislation forming the program was passed, and spending was higher than early projections (Thompson 1981). During the late 1970s the national government sought to reduce spending and shifted its emphasis from guaranteeing access to controlling cost.

The Carter administration's most visible contribution to Medicaid reform lay in the creation of the Health Care Financing Administration (HCFA), lodged in the Department of Health and Human Services, in 1977. The major task of HCFA is to administer Medicare and Medicaid. Its

presence has strengthened the national government's ability to oversee state Medicaid bureaucracies. Congress passed legislation aimed at reducing Medicaid spending during the early 1980s. The major vehicle for effecting changes in Medicaid financing arrangements was the Omnibus Budget Reconciliation Act of 1981 (OBRA 1981), in which the national government reduced its rates of financial participation and increased states' flexibility to design reimbursement practices and to pursue other administrative reforms. Cost containment efforts initiated as a result of the act initially slowed the rate of spending increases dramatically. Efforts to reduce expenditures continue (Holahan and Cohen 1986, 13-16).

State administrators, responding to their own budgetary constraints as well as to changes wrought by OBRA 1981, began to seek efficiencies and savings more vigorously than before. Since total expenditures are a product of eligibility, utilization, and the cost of services, the obvious expenditure control strategies were to hold each component as low as possible (Davidson 1980; Hanson 1984). Thus the 1980s have been a period of tinkering and experimentation with Medicaid programs, some of which has reduced rates of spending increases (Holahan and Cohen 1986; Cromwell, Hurdle, and Schurman 1987; Miller n.d.). Despite lessened cost increases during this decade, spending remains high and consumes an increasing portion of state budgets, leading state and federal administrators to seek further cost control measures.

Controlling Medicaid spending is difficult. Some of the Medicaid spending problem is a function of program design: states set eligibility criteria and other program characteristics and wait to see how much it costs in a given year. If, for example, a state's economy falters and unemployment rises, the number of Medicaid eligibles increases, leading to greater provider billings for which the state is

legally liable. Thus total expenditures are often beyond legislative control. Consequently, the program cannot be budgeted in a traditional sense and represents a wild card in state and national budgets.

Part of the cost control problem is due to the nature of health care as a consumer good. The price elasticity of demand for a commodity, defined as the percentage change in quantity demanded resulting from a 1% change in price, provides guidance for modeling transactions. Health care services' price elasticity of demand is low. Consumers of health care purchase services under conditions of uncertainty and expect providers to deliver appropriate treatments (Arrow 1963). Providers are regulated by personal and professional norms; their expertise and ethics are thus highly valued and important for the continued functioning of the health care system. Health care markets, given providers' control of expertise and information, are asymmetrical, thereby violating one of the central assumptions of neo-classical economics. Medicaid eligibles in the vast majority of states pay nothing out-of-pocket for services received. Hence service recipients do not associate a monetary price with the receipt of Medicaid-paid health care. Vendors, realizing that charges do not deter eligibles' service utilization, may inflate their billings by delivering unnecessary services (Auster and Gordon 1974). Even where providers do not seek to increase billings, the threat of malpractice suits, abetted by the low price elasticity of demand, induces them to practice medicine defensively, which increases billings.

The major issue revolving around Medicaid during its formative years was that of service delivery. It was created to ensure eligibles' access to health care. Access has been attained (Long, Settle, and Stuart 1986) but at a high financial cost. The expenditure control issue is now the most pressing concern for states and the national government.

The Political Economy of Medicaid

Medicaid is unique among public welfare programs because its functioning depends on the cooperation and support of private practitioners. As a result, modeling Medicaid requires consideration of the peculiar economics of health care, which are confounded further when viewed within the confines of a policy market. We do not know how much medical care is demanded by an informed consumer. Most health care recipients have little understanding of what constitutes appropriate care and, especially where the only expense is time (and maybe a little indignity), are susceptible to receiving inappropriate treatments by sometimes unscrupulous, but usually just cautious, providers. Demand for care leads to its being supplied. Supply begets demand. Supply and demand are best estimated simultaneously.

Medicaid programs cannot function without the cooperation of providers, and efforts to regulate various sectors of the health care industry have proven difficult. Medicaid decisions must be framed in a manner in which they are acceptable to the provider community (Davidson 1982). Policymakers' perceptions of Medicaid and subsequent program design decisions are colored by their estimates of its costs to the state; cost consciousness influences other aspects of the program. Policy decisions concerning eligibility affect demand directly. Likewise, expansion of eligibility may increase the number of physicians willing to participate in Medicaid by broadening the program's recipient pool and thus removing some of the stigma associated with being a Medicaid recipient or a Medicaid provider. Policy, supply, and demand for services determine one another; they also share relationships with some political and socioeconomic constraints.

In this research we model variations in

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state Medicaid spending effort, demand for Medicaid services, and potential supply of services in a system of three simultaneous equations. Measures of supply, demand, and policy are endogenous to the system; measures of states' social, economic, and political characteristics are exogenous.

Measuring Supply, Demand, and Policy

The proportion of total state personal income spent on Medicaid vendor payments (minus federal financial contribution) during 1982 is used as a proxy for welfare effort. Expenditure-based policy indicators are open to criticism on both conceptual and empirical grounds. Hanson (1984, 313-15) provides a convincing conceptual criticism, arguing that aggregate or per capita spending measures do not indicate policy decisions, *per se*, leading to misrepresentation of the results of existing models. He demonstrates that measures of the scope of state policies better indicate their distributional properties. Empirical criticisms are related to conceptual problems that arise when using aggregate or per capita spending measures: their use lends itself to income-driven explanations for differences in spending (see, e.g., Aronson and Hilley 1986, 31-47).

The spending-based measure used herein, however, captures states' welfare efforts and is not necessarily income-driven. It is empirically distinct from aggregate and per capita spending measures.¹ We measure the relative amount of resources policymakers devote to providing health insurance to their indigent populations. The range of behaviors is wide. Some poor states' welfare efforts are disproportionate to the adequacy of their welfare grants package. Some wealthier states exert lower effort yet provide more generously than poorer states. Still other states spend a moderate amount of their revenues on welfare and provide fairly

adequate grants (Albritton 1983, 392-93).

Supply and demand are difficult to measure accurately. Empirical studies show that utilization of services increases with the number of providers in an area (Fuchs 1978; Roemer 1961), especially when the recipient of services pays little out of pocket (Wilensky and Rossiter 1983). The total number of registered physicians in nonfederal employment in each state during 1982 is used as a proxy for supply in this research. Use of the physician measure reflects their portrayal as agents who channel patients to other services (e.g., hospital inpatient care, etc.) through referrals (Pauly and Redisch 1973).²

We use a simple measure of demand: the unduplicated count of the number of persons receiving Medicaid services of any kind during 1982. Two other indicators, poverty population and eligibility pool, were considered but discarded. Poverty population is inappropriate since many of the poor are not eligible for Medicaid because of differences in state eligibility policies. The use of an estimate of potential eligibility in a given state as a measure of demand was discarded because persons eligible for Medicaid who fail to enroll do not reflect demand for the program. In the final analysis reciprocity (i.e., realized demand) most closely approximates the construct we wish to make operational.

Operational Models

We embrace the premise that socioeconomic and political characteristics of states are complementary, not competitive, in their influence on policy outcomes (Godwin and Shepard 1976; Hanson 1983, 1984; Plotnick and Winters 1985). States' socioeconomic and political traits may facilitate or constrain Medicaid effort. Wealth exerts a positive influence on the breadth of welfare coverage (Albritton 1983; Hanson 1983; Plotnick

and Winters 1985). Similarly, higher wealth is associated with more expansive Medicaid policy coverage (Davidson 1980; Hanson 1984). State policies reflect decisionmakers' collective understandings of how best to use available resources. Available resources limit the range of policy possibilities.

Political and economic development are crucial determinants of governmental actions. First, increased development generally is associated with increased wealth. Second, political development occurs alongside social and economic development (Sharkansky and Hofferbert 1969; Sullivan 1973). As social and economic development advance, interests become more diverse. Where interests are diverse there is increased political conflict and greater demand for government action. Development increases demand for distributive decisions.

Institutions adapt to changes in the political environment; complex institutions and institutional relationships are needed to process the demands of diverse publics. Well-developed political systems characteristically have competitive political parties, highly diverse interest groups, professionalized bureaucracies and legislatures, and high voter turnout and citizen participation. We seek to portray variations in the endogenous variables of interest as functions of varying levels of economic and political development in the states.

Demand for, and supply of, medical care are best expressed as functions of one another, an endogenous policy measure, and exogenous measures of states' economic and demographic characteristics. Policy is a function of endogenous measures of demand for, and supply of, care and of exogenous measures of states' economic, demographic, and political characteristics. Equations operationalized below follow the forms specified in equations 1-3. Demand for Medicaid policy has no explicitly political content and

takes the form

$$\begin{aligned} \text{DEMAND} = & a + b_1 \# \text{SUPPLY} \\ & - b_2 \text{PRICE} + b_3 \text{URBAN} \\ & + b_4 \text{UNEMPLOYMENT} \\ & + b_5 \# \text{MEDICAID EFFORT}, \end{aligned} \quad (4)$$

where DEMAND = unduplicated count of persons receiving Medicaid services, #SUPPLY = estimated number of non-federal physicians, PRICE = expenses per inpatient hospital day, URBAN = urban population, and #MEDICAID EFFORT = estimate of the proportion of total personal income devoted to Medicaid.

As expressed in equation 4, we expect demand for Medicaid services to be driven by supply, policy, economic, and demographic features of state environments. We expect demand to rise with increases in the supply of services available, urbanization, unemployment, and Medicaid effort. Demand is expected to diminish in the face of higher prices.

Since no out-of-pocket expenses are incurred by recipients of Medicaid services, charges are not generally thought to deter eligibles' seeking care. However, Medicaid reimbursement rates, set by the state, come into play. Setting Medicaid reimbursement rates low relative to private reimbursement rates or to the actual cost of providing care diminishes providers' incentives to participate in the program (Davidson 1982; Grannemann and Pauly 1983; Long, Settle, and Stuart 1986). Medicaid recipients, for whom the psychological discomfort of seeking charity services already depresses service utilization (Dutton 1978), may be even less prone to seek care in the face of provider dismay with low reimbursement rates. Ergo, low reimbursement relative to prices may suppress both supply and demand of Medicaid.

Supply of Medicaid services is explained somewhat as demand is. The supply of medical care services generally

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increases along with economic development and urbanization. The profitability of medical enterprises depends on characteristics of the market for care. Hospitals require an adequate market to operate in the black. Physicians prefer to practice where there are sufficient numbers of persons to constitute an adequate caseload, other physicians, and adequate research and clinical facilities (O'Donoghue 1976). Following this logic, supply is expressed

$$\begin{aligned} \text{SUPPLY} = & a + b_6 \# \text{DEMAND} \\ & + b_7 \text{PRICE} + b_8 \# \text{MEDICAID} \\ & \text{EFFORT} + b_9 \text{URBAN} \\ & + b_{10} \text{INCOME}, \end{aligned} \quad (5)$$

where SUPPLY = number of nonfederal physicians, #DEMAND = estimate of the unduplicated count of Medicaid service recipients, PRICE = average expenses per inpatient day, #MEDICAID EFFORT = estimate of the proportion of state's total personal income devoted to Medicaid, URBAN = urban population, INCOME = personal income.

The supply equation 5 reflects the notion that providers locate in strong markets. We expect the endogenous measures of demand (#DEMAND) and state Medicaid effort (#MEDICAID EFFORT) to influence supply positively. Physicians choose to locate in strong markets; the absolute number of persons eligible for Medicaid increases with income and urbanization. Both urbanization and income influence the supply of health providers directly and positively. The positive influence of price on supply is consistent with the logic that providers locate where they can maximize income.

Estimates of demand and supply reflect the developmental model discussed above. In equation 6 we model variations in states' Medicaid spending efforts. Equation 6 differs from equations 4 and 5 inasmuch as we incorporate measures of political development in our explanation. Medicaid effort is estimated

$$\begin{aligned} \text{MEDICAID EFFORT} = & a \\ & - b_{11} \# \text{SUPPLY} - b_{12} \# \text{DEMAND} \\ & + b_{13} \text{PRICE} + b_{14} \text{IDEOLOGY} \\ & + b_{15} \text{GROUPS} \\ & + b_{16} \text{BUREAUCRACY} \\ & + b_{17} \text{URBAN}, \end{aligned} \quad (6)$$

where MEDICAID EFFORT = the proportion of total personal income devoted to Medicaid spending, #SUPPLY = estimate of the number of nonfederal physicians, #DEMAND = estimate of the unduplicated count of Medicaid service recipients, PRICE = average expenses per inpatient day, IDEOLOGY = Americans for Democratic Action rating of states' congressional delegation, GROUPS = Morehouse (1981) index of interest group density, BUREAUCRACY = spending for Medicaid administration, URBAN = urban population.

The proxies for supply and demand are expected to influence Medicaid spending effort negatively. Higher proportions of less wealthy states' total populations have incomes at or near Medicaid eligibility cutoffs; but wealthy states, which generally are larger, have greater absolute numbers of Medicaid recipients, and thus have higher aggregate demand for care. Wealthy areas attract more physicians, increasing supply. Medicaid consumes a larger portion of state budgets in poor states. Thus we expect spending effort to decrease as the supply of, and demand for, Medicaid policy increase.

State political characteristics reflect economic development. Poor states tend not to have professionalized state governments. They tend to tax regressively, have low levels of political participation and competition, and tend to sponsor the least innovative or generous welfare programs. Economic development is a catalyst for political and policy development. The political systems in wealthy states, already better developed than those in

poor states, continue to develop. Poor states' systems are hard pressed to catch up with those of wealthy states. We expect coefficients between measures of political competition and development to be related positively to the measure of policy effort. Three measures of state political characteristics—bureaucratic professionalism, interest group diversity, and political ideology—are incorporated to capture unique dimensions of each state's degree of political development.³

Bureaucracies influence complex policy areas through their control of expertise and information (Niskanen 1971; Thompson 1981; but see Miller and Moe 1983). The primacy of the states at Medicaid's inception underlines the importance of having a well-developed system for program administration. Responsibility for the interpretation of complex Medicaid eligibility policies, determinations of what constitutes appropriate care (and thus whether it will be paid), and other specialized decisions heighten bureaucratic power. We use the amount spent during 1982 for Medicaid administration as a proxy for bureaucratic professionalism. We expect it to influence the Medicaid effort indicator positively.

The diversity of a state's interest groups is also important in Medicaid policy-making. Decision makers seldom decide explicitly to ignore the needs of particular groups. Instead, they do what they can with what they have, and groups are excluded due in part to poor articulation of demand. Olson (1982) argues that the overall direction of state Medicaid policies reflects the demands of the majority, who are attuned only to "conspicuous and controversial choices" but that "smaller choices needed to implement [the] programs are influenced primarily by a minority of organized providers of care" (Olson 1982, 27-28). That all states now have Medicaid policies reflects the observation that the broad outlines of policies are passed to satisfy broad public de-

mand. Few citizens know the proportion of their incomes spent to support Medicaid. The specifics of policies are determined largely by the group conflicts in a state.

Our measure of interest group diversity is based on Morehouse's (1981) three-tier categorization of interest group strength in the states. Guided by existing literature on the states, she categorizes their group systems on a continuum ranging from strong groups (representing states with monolithic industries and poorly developed group systems subject to control by single groups) to weak groups (suggesting a multiplicity of groups representing diverse interests).⁴ States with strong groups are coded one, moderate groups two, and weak groups three. We expect diverse interest groups to influence states to devote higher proportions of their budgets to Medicaid, as their diversity opens the door for customarily weak groups. This may work in a couple of ways. First, a diversity of groups may signal heightened power for welfare rights groups or others demanding broad welfare policy. Second, and possibly more importantly, providers' groups are more diverse where there are more groups in the aggregate. Diverse providers may demand different things from Medicaid, so the program may prove larger in terms of the pool of eligibles included, the types of services covered, and in the share of state income it consumes.

Finally, we expect state ideology to enter significantly into the Medicaid effort equation. The portion of personal income dedicated to Medicaid will increase with heightened ideological liberalism. State ideology is not especially easy to measure; we use as proxy for liberalism the average Americans for Democratic Action (ADA) legislative score for members of each state's congressional delegation. This is not an ideal measure. It addresses neither state policy issues nor state policymakers' voting records, but it is sufficiently sensi-

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Table 1. The Determinants of Demand for Medicaid Services

Variable	Elasticity	Standard Error	T-Ratio	Approximate Probability > T
INTERCEPT	4.32	2.02	2.14	.04
#SUPPLY	1.21	.25	4.77	.01
PRICE	-.45	.27	-1.64	.11
URBAN	-.18	.21	-.90	.37
UNEMPLOYMENT	.87	.20	4.43	.01
#MEDICAID EFFORT	.18	.19	.94	.35

Note: $N = 49$; $DF = 43$; $R^2 = .94$; $F = 142.6$; $P > F = .01$. All variables are expressed as natural logarithms. See the appendix for variable sources and definitions.

tive to pick up variations among states that translate into state policy differences.⁵

To summarize, the model of Medicaid policy-making developed above is based on the assumption that there is a supply and demand for public policy. Socio-economic measures are exogenous determinants of demand, supply, and policy. Measures of political characteristics are exogenous determinants of policy outputs.

Data Analysis

Models are estimated cross-sectionally using 1982 data.⁶ We choose 1982 because several states engaged in Medicaid reform in response to OBRA 1981. The results reported are products of two-stage least squares estimation, which differs from ordinary least squares estimates inasmuch as they account for mutual causation among the endogenous variables. Estimation of a simultaneous system best fits the conceptual model developed above.⁷

All variables used are transformed to natural logarithms for two reasons. First, logging variables mediates heteroscedasticity in some distributions.⁸ Second, parameter estimates between two logged variables may be interpreted as elasticities. An elasticity of one indicates that the dependent variable changes 1% with each percentage change in the independent

variable. Thus the use of logged variables reduces the problem of heteroscedasticity and clarifies the meaning of the regression coefficients.

Demand for Medicaid services (Table 1) is affected most strongly by supply and unemployment. Demand for services increases 1.2% with each percentage increase in the supply of physicians, and almost .9% in the presence of a 1% rise in unemployment. Parameter estimates for supply and unemployment are statistically significant well under the .01 critical range.⁹ The price variable coefficient is negative ($b = -.45$) and approaches statistical significance ($p = .11$), suggesting that price constraints demand for Medicaid services despite there being no real financial cost to the recipient.

The elasticity for the Medicaid effort coefficient is .18; demand increases less than .2% with each percentage increase in the proportion of a state's personal income devoted to Medicaid spending. Contrary to our expectations, urbanization's influence on demand is opposite that of the Medicaid effort variable ($b = -.18$), indicating that demand for services decreases almost .2% with each percentage increase in urban population.

Supply of services (Table 2) is most sensitive to income, followed in turn by Medicaid effort, price, and urbanization. The supply of physicians increases over .7% in the presence of a 1% increase in

Table 2. The Determinants of Supply of Medicaid Services

Variable	Elasticity	Standard Error	T-Ratio	Approximate Probability > T
INTERCEPT	7.23	1.81	3.99	.01
#DEMAND	-.04	.12	-.34	.74
PRICE	-.33	.17	-1.89	.07
#MEDICAID EFFORT	.34	.13	2.60	.01
URBAN	.27	.09	3.05	.01
INCOME	.73	.15	4.99	.01

Note: $N = 49$; $DF = 43$; $R^2 = .98$; $F = 339.4$; $P > F = .01$. All variables are expressed as natural logarithms. See the appendix for variable sources and definitions.

personal income. The effect of spending effort is not so pronounced: physician supply increases about .3% with each percentage increase in Medicaid effort. Contrary to our expectations, price has a polar opposite effect: supply decreases .3% with each 1% increase in hospital expenses per day. Physician supply increases about .25% with each 1% increase in urbanization. The demand elasticity ($b = -.04$) is neither statistically significant nor in the hypothesized direction.

Regression results for equation 6, in which Medicaid spending effort is the dependent variable, are contained in Table 3. Medicaid effort is most sensitive to supply: states' Medicaid spending efforts decline by .8% with each percentage increase in the supply of physicians. Consistent with the supply varia-

ble, the demand variable is associated negatively with the spending effort measure. As expected, the price variable is related positively to the Medicaid effort measure.

Each of the three political system measures enters the model significantly. Medicaid spending effort increases over .6% with each percentage increase in administrative funding. The interest group measure also exerts a significant impact: Medicaid effort increases as the interest groups in a state political system become more dense. Ideology also influences the degree to which a state devotes resources to paying for Medicaid. Medicaid spending as a proportion of states' total personal income increases by about .17% with each percentage increase in political liberalism. Finally, Medicaid spending

Table 3. The Determinants of Medicaid Effort

Variable	Elasticity	Standard Error	T-Ratio	Approximate Probability > T
INTERCEPT	-1.13	2.64	-.43	.67
#SUPPLY	-.80	.42	-1.92	.06
#DEMAND	-.23	.29	-.78	.44
PRICE	.12	.31	.38	.71
IDEOLOGY	.16	.06	2.65	.01
GROUPS	.35	.12	3.06	.01
BUREAUCRACY	.63	.25	2.56	.01
URBAN	.38	.26	1.44	.16

Note: $N = 49$; $DF = 41$; $R^2 = .49$; $F = 5.74$; $P > F = .0001$. All variables are expressed as natural logarithms. See the appendix for variable sources and definitions.

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effort increases nearly .4% per percentage increase in urban population.

Discussion

The empirical results support our characterization of policy as an outcome of market influences, reflecting the effect of the peculiarities of the health services market on public spending effort. First, they demonstrate that the supply of medical services affects both demand and Medicaid spending effort strongly. Increasing the number of physicians in a state by 1% leads to a greater-than-1% rise in demand for services. Despite this higher demand for services, Medicaid spending effort declines precipitously in the presence of high numbers of providers. Hanson (1984) reports that the eligibility and service coverage of state Medicaid policies increase along with the number of physicians. Taken together, this implies that high spending effort is not associated with good service coverage. States exerting greater effort get worse returns than states spending relatively less.

In a seemingly contrary result, our results show that the supply of services increases with spending effort. The negative influence of supply on spending effort is immediately confounding, but makes sense upon further consideration. States that spend relatively high proportions of personal income on Medicaid are primarily poor states with correspondingly ill-developed health sectors. There are fewer physicians where the proportion of spending is highest. Physicians are drawn to wealthy areas, so areas with high incomes have more physicians and devote lower proportions of total income to Medicaid.

Consistent with our expectations, the sign of the price variable is positive in the Medicaid effort equation. Price measures variation in states' costs of providing access to care. In two states with identical amounts of personal income and identical

Medicaid programs, higher costs of care lead necessarily to higher proportions of personal income being spent on Medicaid. Under conditions of physician or hospital bed oversupply states may have the latitude to set reimbursement rates well below the usual fee as result of weakened provider interest groups.

Treatment of demand as endogenous to the system of equations best reflects our understanding of theories of health markets. These results indicate that demand for Medicaid-paid health care significantly affects neither the supply of services nor the relative Medicaid spending efforts of states' citizens. These findings suggest that supply of services is the driving force in determining both demand and spending effort, although it affects them in opposite ways, driving the former up and the latter down. Demand is most sensitive to measures of supply and unemployment and insensitive to the policy indicator, reflecting the widespread notion that spending for the program is out of control.

The positive influence of political system traits reflects the redistributive aspects of Medicaid policies. Consistent with our expectations, ideology, interest group diversity, and administrative professionalism influence Medicaid effort positively. Their influence, taken with the negative effect of supply, suggests an independent influence of state political characteristics on redistributive policy decisions. Operating within socio-economic constraints and federal guidelines, states' spending efforts are influenced independently by ideology and political development.

State efforts for providing health insurance to the indigent population are conditioned by political, market, and socio-economic factors. Increases in the number of providers leads to decreases in spending effort. Higher spending effort increases supply. Where interest groups are most dense there is higher Medicaid spending effort. According to the logic of

Olson (1982, 1985), this is to be expected: organized interests exert direct influence on policy decisions. In a related vein, the health care bureaucracy takes on characteristics of a captured regulatory agency as described by Stigler (1968), Mitnick (1980), and others. From that perspective there is no need for the Medicaid recipient population to demand generous Medicaid policy in the presence of a large provider sector. Their interests are protected because they converge with providers' interests. The incorporation of political and economic components in the model provides a more thorough explanation for differences in state Medicaid effort than is gained through the exclusive use of either approach.

Conclusion

The results reported herein have both substantive and methodological implications. Substantively, they suggest that large portions of state and national budgets are at the mercy of a relatively small and well-organized interest. This is not especially surprising. Medical associations have successfully blocked various national health insurance plans (Poer 1979), influenced the design of Medicaid and Medicare (Olson 1985; Starr 1982), and protected members' economic interests by strategically donating funds to house members sitting on committees with jurisdiction over health care regulatory issues (Keiser and Jones 1986). Our finding that states with lower numbers of providers spend higher proportions of personal income on Medicaid may be interpreted in at least two ways. First, and most charitably to providers, it may indicate that poor states by definition have lower amounts of personal income and higher percentages of persons in need of state-supported health insurance, thus increasing Medicaid effort. Another interpretation, supported by the finding that Medicaid effort increases as the number of

providers decreases, is that relatively smaller provider groups are better able to influence policy than larger, more diverse groups, as are found in wealthier states. Where there are large numbers of providers the health care industry is sufficiently fragmented by recent changes in the organization and delivery of health services that neither the industry as a whole nor its parts—physicians, hospitals, nursing homes, and so on—constitute monolithic interests that can reasonably be expected to speak with a unified voice.

Physicians are fragmented along the lines of specialization, form of practice (e.g., private office, group, prepaid, etc.), and other characteristics. Hospitals, where most of the money goes, are also divided along several dimensions. Interests of charity hospitals differ from those of for-profits, rural hospitals' needs differ considerably from urban hospitals', teaching hospitals' needs differ from nonteaching hospitals', and so on. Small, unified interests are more effective than large, diverse interests (Olson 1965, 1982). As a state's health care providers become more diverse their ability to influence policy decreases.

The strength of individual health interest groups also has been eroded by the growing strength of insurance companies, whose interests are often opposed directly to those of provider groups. Blue Cross and Blue Shield, established during the 1930s as financing adjuncts of the hospitals and physicians, respectively, once controlled the majority of the health insurance market and have seen that share diminish rapidly during the past decade. Neither Blue Cross nor Blue Shield does the bidding of its respective patron organization as once was the case. More threatening to providers' groups are proprietary insurers, who confront providers more openly on questions of cost containment. Providers are being forced to accept the role of private and not-for-profit

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insurers in determining appropriate forms of treatment and payments for particular episodes of illness. Medicaid programs may simply be lagging behind their private sector equivalents and may in the future exert a greater influence over spending outcomes.

A diversity of interest groups, especially in the presence of liberal ideology and a well-funded bureaucracy, may lead to higher amounts of income being used to purchase health care services for purely altruistic reasons. However, Medicaid may simply be a case in which the interests of the indigent population and the health care industry converge. With few groups the physicians' and hospital lobbies are likely to be less fragmented and, because expansion of Medicaid opens the medical market to numerous types of providers, may be biased toward having a fairly restrictive program. In that case physicians may opt to discriminate against other types of providers, choosing control over a small pie to competition for a slice of a larger one.

These findings also indicate that citizens living in relatively more developed states get proportionately more from Medicaid spending than persons in poorer states. Medicaid recipients receive better access to care in states with more providers and pay a lower proportion of personal income than they would for worse access in a poorer state. Ironically, conservative citizens who wish to hold down access to Medicaid for ideological reasons may be better served economically by living in a wealthier, more liberal area. There a smaller portion of their income is spent for welfare medicine, which may better reflect their preferences. Federal financial contributions, designed to equalize programs among states, are not achieving that goal; citizens in poorer states bear a larger Medicaid burden than they would if they lived in a wealthier area.

These results also point to what may be

a promising avenue for future comparative policy research. Although applied only to Medicaid, the approach used herein may be incorporated into a more general model of policy determination. Relationships in the Medicaid policy environment may be likened, for example, to relationships that affect governmental dealings with the road-building industry. In each case, neither providers (i.e., physicians or construction companies) nor recipients (i.e., Medicaid eligibles or parties to be served by new roads) need be entirely satisfied with the policy enacted, but the groups' demands are joined by a common bias toward program expansion. Medicaid eligibles want more services. Providers want an expanded market. The public may be unwilling to bear the costs of expanded service but is not easily mobilized given their general disorganization and apathy. Medicaid's reliance upon private providers ensures physicians' groups', hospital associations', and other health care industry's interests a prominent role in determining the scope of a state's Medicaid program. Eligibles receive more expansive services as government meets the demands of providers. Government's role in the program is to monitor and control spending; but the political costs of doing so may outweigh the fiscal gains, thus providing few political incentives.

The empirical evidence supports portions of the market model of policy determination. This is consistent with the progression of comparative policy outputs research. The aim of early comparative policy research (e.g., Dawson and Robinson 1963; Dye 1966; Hofferbert 1966) was to test competing explanations for differences in state spending, one based in public finance economics (see, e.g., Fabricant 1952), the other deduced from the work of political scientist V. O. Key, Jr. (1949).¹⁰ More recently, researchers have moved beyond debates addressing the primacy of either economic or political ex-

planations for policy differences to the synthesis of political and economic models of governmental policy decisions (see, e.g., Chubb 1985; Godwin and Shepard 1976; Hanson 1983, 1984; Plotnick and Winters 1985). This synthesis has done much to aid in developing theories of governmental policy decisions: Charles Jones' once-apt observation that the cumulative theoretical contribution of the literature was "like taking a long trip only to find yourself back home again" (1973, 34) no longer holds. We think the road is now fairly clearly marked.

Appendix: Variables and Sources

SUPPLY: Total nonfederal physicians, 1982. Source: American Medical Association, *Physician Distribution and Medical Licensure in the United States* (Monroe, LA: American Medical Association, 1984).

DEMAND: Total unduplicated Medicaid recipients, fiscal year 1982. Source: Health Care Financing Administration, HCFA 2082 data files (Washington: U.S. Department of Health and Human Services, 1982).

MEDICAID EFFORT: State Medicaid vendor payments as a proportion of total personal income, 1982. Source: Authors' calculations, Health Care Financing Administration, HCFA 2082 data files (Washington: U.S. Department of Health and Human Services, 1982); and U.S. Bureau of Census, *Statistical Abstract of the United States*, 104th ed. (Washington: U.S. Bureau of Census, 1983).

PRICE: Inpatient expense per day, 1982. Source: American Hospital Association, *Hospital Statistics* (Chicago: American Hospital Association, 1983).

UNEMPLOYMENT: Total unemployment, 1982. Source: U.S. Bureau of Census, *Statistical Abstract of the*

United States, 104th ed. (Washington: U.S. Bureau of Census, 1983).

URBAN: Total number of persons in metropolitan areas, 1982. Source: U.S. Bureau of Census, *Statistical Abstract of the United States*, 104th ed. (Washington: U.S. Bureau of Census, 1983).

INCOME: Total personal income, 1982. Source: U.S. Bureau of Census, *Statistical Abstract of the United States*, 104th ed. (Washington: U.S. Bureau of Census, 1983).

IDEOLOGY: State congressional delegation average Americans for Democratic Action (ADA) legislative score, 1982 (ranges from zero to one hundred). Source: Americans for Democratic Action, *ADA World* 40 (1983).

GROUPS: Morehouse (1981) index of group strength.

BUREAUCRACY: Total state Medicaid administrative expenditures, 1982. Source: Health Care Financing Administration, 1984. Medicaid Program Characteristics data files (Washington: U.S. Department of Health and Human Services, 1982).

Notes

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1. The simple correlation (r) between the Medicaid effort measure and total spending is .31.

2. Thus recipients enter a principal-agent relationship as described by Moe (1984), with government acting as monitor.

3. With these measures we seek to capture three dimensions of state political systems: professionalism, competitiveness, and ideological predisposition. (For a discussion of the dimensionality of state

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systems, see Sharkansky and Hofferbert 1969.) Other measures may be substitutable, but we consider these most appropriate for the case at hand. We think the interest group measure is superior, conceptually, to a measure of partisan competition. As has been noted elsewhere, partisan competition is a function of diversity within states (Barrilleaux 1986; Patterson and Caldeira 1984; Sullivan 1973). Further, states with low interparty competition have been shown to be internally competitive (Jennings 1976; Wildgen 1974), suggesting that interparty competition is not necessarily a sound measure of the competitiveness of political systems. Political parties are simply one of the cures for the problems arising from faction noted by James Madison in the *Federalist Papers*; the diversity of interest groups is closer to the heart of the matter.

4. Morehouse's categorization of group strength in the states is based upon her fairly impressionistic reading of materials pertaining to the subject within the states. To validate her measure, she reports simple correlations between her categorical measure and an updated Hofferbert integration index (Morehouse 1981, 113). The distribution of weak group states (i.e., numerous groups) correlates strongly and positively with states with high integration scores ($r = .87$). The distribution of strong group states (i.e., few groups) correlates negatively and strongly with the distribution of states scoring low on the integration measure ($r = -.57$). The integration index is designed to measure a state's social and tertiary economic development. Integration indicates diversity, as does diffuse group power. Hence we present the Morehouse measure as a suitable measure of the diversity of interests within the states.

5. The measure of state citizens' ideology developed by Wright, Erikson, and McIver (1985) appears to have greater face validity than the ADA scores indicator used herein. Briefly, Wright, Erikson, and McIver's measure is constructed by aggregating 1976 through 1982 CBS-*New York Times* poll responses to an ideological self-evaluation question. Index scores are calculated for 47 contiguous states and for a pseudostate constructed from District of Columbia responses. They construct two indexes from the survey data—a "general population" measure and an "active electorate" measure—each of which was tested in our model. The survey-based measures have little effect on parameter estimates of ideology's effect on Medicaid effort. Using the Wright, Erikson, McIver "active electorate" measure in the Medicaid effort equation, $b = -.15$, $t = -2.26$. The parameter estimate with the ADA measure ($b = .16$, $t = 2.6$) is remarkably similar. Signs are reversed because higher scores on the ADA measure indicate a more liberal ideology and high scores on the survey measure represent less liberal ideology. Other relationships in the system are stable, although the explanatory power of the model increases when the survey measure is used.

Given this, we use the ADA measure for two reasons. First, it is available for each of the 49 states that had Medicaid programs during 1982. We think it more valuable to maintain cases, especially when the empirical gain of using the survey-based measure is marginal, especially given our use of simultaneous estimation methods. Second, the ADA measure, though not directed explicitly at state policy issues, contains some policy-relevant information inasmuch as it reflects voters' willingness to send "liberal" members to Congress. The survey measure contains none. Our findings are consistent with those of Holbrook-Provow and Poe (1987), who report little difference in the empirical performance of roll call measures of state ideology and the Wright, Erikson, and McIver measure.

6. Our data cover the 49 states that had fully implemented programs during 1982. The excluded state, Arizona, began implementing a demonstration program during the early 1980s.

7. For discussions of simultaneous equations methods, ranging from practical to somewhat less so, see Gow and White 1986; Kennedy 1984, 106–26; Maddala 1977, 470–92; and Pindyck and Rubinfeld 1981.

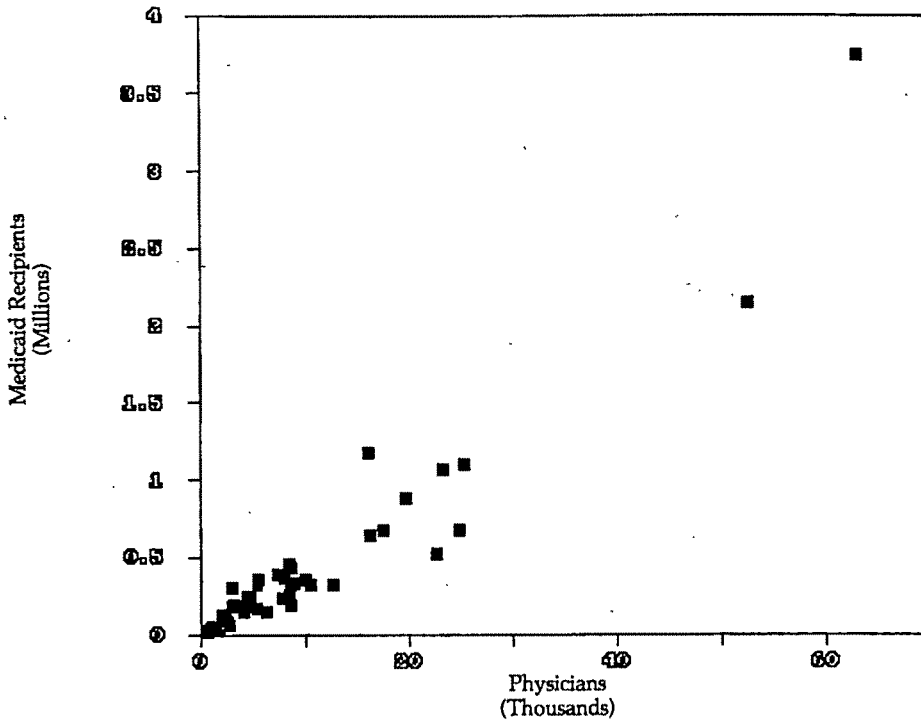
8. Heteroscedasticity occurs when the variances of error terms are not equal for each observation. It is often a problem with aggregate data research, especially when the size of units of analysis varies widely, as with state data. The figures below illustrate the mediating effect of logging variables.

Figure 1, in which raw values of physicians are plotted against raw values of Medicaid recipients, is a heteroscedastic distribution. When values are converted to natural logs (Figure 2), the distribution assumes a linear form.

9. We are sensitive to arguments against the use of tests of significance when using a population, as is the case with this research. One critic pointed out to us that "God and Eisenhower" gave us only 50 states and, more importantly, argued that good policy decisions may be ruled out based on arbitrary decision rules. Though we accept these problems, we consider statistical tests useful evidence in evaluating research. Further, compelling conceptual and statistical arguments for the use of significance tests in population-based research remain. Conceptually, a cross-section of states constitutes neither a closed theoretical nor a closed population system (Blalock 1967, 131–32). For a theoretical system to be closed, no variables may have been omitted and mathematical functions must be perfect with no random or error terms (Blalock 1967, 131). Empirically, statistical tests are important when used to assess the reliability of estimates. The standard errors of regression coefficients may be viewed as evidence of the reliability of estimates rather than as tests of the null hypothesis (Blalock 1967; Schneider and Darcy 1984; Winch and Campbell 1970).

10. Much of the "politics-versus-economics" de-

Figure 1. Plot of Unlogged Values



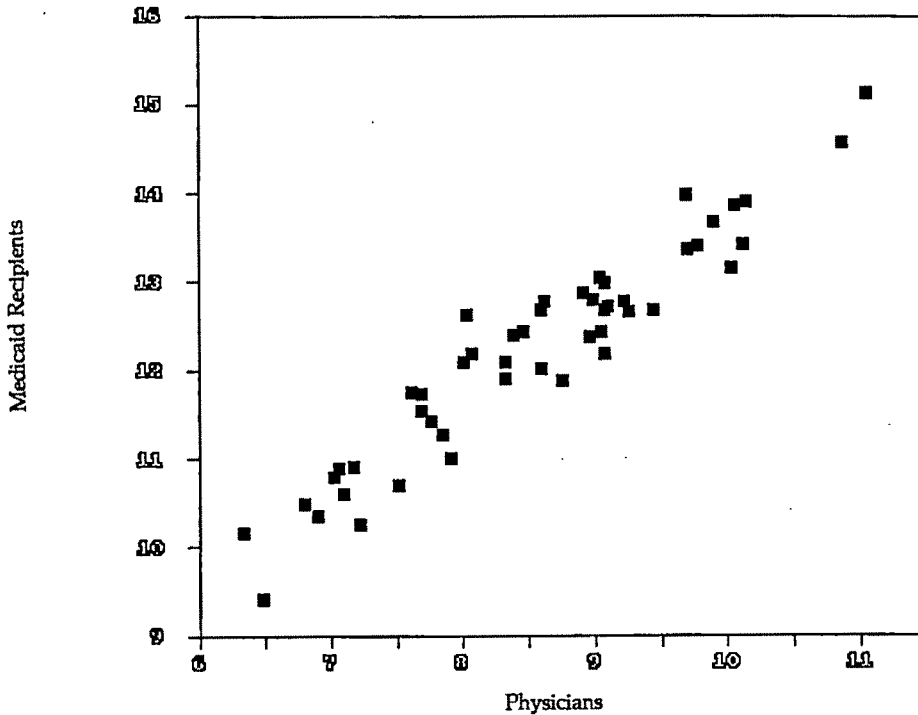
bate seems to be based on loose readings of Key's *Southern Politics* (1949). Jones (1973) documents widespread incorrect citation of putatively seminal passages, suggesting that scholars relied on their forebearers' interpretations rather than the original. More conclusively, Uslaner (1978) argues that Key's research provides little theoretical justification for widely tested hypotheses regarding the effects of party competition and legislative malapportionment on the policy outputs of democratic governments.

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**Figure 2. Plot of Logged Values
Using Natural Logarithmic Function**



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ORGANIZED INTERESTS AND AGENDA SETTING IN THE U.S. SUPREME COURT

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Participation as amicus curiae has long been an important tactic of organized interests in litigation before the U.S. Supreme Court. We analyze amicus curiae briefs filed before the decision on certiorari and assess their impact on the Court's selection of a plenary docket. We hypothesize that one or more briefs advocating or opposing certiorari increase the likelihood of its being granted. We test this hypothesis using data from the United States Reports and Briefs and Records of the United States Supreme Court for the 1982 term. The statistical analysis demonstrates that the presence of amicus curiae briefs filed prior to the decision on certiorari significantly and positively increases the chances of the justices' binding of a case over for full treatment—even after we take into account the full array of variables other scholars have hypothesized or shown to be substantial influences on the decision to grant or deny.

Each year a multitude of interest groups participate in various guises before the Supreme Court. It is clear that in the last couple of decades interest groups have stepped up their activities before the courts, just as they have in other political arenas (Bradley and Gardner 1985; O'Connor and Epstein 1982; Schlozman and Tierney 1986; Walker 1983). Interest groups can of course choose to take part before the Supreme Court in many different roles, and they most often do so as amici curiae. Organized interests have long participated actively as amici curiae for decisions on the merits. But political scientists, with few exceptions, have not measured the impact of these briefs, and no one has done so for decisions on writs of certiorari and appeal.

We contend that not only are amicus

curiae briefs important, but that the decision to review a case ranks as important as—if not more important than—the decision on the merits. In setting its agenda, the Supreme Court each year determines new winners and losers in the struggle for economic, political, and social power. We believe, therefore, that powerful interests will not stand by passively as the Supreme Court legitimizes literally hundreds of decisions in the lower courts each year and calls many others into question. And so, as Justice Brandeis once remarked, “the most important thing the Court does is not doing.” Thus we ask, To what extent does amicus curiae participation by organized interests influence the Supreme Court's selection of cases for plenary hearing? We address this question within a more general analysis of other relevant forces that impinge upon the Court's

choices in gatekeeping.¹

Here we present the first of our empirical results on the impact of amicus curiae participation on decisions to grant writs of certiorari during the 1982 term of the Court.² These data come from the *United States Reports* and *Briefs and Records of the United States Supreme Court*. For each case we have determined whether or not amicus briefs were filed *before* the decision on certiorari or appeal; which, if any, organization filed; the presence of the federal government as a party or of "conflict"; dissents below; ideological outcome in the lower court; the type of case at hand; the Court's decision; and a variety of other relevant variables. The statistical analysis demonstrates that the presence of amicus curiae briefs significantly and positively increases the chances of the justices' binding a case over for full treatment—even after we take into account the full array of variables that other scholars have hypothesized or shown as substantial influences on the decision to grant or deny.

We organize the discussion and analysis into six sections. After presenting a brief overview of organized group activity in the judicial process, we describe our theoretical perspective and present our primary hypotheses. We then review various other explanations for certiorari analyzed in empirical studies over the years. We describe our data and present the results of our statistical analysis. Finally, we conclude with some general observations on the role of groups in the judicial process and describe some of our plans for future research.

Organized Interests in the Judicial Process

The idea that interest groups have significance in litigation before the Supreme Court goes back at least as far as Bentley's *Process of Government* (1908): "So far

from being a sort of legal machine, courts are a functioning part of this government, responsive to the group pressures within it, representatives of all sorts of pressures, and using their representative judgment to bring these pressures to balance, not indeed in just the same way, but on just the same basis that any other agency does" (p. 393; see also Truman 1951, 479–99; Peltason 1955). The tactics groups can use to influence legislation are considerable, including "class actions" and test cases; participation as amici curiae; giving of advice and service; expert testimony, and financial assistance; and taking control of the law suit (see Barker 1967; Hakman 1966). Vose (1959) blazed a trail with his meticulous and intriguing case study of how the NAACP Legal Defense Fund used a variety of strategies and tactics in its campaign of litigation to end the restrictive covenant in housing (see also, e.g., Manwaring 1962 and the citations collected in Vose 1981).

Interest groups, of course, vary enormously in the extent to which they choose to use the courts in a direct fashion. On the basis of a study of the 1958–64 terms of the Court, Hakman (1966, 47) concluded that the "actual judicial process appears to be a close approximation of the traditional legal model in which judicial policymaking emerges through ad hoc private controversies. . . . Organized interest groups would appear to play a relatively minor role in Supreme Court decision-making" (p. 50). He went on to state in a later paper that "most 'established' or 'permanent' organizations had little or no connection with the judicial process" and seldom used litigation to achieve their goals (Hakman 1969, 245). Nonetheless, despite Hakman's objections, scholars have demonstrated over and over again the vigorous, extensive, and continuing efforts on the part of interest groups to lobby the courts (see O'Connor and Epstein 1983a; Olson 1981;

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Schlozman and Tierney 1986, 358–85; Vose 1981). At least since the 1960s evidence of the participation of interest groups as amici curiae or sponsors has virtually leaped from the pages of the *United States Reports* even on the most cursory inspection.

Although the filing of amicus curiae briefs is just one of many tactics available to organized interests,³ participation as amicus has figured as an indicator of interest group activity in virtually every pertinent study. More important, we have good reason to believe that such filings are consequential. The filing of an amicus brief, apart from the quality or persuasiveness of the arguments presented, provides the justices with an indication of the array of social forces at play in the litigation. In *Baake v. Regents of the University of California*, for example, the Court had 57 briefs from “friends” and without doubt obtained a vivid picture of whose interests were at risk. The Supreme Court, as Krislov has remarked, sometimes views the amicus as a “potential litigant in future cases, as an ally of one of the parties, or as the representative of an interest not otherwise represented” (1963, 704). Even though Hakman (1969) deprecated the importance of interest groups, he reported filings of amicus briefs in nearly 20% of the decisions on the merits between 1928 and 1966 (for a similar figure, see Puro 1971). O’Connor and Epstein (1982) validate the percentage Hakman (1969) and Puro (1971) reported for 1928–1966 and then go on to show that the percentage of cases in which amicus briefs appear has risen to more than half in the last decade.

So, clearly, interest groups have used the vehicle of amicus briefs often and, in recent years, with increased frequency. There is, in addition, some fragmentary evidence that participation as an amicus curiae constitutes an efficacious route for interest groups to take in their attempt to influence the Court. Puro (1971) found

that groups on the “liberal” side participated more often as friends of the Court and were more likely than conservatives to triumph on the merits (pp. 254–55).

We, too, believe that amici curiae make a difference in litigation. And we propose that their impact will be apparent at the agenda-setting phase.⁴ The presence of amici during case selection communicates to the Supreme Court information about the constellation of interests involved, and this information, we suggest, is both valued and heeded by the justices and their clerks.

A Theory of Case Selection

Theoretically, we propose that justices of the U.S. Supreme Court are motivated by ideological preferences for public policy and that they pursue their policy goals by deciding cases with maximum potential impact on political, social, or economic policy (see Murphy 1964; Rohde 1972; Rohde and Spaeth 1976). Chief Justice Rehnquist has spoken words consistent with this notion: “There is an ideological division on the Court, and each of us has some cases we would like to see granted, and on the contrary some of the other members would not like to see them granted” (quoted in O’Brien 1986, 160). This is not, of course, to say that the justices act without other constraints. We recognize that the justices maximize their positions subject to a complicated matrix of legal rules, social norms, and institutional expectations. Nevertheless, the efficient pursuit of policy objectives implies that justices will devote their resources to those cases that have the most profound implications for policy relative to the effort required to hear and to decide the cases. In cases where a justice agrees with the decision of the lower court there is little incentive to alter the status quo, but the costs of reviewing the case may well

be considerable, as the time constraint on justices is enormous (see Baum 1977).

We predict that justices will review the cases they wish to reverse on ideological grounds and the cases they think will have the greatest practical significance.⁵ Normally, of course, the ideological issues at stake in a case are apparent upon a close reading of the petition for certiorari and the brief in opposition. But the practical impact of a case can be quite difficult to assess. Thus, in our theory, we focus on the process by which the justices learn about the practical significance of cases brought before the Supreme Court.

Quite simply, our theory assumes that the potential significance of a case is proportional to the demand for adjudication among affected parties and that the amount of amicus curiae participation reflects the demand for adjudication. We propose that amicus curiae participation by organized interests provides information, or signals—otherwise largely unavailable—about the political, social, and economic significance of cases on the Supreme Court's paid docket and that justices make inferences about the potential impact of their decisions by observing the extent of amicus activity. Although potential amici have the option of filing briefs either in support of, or in opposition to, petitions for writs of certiorari, our theory suggests that both types of briefs should have the same effect—that is, increase the likelihood of a grant. The reason is simply that both kinds of briefs draw attention to the potential significance of a case. Consequently, the mere presence or absence of a brief amicus curiae may weigh in more heavily than the substantive arguments presented.

In this way amicus curiae activity assists the justices in winnowing down the set of plausible cases to an even smaller set of cases that satisfy their own individual ideological agendas. Naturally, even this subset of cases will be larger than what the Court can accommodate

during a given term, and we recognize that further winnowing criteria must be used. Nevertheless, we predict that cases within this smallest set will have an unusually high probability of being accepted for full review.

That participation as amicus curiae is costly to an organized interest is an implicit but necessary assumption of our theory. Only if the preparation and filing of a brief amicus curiae is costly can a brief function as a reliable signal about the importance of a case; otherwise a group might very well file one in each and every case. Naturally, this situation of costless participation would communicate no discriminating information at all to the members of the Court. So far as we can determine, an amicus brief on a petition for certiorari does indeed involve a non-trivial expense. From the general counsels of several large organizations with headquarters in Washington, DC we have obtained estimates of the external costs of preparing an amicus brief on a petition for certiorari. Their estimates consistently fell in the range between \$15 thousand and \$20 thousand for a single brief prepared by a reputable law firm. One suggested that he had paid as much as \$60 thousand. Thus, even organizations with a large budget for litigation cannot and do not file amicus curiae briefs with reckless abandon.

The importance our theory places on the informational value of amicus curiae briefs implies that the rules and procedures of the Supreme Court should encourage—or at least not impede—the filing of briefs and that practitioners should have some awareness of the importance of filing briefs (see O'Connor and Epstein 1983a on changes in the rules). Even though the Court officially does not look with favor on amicus briefs filed prior to the decision on whether or not to grant certiorari or note probable jurisdiction (see Rule 36.1), the Court, in fact, rarely denies motions for potential

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amici to participate at this stage of the litigation. Potential amici have 30 days from the date on which the respondent receives the petition for writ of certiorari or jurisdictional statement to file a brief for or against certiorari or jurisdiction. To qualify as an amicus curiae, a nonparty must show that it has a substantial interest in the legal question and must file a motion and brief in a "timely" fashion. Governments may of course participate as amici without leave of the parties (Rule 36.4). Others may do so with the permission of the parties. If the parties refuse to permit a nonparty to submit a brief, the hopeful amicus may file a motion with the Supreme Court for leave to submit a brief. An organization or individual seeking to participate as amicus curiae will usually submit a brief at the same time it files a motion for leave to file a brief. The Court then acts on both the motion and the brief (see, generally, Stern, Gressman, and Shapiro 1986).

Not only are briefs relatively easy to file from a procedural standpoint, but practitioners recommend doing so. It makes good sense, according to one practitioner, to file a brief prior to certiorari even if the Court denies the motion. The Court, after all, has to read the brief to find out whether the motion is meritorious. The leading manual of practice in the Supreme Court states that "where there is doubt, as there usually is, that a petition will be granted, statements by amici which show that the case is generally important can be of significant aid to the petitioner" (Stern, Gressman, and Shapiro 1986, 391; see also Ennis 1984). These scholars go on to add that "indeed, an amicus brief that demonstrates that the case is important to others besides the immediate parties may be more helpful at this stage than after certiorari has been granted" (Stern, Gressman, and Shapiro 1986, 395; see also Baker 1984). But they and others advise that nonparties who do not favor jurisdiction should refrain from

filing a brief amicus curiae against a grant of certiorari. A brief of this sort may simply call attention to a case. One scholar-practitioner boldly states, "Never file an amicus curiae brief against certiorari" (Shapiro 1984, 24). Such advice implies, as does our theoretical perspective, that the primary informational value of amicus briefs lies in their presence or absence alone, not in the direction of the legal arguments advanced within.⁶

Because of the confidentiality of the conference and individual deliberations, we have no means of knowing with certainty how the justices view and value briefs amicus curiae prior to certiorari. Still, evidence in the private papers of the justices suggests that at least some of the justices some of the time pay attention to the presence, identity, and positions of amicus curiae for or against certiorari or jurisdiction.⁷ For example, in a memorandum on *Citizens Publishing Company et al. v. United States*, Chief Justice Warren's clerk remarked,

Two amicus briefs have been filed urging that the Court N[ote] P[robable] J[urisdiction]. The amici are publishers and individual papers operating in towns where papers operate under joint agreements which merge production, advertising, and circulation. The amici argue that this case has significance for the metropolitan newspaper industry, an industry in which it has become increasingly difficult for papers to operate without joint agreements. The appellants and amici present detailed arguments urging that it is not financially feasible for many papers to operate if the present decision stands. . . . It appears that the question is clearly substantial and it seems the Court should NPJ. (no. 243, 1968 term)

We could, without difficulty, present numerous examples of communications of this sort during both the Warren and Burger Courts.⁸

Our theoretical perspective gives us confidence that amicus curiae briefs and the ideological outcome in the lower court are important variables in justices' calculations about certiorari. We make no claim, though, that amicus briefs and

ideological considerations exhaust the list of important predictors of certiorari. Previous research has revealed several other variables that appear to carry considerable weight in certiorari decisions, and we must take these forces into account in the subsequent test of our hypotheses. We turn now to a brief review of these findings.

Explanations of Gatekeeping in Previous Research

Since the passage of the Judiciary Act of 1925 the Supreme Court has for all intents and purposes functioned as a court of discretionary jurisdiction. Rule 17 (formerly 19) sets out the technical criteria for acceptance of a case via a writ of certiorari: the presence of important issues the Court has yet to decide, conflict among the courts of appeals on an issue, disagreement between the lower court and the Supreme Court's precedents, and deviation from the "accepted and usual course of judicial proceedings" in the court below (Stern, Gressman, and Shapiro 1986, 193-253). These desiderata, of course, only establish a starting point for the justices in their search.

Empirical evidence strongly suggests that Rule 17 does not explain how the Court makes gatekeeping decisions. Tanenhaus and associates (1963) discovered that reasons mentioned in Rule 17 appear in only two-thirds carried over for decision. Rather than simply applying Rule 17, Tanenhaus argued, the justices scan the petitions for a writ of certiorari in quest of readily identifiable cues. Justices, like other public officials, feel the pressure of work and the lack of time and so develop routines and shortcuts to achieve their goals. Using the *United States Reports*, Tanenhaus and his colleagues assessed the effects of four potential cues: presence of the United States as a party, presence of a civil liberties issue, dissen-

sion in the courts below, and an economic issue. These scholars found that all of the cues with the exception of economic issues increased the probability of the Court's taking the case.

Mining the data in Justice Burton's papers, Ulmer, Hintze, and Kirklosky (1972) studied the petitions that the chief justice had not placed on the "deadlist" and that thus had been discussed in conference. They reported that of the several cues, only the United States as a party made a difference in the Court's decision to grant or deny certiorari. Provine (1980)—also working with Justice Burton's papers—sought to controvert Tanenhaus and his associates. She argues that the patterns of voting do not support "cue theory" (p. 80): "The justices do indeed seem to act on certain shared ideas about the proper work of the Supreme Court, ideas that correspond with the responsibilities of the court of last resort in any complex legal system" (p. 103). Nevertheless, Provine's own results suggest a significant relationship between the variables Tanenhaus had identified and the decision to grant review.

More recently, Ulmer (1983, 1984) has persuasively shown that conflicts between the circuits and conflicts between the lower court and Supreme Court precedent dramatically raise the probability of the justices' placing the case on their agenda; and "genuine," as opposed to "alleged," conflict apparently motivates the Court (see also Feeney 1975). Other scholars have contended that the justices take into consideration not only attributes of the case but also the direction of the lower court's decision. The justices, in a word, take the case in order to reverse it in accordance with their preferences. Thus, for example, Armstrong and Johnson (1982) found that the Burger Court was more likely to grant a writ when the lower court's decision favored civil liberties than when the decision went against the claim of individual rights (see also Palmer 1982;

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Provine 1980; Songer 1979; Teger and Kosinski 1980; Ulmer, Hintze, and Kirklosky 1972; for macro-level analyses see Caldeira 1981 and Likens 1979; for a process model, see Perry 1985, and Perry 1986).

Quite apart from the ideological alignments of the justices, the presence or absence of conflict between or within lower courts has proven useful for explaining the granting or denial of certiorari (Tanenhaus et al. 1963; Ulmer 1984). When the appellate court immediately below has reversed the lower court, the Supreme Court is more likely to grant certiorari, and when one or more judges in the court immediately below have dissented, the justices on the Supreme Court have, *ceteris paribus*, evinced a greater propensity to grant. The justices themselves will on occasion mention a dissent from below. More important for our purposes, in their memoranda on petitions law clerks regularly note who has written majority and dissenting opinions in the lower court and whether a reversal has occurred.⁹ Both of these indicators—dissent and reversal below—signal ferment in the lower courts and suggest a problematic outcome, one perhaps worthy of a closer look.

Several scholars have demonstrated that as the Supreme Court chooses its plenary docket, it exercises biases in favor of or against certain kinds of issues. Thus, for example, Tanenhaus and his colleagues reported a significant relationship between the presence of a "civil liberties" claim and gatekeeping decisions (see also Provine 1980). We distinguish between "civil liberties" and other kinds of cases and test for the possibility that the Supreme Court is differentially sensitive to petitions based on these claims.¹⁰

Research has also demonstrated that when the federal government petitions as a party for a hearing, the Supreme Court is significantly more likely to grant certiorari than under other circumstances.

The solicitor general, as others have demonstrated, maintains a special relationship with the Supreme Court; in fact, some refer to him as the "Tenth Justice" (Caplan 1987; Scigliano 1971; Segal 1988; Ulmer and Willison 1985). For a variety of reasons, he and his office have over the years maintained a high winning percentage as petitioners seeking certiorari and on the merits. For one, the solicitor general carefully screens cases for good vehicles and refuses to bring others. For another, because of the long-term stability of the staff, the Office of the Solicitor General constitutes a classic instance of a "repeat player," with all of the advantages attendant on that status (see Galanter 1974). (But see Caplan 1987 for a suggestion that recent solicitors general have squandered some of those advantages.)

On the one hand, the previous work on agenda setting in the Supreme Court impresses us as empirically sound, cumulative, and statistically sophisticated. On the other hand, we worry about the excessive and exclusive reliance on "cue theory" and the inability of existing models to account for much of the Court's agenda. Quite clearly, the Court is not bound by custom or procedure to hear only "conflictual" cases or cases in which the federal government is a party (e.g., Estreicher and Sexton 1986). In fact, a reasonable argument could be advanced that Supreme Court justices have routinely resisted imposing more restrictive procedures for the selection of cases in order to respond easily to changing political and economic demands. Consequently, we think models of the certiorari decision could profit greatly from consideration of preferences and demands of outside interested parties as revealed through *amicus* participation.

Statistical Analysis

Our primary hypothesis is that the greater the number of *amicus curiae* briefs

filed for a given case—either in favor of, or in opposition to, certiorari—the greater the likelihood that the case will be granted a writ of certiorari. We test this hypothesis using data we have collected on all paid cases in which the Supreme Court granted or denied a writ of certiorari or affirmed, dismissed, reversed, or noted probable jurisdiction on a writ of appeal during the 1982 term ($N = 2,061$).¹¹ Of these 2,061 cases, 1,906 involved petitions for writs of certiorari, and the rest were decided on appeal. We confine our analysis to petitions for writs of certiorari, of which 145 (7.6%) succeeded. The dependent variable is whether or not the Supreme Court granted certiorari and the parties submitted briefs on the merits.¹²

A simple bivariate analysis of decisions on certiorari and filing of amicus briefs indicates considerable preliminary support for our main hypothesis. At least one brief was filed in 148 of the 1,906 petitions (108 cases with briefs in support only, 29 cases with briefs in opposition only, and 11 with briefs both for and against), and the Court granted certiorari in 54, or 36%, of these cases. In contrast, when no one filed an amicus brief on a petition, the Court granted only 5% of the cases.

We think that the relationship between the decision on a petition for certiorari and the filing of amicus briefs is much more complicated than the simple, bivariate association indicates. In particular, we expect that most organizations, ever mindful of the need to sustain the support of members, attempt to demonstrate the effectiveness of their activities in litigation and as a result are more likely to prepare and file briefs for those cases having a reasonable chance of acceptance. Previous research indicates that cases are most likely to be selected for plenary review when real conflict is present, when the ideological outcome in the lower court is at odds with the preferences of a majority of the justices, and when the solicitor

general is the petitioner. Accordingly, we view them as important variables to control.

Our data, like those in other studies, demonstrate a substantial connection between a positive decision on certiorari on the one hand and the presence of the United States as a petitioner or of real conflict or ideological outcome in the lower court on the other. Of the 1,906 petitions for certiorari in our sample from the 1982 term, 109 (5.7%) involved cases with "real" conflict,¹³ and of those 109, 76 (70%) were granted certiorari. For information on alleged and "real" conflicts in our sample, we have relied on a series of special issues of the *New York University Law Review* comprising the New York University Supreme Court Project (1984a, 1984b; see also Estreicher and Sexton 1986). The staff of the *New York University Law Review* made judgments for each of the 2,061 cases on whether the petitioner called attention to a conflict between or among courts and whether the alleged conflict was in fact a "square" or "real" conflict. A conflict, according to Estreicher and Sexton, "occurs when two or more courts of last resort—federal courts of appeals or state courts of last resort—take contrary positions on the same legal issue. We term this as a *square* conflict" (1986, 54). From everything we can discover, the staff of the New York University Supreme Court Project seems to have executed the research with extraordinary care and diligence.¹⁴ For the purposes of this article, we have created two variables from that source: alleged conflict (whether the petitioner claimed conflict between two or more circuits, two or more state supreme courts, two or more federal or state courts, or the lower court and precedents of the Supreme Court), and "real" conflict (whether or not the staff of the *New York University Law Review* concluded that conflicts of the sort alleged were present). Further, we have coded the number of alleged and real

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conflicts present in each.¹⁵

In addition to the Court's propensity to review conflictual cases, we anticipate that justices are most likely to hear those cases they would like to reverse on ideological grounds. For each of the 1,906 cases in our sample, we have used *United States Law Week* and *Briefs and Records* to make a judgment about the ideological direction of the decision in the court below—that is, whether the lower court decided the case in a liberal, conservative, or an ideologically indeterminable or ideologically irrelevant fashion.¹⁶ Clear liberal or conservative assessments were made in 1,773 of the 1,906 certiorari cases. The outcome at the lower court on these cases was predominantly conservative, with just under 40% being decided in a liberal direction. Yet these liberally decided cases were much more likely to be granted plenary review. Only 3.8% of the cases decided conservatively were granted certiorari whereas 16.1% of the liberal cases reached the plenary stage. And the presence of the U.S. government as petitioner also appears to be an excellent indicator of certiorari. Among the cases in our 1982 sample a writ of certiorari was granted in two-thirds of the cases petitioned by the United States.

Because amicus activity tends to coincide with factors that enhance a case's chances for plenary treatment, the independent effects of amicus activity can be identified only after carefully controlling these effects. We implement these controls in the following probability model. We specify the probability model that the Court grants a writ of certiorari on the i th case in the paid docket as

$$\Pr(\text{Cert}_i) = \beta_0 + \beta_1 x_{i1} + \dots + \beta_{10} x_{i10} + e_i$$

where x_1 through x_{10} are coded respectively as follows:

1. $x_{i1} = 1$ if the federal government is

petitioning party,
0 otherwise

2. $x_{i2} = 1$ if the appellate court reversed a lower court decision,
0 otherwise
3. $x_{i3} = 1$ if conflict was *alleged* by the petitioning attorney in one or more of the following situations:
 - a. conflict between two state supreme courts
 - b. conflicts between two federal circuit courts
 - c. conflict with U.S. Supreme Court precedent
 - d. conflict between a state court and a federal court,
 0 if no alleged conflict or more than one alleged conflict
4. $x_{i4} = 1$ if *real* or *square* conflict occurred in one or more of situations 3a-d,
0 otherwise
5. $x_{i5} = 1$ if the case concerned civil liberties,
0 otherwise
6. $x_{i6} = 1$ if the court's decision immediately below was ideologically conservative,
0 if the lower court decided liberally
7. $x_{i7} = 1$ if one amicus curiae brief was filed in support of certiorari,
0 if more than one brief or no briefs were filed
8. $x_{i8} = 1$ if two or three amicus briefs were filed in support of certiorari,
0 otherwise
9. $x_{i9} = 1$ if four or more amicus curiae briefs were filed in *support* of certiorari,
0 otherwise
10. $x_{i10} = 1$ if one or more amicus curiae briefs were filed in *opposition* to certiorari,
0 otherwise

Table 1. Probit Coefficients for Certiorari Model

Variable	Coefficient	t-Statistic
Intercept	-2.14	-14.96
Solicitor general	2.16	8.62
Disagreement among lower courts	.29	2.26
Alleged conflicts	.28	3.58
Actual conflicts	1.44	10.49
Issue area	.09	.63
Ideological direction in court below	-.71	-5.16
One amicus curiae brief in support of certiorari	.81	3.94
Two or three amicus curiae briefs in support of certiorari	1.42	5.01
More than three amicus curiae briefs in support of certiorari	1.74	5.21
One or more amicus curiae briefs in opposition to certiorari	.36	2.02

Note: $N = 1,771$ (141 grants, 1,630 denials); $-2 \log$ likelihood ratio = 1970.1; pseudo R-squared = .80.

We amassed information for variables 1, 2, 5, and 7-10 from *Briefs and Records of the United States Supreme Court* on microfiche and drew the other variables from issues of the *New York University Law Review* and *United States Law Week. Law Week*.

Because of the nonlinear form of probability models, we estimated the coefficients in the model through a maximum likelihood probit procedure. The estimated coefficients and their t-statistics appear in Table 1. The number of cases in Table 1 is based on all 1,906 certiorari petitions in our 1982 sample but reflects the loss of 135 cases due to missing data. We excluded a total of 133 of the 135 cases because the ideological direction of the lower court's decision could not be ascertained.

Overall, the model as defined fits the data very well. Taken together, the coefficients for the independent variables provide a significant improvement in explanatory power over the intercept alone. The probability of a chi-squared value of 1,970.1 (minus twice the log likelihood ratio distributed as chi-square with 10 degrees of freedom) occurring by chance is virtually zero. Furthermore, the pseudo R-squared value, which takes on a value of one when the full model is a perfect

predictor and a value of zero when the full model predicts no better than the intercept alone, is relatively large. Among the individual coefficients, only one does not attain statistical significance at a reasonable level of confidence (.05). The Court, it appears, is little inclined to hear one particular type of case over another—all else being equal.¹⁷ This finding, of course, runs counter to the conventional wisdom.

The predictive accuracy of the model is also quite good in terms of distinguishing between the cases the Supreme Court will select for further review and those on which it will deny certiorari. Of course we need no statistical model at all to predict with great accuracy the percentage of cases that the justices will choose for plenary treatment. One need only predict that the Court will deny certiorari in all cases in order to be correct 92% of the time for our 1982 sample; the Supreme Court granted certiorari in 141 of the 1,771 cases in the probit analysis. Accordingly, we must assess the predictive accuracy of our model against this baseline of 92%.

In order to compare the model's predictions against this baseline, we chose an estimated probability of .31 as a cutoff for predicting whether or not the Supreme Court would grant certiorari. Cases with an estimated probability of certiorari

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being granted at or above .31 were predicted to be given full treatment, and denial was predicted for cases with probabilities below .31. We chose this cutoff because it yielded a prediction of a grant of certiorari in 141, the actual number of cases in the probit analysis found over for plenary treatment by the Supreme Court.

Of the 141 cases identified by the model as most likely to be accepted for plenary review, 93 (66%) were in fact granted certiorari. And of the 1,630 cases identified by the model as least likely to be granted certiorari, 1,581 (97%) were in fact not granted certiorari. Thus a total of 97 errors in prediction resulted from the full model, as compared to 141 without a model. Compared with other models of certiorari, we think a 31% reduction in prediction errors is excellent, although there is obviously much room for improvement.

Most importantly, however, our statistical results indicate strong support for our primary hypothesis. The coefficients for the three amicus curiae variables provide strong support for the proposition that justices pay close attention to the demands of outside parties when making certiorari decisions. Not only does one brief in favor of certiorari significantly improve the chances of a case being accepted, but two, three, and four briefs improve the chances even more. Given our efforts to control other important forces in the certiorari process, we are impressed with the striking significance of this result. The coefficient estimates for briefs in support of certiorari are three to five times larger than their standard errors for all three variables. Thus it is highly unlikely that these large effects are random occurrences.¹⁸

Most of the cases in our full sample where an amicus brief was filed prior to the certiorari decision had only one brief filed, but a noteworthy number of cases did attract multiple briefs. Single briefs were filed in support of certiorari in 71

cases, but two or three briefs were filed in 28 cases, and more than four briefs were filed in 20 different cases. The largest number of amicus briefs filed prior to certiorari in any one case was nine. In general, the more briefs filed in favor of certiorari on any given case, the better the chances for plenary review. It must be noted, however, that the marginal increment in the probability of certiorari being granted decreased with additional briefs.¹⁹

Importantly, briefs amicus curiae in *opposition* to a petition for certiorari significantly *increase*—rather than decrease—the likelihood that the Supreme Court will grant a review. This result is not nearly so striking as that for briefs in support of certiorari, but the chance of observing a coefficient as large or larger than that reported in Table 1 purely by accident is less than 1 in 20. Further, we note that the coefficient for briefs in opposition is larger than that for reversal between the lower courts. In any event, this result further supports our theory that an amicus brief at the agenda-setting stage is a signal by organized interests to the Supreme Court about the practical importance of a case. The direction of the statistical result suggests that what matters most is the presence or absence of an amicus brief *per se*, not the direction of the substantive arguments presented. If justices were strongly influenced by the contents of the briefs themselves, it is quite unlikely that briefs in opposition to certiorari would significantly increase the likelihood of certiorari being granted. Briefs amicus curiae in opposition, plainly and simply, pique the Court's interest in a case.

The amicus curiae briefs on our sample were filed by an extremely diverse collection of entities, including corporations, labor unions, professional and trade associations, ideological and single-issue membership groups, religious organizations, racial and ethnic groups, individuals, and units of local, state, and federal government (Caldeira and Wright 1988).

While we have not yet had the opportunity to analyze possible variations in the significance of briefs from all of these different types of organizations, we have isolated one important set of amicus briefs—those filed by the U.S. solicitor general. Because of the prestige and experience of the solicitor general's office, its involvement often carries considerable influence in the Court's certiorari decisions. As a result, we were concerned that the significant effects of amicus activity observed in the initial estimations might be due largely to briefs from the solicitor general's office.

Further analysis, however, indicated that this concern was unwarranted. In our sample of 1,906 cases, the solicitor general filed amicus briefs prior to certiorari in only 18 instances, not enough to exert a systematic and significant effect by itself on the Court's certiorari decisions during the 1982 term. Even with these cases controlled, the statistical impact of the remaining amicus briefs was essentially unchanged from the estimates reported in Table 1. Thus, even though we cannot yet say which groups or which types of groups have the greatest impact on certiorari decisions through amicus activity, we can state with a considerable degree of confidence that amicus curiae briefs from nonfederal entities have a strong and significant impact on the Court's propensity to grant certiorari.

Amicus activity, although a powerful predictor of certiorari, is one among several strong predictors in the model. One of the most important variables is the presence of actual conflict between circuit courts, between state supreme courts, between federal courts and state courts or conflict with Supreme Court precedent. Whenever actual conflict was present, the likelihood that certiorari was granted jumped dramatically. Given the previous research on the importance of conflict for setting the Court's agenda, a strong effect for conflict was expected but

perhaps not quite as strong as found here.²⁰ More interestingly, though, *allegations* of conflict are also important for predicting certiorari, and the more allegations, the better the chances that the Court will accept a case. Evidently, the best advice to petitioning attorneys is to allege as many conflicts as possible. There is some evidence that attorneys understand this, for allegations of conflict were quite common during the 1982 term. At least one of the four types of conflict was alleged in nearly two-thirds of the cases considered for plenary review.

Closely related to conflict is the presence of a split decision, or reversal, between lower courts. In cases where a federal circuit court has overturned the decision of a federal district court, for example, the probability that certiorari will be granted is significantly higher. While not one of the strongest predictors in the model, a dispute between lower courts nevertheless is a factor that does distinguish a case and nontrivially increases its chances for further review.

The Court's attention to conflict and to disputes in the lower court underscores the justices' institutional concern with resolving conflicts of opinion on cases of wide importance. Yet, as we have hypothesized, justices appear also to have political agendas of their own, and these agendas are realized in part through the justices' ideological decisions about which cases to hear. Other things being equal, the Court is significantly more likely to hear cases that were decided liberally in the court immediately below. After Justice O'Connor's appointment in 1981, the Court had a decided, but certainly not extreme, conservative ideological orientation. Thus the clear tendency for the Court to select cases decided liberally in the court below affirms our theoretical expectation that justices' ideological predilections affect their decisions in much the same way that other elite political actors are motivated by their personal

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**Table 2. Estimated Probabilities of Certiorari for
Selected Values of the Independent Variables**

Solicitor General As Petitioner	Actual Conflict	Two or Three Amicus Briefs in Support of Certiorari	Estimated Probability
no	no	no	.01
no	no	yes	.35
no	yes	no	.33
no	yes	yes	.88
yes	no	no	.37
yes	no	yes	.96
yes	yes	no	.97
yes	yes	yes	— ^a

^aThe sample contained no actual cases with all three factors present.

ideological agendas.

It may well be that, as the motto of the solicitor general says, "The United States wins its point where justice is done its citizens in the courts"; but the fact that the Court's agenda is shaped by ideological considerations is keenly appreciated by the U.S. solicitor general's office, which itself has an ideological agenda to fulfill. Of the 56 cases during the 1982 term petitioned for review by the federal government, 73.4% were cases with liberal outcomes in the court immediately below. This ideological compatibility between the cases advocated by the federal government and selected by the Court suggests that the solicitor general should be relatively successful in convincing the Court to accept cases for full review, as indeed is the case. Of the parameters in our model, the size of the coefficient for the United States as a petitioner is second to none. The significance of a petition from the United States is even more impressive and interesting, given that much of the ideological basis of the Court's behavior has been controlled through the effect of the lower court's decision. Thus, ideological considerations aside, the solicitor general's expertise is evidently highly respected by the justices.

Having controlled for these various

influences, we now ask, In probabilistic terms, what is the marginal impact of amicus curiae briefs on the Supreme Court's decision to grant or deny certiorari? In Table 2 we display the estimated probabilities for cases, given the presence or absence of three significant forces: the United States as a petitioner, actual conflict, and amicus briefs. We computed these estimated probabilities using the coefficients reported in Table 1, together with mean values for the remaining independent variables.²¹

The estimated probability that the Supreme Court will grant certiorari is .01 when none of the forces is present—that is, when the United States is not a petitioner, when no actual conflict is apparent, and when no one has filed an amicus brief in support of certiorari. Most of the cases in our 1982 sample were of this type. If we hold all characteristics of this hypothetical case constant except for the addition of two or three amicus curiae briefs, the estimated probability of acceptance increases to .35. Alternatively, for the same basic type of case with no amicus curiae briefs but at least one actual conflict present, the probability of a grant goes up to .33. The addition of two or three amicus curiae briefs to a case with an actual conflict raises the predicted

probability of a grant of certiorari to .88. We predict similar changes as a result of the addition of two or three amicus briefs for a case in which the United States petitions for certiorari but which contains no actual conflict.

In general, then, amicus curiae briefs filed in support of the petition for certiorari increase the estimated probability that the Supreme Court will grant by a magnitude of .5 or .6, depending upon the characteristics of a particular case. It is true, of course, that the Court does not grant certiorari in all cases in which an organized interest happens to file an amicus brief in support. But for cases with a reasonable chance of acceptance in the first place, amicus curiae briefs can mark the difference between success and failure. For organized interests, according to our results in Table 2, the most cost-effective strategy is to file an amicus brief in cases in which *one* but not *both* of the two variables—United States as a petitioner and actual conflict—is present. When both are present, because the probability that the Supreme Court will grant certiorari is already at .96 or .97, the filing of an amicus brief can make little or no difference. Indeed, organized interests seem to recognize this point, for we found no cases in our 1982 sample in which amicus briefs, actual conflict, and the United States as a petitioner were all present.

Conclusions

We have proposed that U.S. Supreme Court justices attempt to select cases for plenary review with the greatest potential social, economic, or political significance consistent with their own ideological predispositions. We assumed that the potential significance of Supreme Court decision making is proportional to the demand for adjudication among interested parties and that justices acquire information about demand, or signifi-

cance, by observing the extent of amicus curiae participation. Thus our primary hypothesis was that one or more amicus curiae briefs advocating or opposing certiorari would increase the likelihood that certiorari was granted.

The data we have collected from the 1982 term offer strong support for this hypothesis and, correspondingly, for our theoretical perspective on the role of interest groups in Supreme Court decision making. When a case involves real conflict or when the federal government is petitioner, the addition of just one amicus curiae brief in support of certiorari increases the likelihood of plenary review by 40%–50%. Without question, then, interested parties can have a significant and positive impact on the Court's agenda by participating as amici curiae prior to the Court's decision on certiorari or jurisdiction.

We believe this result is important both for understanding judicial behavior and for understanding the strategies and influence of organized interests in national politics. The U.S. Supreme Court, in our estimation, is quite responsive to the demands and preferences of organized interests when choosing its plenary docket. In this regard, the Supreme Court is very much a representative institution, and we think it both useful and important to analyze the Court from this perspective. We view the Court's openness to outside demands when choosing its plenary docket as not only necessary for a smoothly functioning representative polity but also as a natural consequence of rational political decision making. To be sure, how well or whether judicial responsiveness to the demands of organized interests comports with more traditional theories of decision making is a matter of considerable controversy and is best left for fuller treatment elsewhere.

Organized interests are generally influential during the Court's agenda phase because they solve an informational prob-

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lem for the justices. Through participation as amici, organized interests effectively communicate to the justices information about the array of forces at play in the litigation, who is at risk, and the number and variety of parties regarding the litigation as significant. Obtained elsewhere, this information would surely be much more costly than as provided through amicus participation, if it could be obtained elsewhere at all. In this regard, organized interests function effectively as interest articulators, a result that generally adds stability to political systems.

In the near future we foresee several extensions of the present analysis. First, we are in the process of gathering information on sponsorship for the cases in our 1982 sample. We think sponsorship holds considerable promise as a predictor of case selection. Second, we are in the process of replicating, as much as possible, all of our variables for the 1968 term. It appears that the scope and intensity of interest group participation has changed considerably since that time, and we think it important and useful to document and explain this change and its consequences. Third, we intend to test the model presented, as well as further refinements of it, on the votes of the individual justices. For example, do some justices respond with greater favor to amicus briefs filed prior to the decision on certiorari? Do some evince greater sensibility to the presence of conflict between or among courts? Finally, we are preparing to survey attorneys for, and representatives of, organized groups in order to understand better the strategies and perceived costs and benefits associated with the role of amicus curiae, as well as other forms of participation in litigation. If amicus participation is as important as our statistical analysis suggests, then we should find considerable corroborating evidence from the attorneys with first-hand experience of how the Court operates.

Notes

We appreciate the intrepid research assistance of Peter Berryman, Kevin McGuire, John Felice, Jodi Govern, and William Johnson. The staff of the Manuscript Division at the Library of Congress helped us to find our way around the papers of Chief Justice Earl Warren and Justice William Brennan. Grant no. SES 86-07291 from the Program in Law and Social Science of the National Science Foundation made our research possible. We thank Lawrence Baum, Karen O'Connor, and Lee Epstein for advice and counsel at various stages of this research.

1. We use the terms *organized interest*, *interest groups*, *organizations*, and *groups* more or less interchangeably, having in mind a very broad conception. It includes under the umbrella "not only associations such as trade associations, unions, professional associations, and environmental groups that have individuals or organizations as members, but also politically active organizations such as universities, hospitals, public interest law firms, and especially, corporations that have no members in the ordinary sense" (Schlozman and Tierney 1986, 10). We employ *organized interest* to denote "the wide variety of organizations that seek joint ends through political action" (Schlozman and Tierney 1986, 11). With rare exceptions, *organized interests*, not individuals, participate as amici curiae.

2. In light of the extraordinary amount of labor entailed in gathering the data needed to test our model, it is simply unrealistic to collect data for more than one or two terms of the Court. We have no reason to believe that the 1982 term was peculiar in any respect. For an explanation of why we chose the 1982 term, see n. 11.

3. We plan, in the future, to investigate the impact of "sponsorship" and direction participation by organized interests on the Court's decision to grant or deny certiorari. Fragmentary evidence suggests the importance of this strategy at the gatekeeping stage (see, e.g., Sorauf 1976).

4. It is quite likely, given our theoretical model, that briefs amicus curiae will exert a greater impact at the gatekeeping stage than on the merits. But of course we do not test the differences between the two phases of decision here. For some evidence of the effects of amicus briefs on the merits, see Ivers and O'Connor 1987.

5. We recognize as well that a justice may not always vote to review a case even when he or she could muster four votes for certiorari (Brenner 1979). This is true because even though a majority decision on the merits requires the agreement of five justices, only four justices need consent to grant a writ of certiorari or to note probable jurisdiction on an appeal. So if a justice believes that a fifth favorable vote on certiorari or appeal is unlikely on the merits, then bringing the case forward for review is,

obviously, counterproductive. Linzer illustrates our point well: "There are surely . . . cases in which a Justice votes to deny certiorari despite his strong dislike of the decision below because he is afraid that there is a majority against him that would affirm on the merits. . . . A former Supreme Court clerk has privately retold how his Justice 'taught a lesson' to a new colleague. The experienced Justice had voted to deny certiorari in a case that both he and the new man found dreadfully decided below, despite three votes to grant. The new man sent a note pointing out that with the elder Justice's vote there would be the four votes needed for review, to which the elder asked back: 'But where will you get the fifth?' " (Linzer 1979, 1303, n. 539).

6. Of course we do not mean to imply that the quality of the legal arguments made in a brief is irrelevant.

7. We base this statement on our sampling of docket books and memoranda in the Papers of Mr. Justice Brennan and Chief Justice Earl Warren, Manuscript Division, Library of Congress. Nowadays, the justices rely heavily on law clerks to do the screening of petitions for certiorari and jurisdictional statements. During the period for which we present data, Chief Justice Burger and Justices Powell, O'Connor, Blackmun, White, and Rehnquist participated in the so-called "pool" for certiorari. Justices Brennan, Marshall, and Stevens choose to screen cases within their individual offices. The reliance on the help of clerks does not at all imply that the justices do not look for cues or signals in the cases. For the clerks, in preparing memoranda on petitions and statements, clearly attempt to ferret out telltale signs for their bosses (see, e.g., Provine 1980, 26). Indeed, the thumbnail sketches the law clerks provide probably reinforce the tendency to scan for a handful of facets about a case.

8. More recently, in *Anderson v. Ohio*, Justice Brennan's clerk wrote to him about a request to expedite consideration of the petition for certiorari. First of all, Brennan's clerk noted the presence of three amicus briefs for the states of Maryland, Maine, and New Mexico. He then went on to write about the situations as described in the briefs amicus curiae: "Ohio is not the only state whose early filing laws have been struck down. The laws in Maine, Maryland, and New Mexico have also been ruled unconstitutional. The decisions in Maryland, Kentucky, and Maine have been appealed. The First and Fourth Circuits have apparently established schedules and may already have heard their cases. The Sixth Circuit had not heard this case as of a week ago" (no. 80-182).

9. For example, in *Kramer v. Union Free School District No. 15 et al.*, no. 258, 1968 term, one of Chief Justice Warren's clerks wrote his memorandum around a dissenting opinion by Judge Weinstein in a three-judge district court. The issue there was

whether the equal protection clause prohibited New York's restriction of the right to vote in school district elections to those who own or lease property or who have children attending the school. The clerk devoted less than a page to a discussion of the majority's view and reasoning and over two pages to Judge Weinstein's dissent. He concluded: "I think appellant has presented a close question on the merits which should be considered by this [Court]. Judge Weinstein's opinion makes a strong case for appellant's position and, more significantly, for the importance of the issues he has raised" (PJM to Earl Warren, 12 September 1968).

10. Scholars are maddeningly inconsistent about which issues might interest the Court more and how to define the various categories.

11. In practice this means that we have some cases with 1981 docket numbers ($N = 387$), but most of course have 1982 numbers ($N = 1,674$). At the end of each term, the Supreme Court carries over several hundred cases. Some of these await a grant or denial of certiorari, others oral argument and a decision on the merits. Regardless of which term one chooses, the sample will include a mixture of cases originally filed in different years. In selecting our sample of cases, we had three primary considerations in mind. First of all, we wished to collect data for cases in which the New York University Supreme Court Project had made assessments about the presence or absence of alleged and actual conflicts between or among courts. To execute a study of these conflicts would have required resources far beyond our command. Second, we wanted to pull together information for one full term of the Court. Third, the term chosen had to be recent, but not too recent. We plan to use our list of amicus participants to generate a partial list of organizations for a later survey, so a more recent term helps to maximize the rate of response. But since we rely on the *Briefs and Records of the United States Supreme Court*, which runs two or three years behind the *United States Reports*, we could not choose a term as recent as we otherwise might.

In *Redefining the Supreme Court's Role* (1986), Estreicher and Sexton tell of the New York University Project's sample: "It examines all of the cases that the Supreme Court agreed to hear (201) and all of the 'paid' cases that the Court refused to hear (1,860) during the 1982 Term. . . . We chose to study the cases the Court decided to hear during the Term, rather than the cases it actually heard, because we wanted to replicate as nearly as possible the case selection process that occurs during a particular Term" (p. 76).

12. We restrict the analysis to petitions for certiorari because, as a formal matter, the Supreme Court treats appeals somewhat differently. We did a separate probit for jurisdictional statements and found that the Court indeed makes its decisions on

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appeals in a manner significantly different from those on certiorari.

13. This number is very close to Feeney's (1975) reported average of 119 cases with conflict during the 1971 and 1972 terms, and to Ulmer's (1984) reported average of 113 cases of conflict.

14. Here is more detailed description of the manner in which the Supreme Court Project conducted its business (Estreicher and Sexton 1986): "Using the criteria derived from our view of the Supreme Court's role, we have examined each of the granted cases. . . . We examined the 201 granted cases ourselves. Each one is discussed in the project as reported in the *NYU Law Review*. Twenty-two *Law Review* editors and four research assistants examined the 1,860 cases denied review. The students determined whether the denied cases presented an intercourt conflict of clash with Supreme Court precedent. . . . Our position on each of the granted cases and the students' position on each of the denied cases—2,061 in all—are summarized either in a textual discussion or in an appendix entry in that journal" (p. 78).

15. We have considered the effects of several other variables, some of them mentioned in previous literature: dissent in the court below and the allegation of a constitutional issue. Neither of these variables exerts a significant impact on the Supreme Court's decision to grant or deny a writ of certiorari.

16. Anyone who has ever attempted to classify judicial decisions according to the ideological direction of the outcome will readily attest to the difficulty of this task. In many—perhaps most—of the cases, we found the ideological direction of the outcome clear and easily coded. In a certain percentage of the cases, we simply could not make a classification. For the most part we have opted for a fairly conventional coding procedure along the lines Spaeth, Goldman, and others have articulated over the years. Some have argued that scholars should code the outcome of a case according to what kinds of parties win or lose, for example, for or against labor unions or criminal defendants. We tried to blend together the best of both approaches.

17. We recast issue area in several different forms. For instance, we included dummies for criminal, civil liberties, and economics cases; and none was significant. Regardless of how we coded issue area, we turned up nothing of significance.

18. Naturally, organizations file briefs amicus curiae for many of the same reasons as the Supreme Court grants petitions for certiorari. One might, therefore, suspect simultaneity in our equation. But the variables in our model reflect the quality of a case as a legal vehicle, not the social, political, or economic importance of the issue to an organization. In our theoretical model, briefs indicate a decision on the part of an organization to signal to the Supreme Court the importance to it of a case. To test for the possibility that the filing of briefs amicus

curiae on certiorari is a function of the same variables as is the decision to grant or deny, we regressed the number of briefs filed on a case on all of the independent variables in the model presented in Table 1. The results differ in important respects from those reported in Table 1. Interestingly, although the presence of the solicitor general as a petitioner exercises an important influence on the decision to grant or deny, it does not exert an impact on whether an organization filed a brief amicus curiae. We believe, in sum, that the decision to file an amicus curiae is independent of the decision to grant or deny certiorari.

19. We entered amicus activity into the model in three different ways, in addition to the results reported in Table 1. One specification included a single dummy variable indicating the presence of one or more briefs or no briefs. A second specification included just the actual number of briefs filed, and the third specification included the total number of amici listed on all briefs for each case. All of these treatments yielded highly significant effects for amicus activity. We finally settled on the specification reported here because it illustrates both the added value of more briefs and the decreasing marginal increments in probability of additional briefs.

20. There are several possibilities for why our results for conflict appear stronger than those reported by Ulmer. First, and most obviously, the procedure for identifying square conflict in the variable we use is different from his procedure. And second, whereas Ulmer used discriminant analysis, we have relied on probit analysis. For a comparison of the two techniques, see Maddala 1983.

21. To compute each of the probabilities reported in Table 2, we used different means for the ideological outcome in the lower court, disagreement among lower courts, issue area, and amicus briefs in opposition. We computed each mean on a different subsample of cases corresponding to a particular combination of actual conflict, the United States as a petitioner, and amicus briefs.

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ORGANIZATIONAL MAINTENANCE AND THE RETENTION DECISION IN GROUPS

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Understanding why members leave or remain in groups has received little attention despite its fundamental importance for organizational maintenance. In this analysis, a theory of experiential search is proposed and applied to Common Cause. Group participation is conceptualized as a process by which imperfectly informed decision makers learn about the organizations they join. This framework makes quitting understandable and provides a link between the initial membership choice and follow-up decisions.

Organizational maintenance is a fact of life all group leaders confront. For the majority of interest group entrepreneurs, who depend on constituent dues as a prime funding source, maintenance dictates the need to keep members contributing (but see Walker 1983).¹ Even seemingly small drops in numbers—10% or 20% net of replacements—are viewed with great alarm; and the loss of long-time contributors is perceived as a threat to the entity's survival. The key to creating a successful organization can be summarized simply: entice potential members to join, keep attrition below the rate at which replacements can be found, and establish a core membership.

If this prescription is correct, it is curious that analysts of interest group membership focus almost exclusively on first-time joiners.² Most authors do not deal explicitly with the dynamics underlying the *retention* choice, that is, the decision by existing members to remain in the group and keep on contributing.³ Attracting members is fundamental for long-term organizational prosperity, but signing

them up in the first place is only half the battle. The conditional joining decisions on whether to stay in the organization are also crucial.

Interest group entrepreneurs encounter a dilemma: how to retain members for whom leaving may be an attractive option.⁴ The leaders focus on producing the selective incentives that the membership wants. They are also preoccupied with not antagonizing constituents, for fear they will cease contributing. They structure and operate the interest group so as to facilitate contributions (Moe 1980). How else, for example, can one explain the elaborate lengths to which many leaders will go to ensure the appearance of rank-and-file participation in the organizational decision-making process?

In the political arena, the retention problem should hit home hardest for public interest groups. Their leaders lack the occupational or industry bases that underlie so many private associations. They cannot draw on a "natural" membership, among whom either selective incentives are easily generated or coercion induces contributions. That people sign

up for public interest groups in the first place in light of the collective action dilemma—not to speak of retaining their organizational allegiance—is a phenomenon that has generated considerable scholarly interest (Berry 1977; Smith 1985).

Why, then, do members of an organization, especially a public interest group, choose to remain? Is this process indistinguishable from the original decision to join—whatever that may be? How are the two connected, if at all?

These issues will be explored by analyzing what is popularly considered the quintessential public interest group: Common Cause. In the fall of 1981, over 12 hundred Common Cause members were surveyed using a mail questionnaire. All respondents were queried about a wide variety of issues—membership and its benefits, personal attitudes, previous history in Common Cause, and future intentions, to name a few. A stratified design oversampled Common Cause-designated activists, who were defined as steering committee coordinators in congressional districts or coordinators or activators of telephone networks. They comprise 23% of the sample, a roughly sixfold overrepresentation. Since this stratification is based on characteristics that are exogenous to the retention decision, this sampling strategy has no impact on the hypothesis tests conducted in this analysis (Amemiya 1985; Hausman and Wise 1979). These data furnish a rare opportunity to explore the retention decision and to learn why people participate in political organizations despite all the obstacles to collective action.⁵

The first part of this analysis lays out the theoretical groundwork by specifying alternative conceptualizations of the membership renewal process.⁶ Despite frequent assertions in the literature that the study of organizational membership is theory-rich and data-poor (e.g., Arnold 1982; Shaiko 1986), it is argued here that the available models require further

development. Once the proper theoretical underpinnings have been laid, the focus shifts to the empirical world. Unlike prior descriptive work on the original membership decision by authors who fail to test hypotheses, competing retention models are made operational and tested to determine whether it is possible to make sense of members' decisions to stay in or leave organizations.

Theoretical Perspectives: Joining, Remaining, and Experiential Search

Although this study looks primarily at renewal decisions, the initial reason for joining is the logical place to start. In the first place, virtually all of the available theoretical research focuses on the original membership calculus. Second, an analysis of the initial choice furnishes a vantage point from which to consider the renewal decision, and vice versa. The challenge is to integrate perspectives on both initial contributions and retention decisions.

Parenthetically, the viewpoint taken in this analysis and the tradition of research in which it is written is decision-theoretic rather than game-theoretic, despite the latter's importance for studying collective action (e.g., Axelrod 1984; Hardin 1982). While game theory furnishes many insights into how actors behave strategically in small-number situations, it is inappropriate for explaining why individuals contribute—and keep contributing—to large groups. Decision-theoretic models in which individuals are playing against nature are more suitable for understanding the retention choice.

Most theorists working in this research tradition (e.g., Clark and Wilson 1961; Moe 1980; Wilson 1973) typically identify a number of benefit types: selective, solidary, and purposive. *Selective*

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benefits are tangible returns that have monetary values and are derived from contributions. They either may be divisible, private rewards or may emanate from members' impacts on the level of collective goods provided to everyone. *Solidary* benefits stem from associational interactions, while *purposive* benefits are intangible rewards garnered from contributing to the group because of its stated goals.⁷ These theorists then examine which of these benefit types are important and why.

In the mid-1960s, Mancur Olson (1965) revolutionized perceptions about why citizens join organized groups. His seminal contribution details the difficulties associated with collective action. Individuals join large groups, he argues, because the value of the available selective benefits exceeds the costs of membership.

Olson assumes that individuals have full information, are only interested in economic rewards, and maximize without error. In large groups, this implies that only dues levels and divisible selective benefits will be relevant. Political activity is simply a by-product of narrowly self-interested behavior. Given this framework, it is obvious that the conditional membership decision should follow the same cost-benefit analysis. Since in general not much will change from one contribution period to the next, particularly given the assumption of perfect information, little explicable organizational attrition is possible.

Those challenging Olson's theory have taken two principal tacts. Some (e.g., Clark and Wilson 1961; Moe 1980; Wilson 1973) emphasize that purposive and solidary incentives are important, along with selective returns, in the decision calculus.

Individuals who possess perfect information but are not satisfied by selective incentives alone may join because the potential purposive and solidary payoffs

push them over the threshold where benefits exceed costs. People derive consumption benefits from interpersonal interactions and the purposive statements their contributions make. The multiplicity of incentives can be easily incorporated into the Olsonian framework, and the same inferences about the conditional membership decision still hold. This argument is intuitively reasonable; Olson himself recognizes the potential importance of nonmonetary returns but ignores them for reasons of parsimony.

Another critique is that the key problem lies less with the breadth of incentives than with the assumption of perfect information. Moe (1980) maintains that potential group members do not possess perfect information. Consequently, some decision makers miscalculate their own contributions to the provision of collective goods and hence the level of selective incentives the group offers. A subset of the population mistakenly thinks of itself as highly influential and incorporates its allegedly substantial contributions to the provision of collective goods into its membership calculi. Individuals whose perceptions of their efficacy put them at the upper end of the population distribution join in disproportionate numbers. The assumption seems to be that once these contributors miscalculate, they keep repeating the same mistake over and over. Again, organizational membership ought to be stable, and there should be little explicable attrition.

The relaxation of the perfect information assumption is eminently reasonable and extremely important. But precisely how imperfect information affects contributions needs to be rethought. It ought to influence more than simply individuals' initial estimates of the benefits they derive from contributing to collective goods.⁸ The conclusion that many members join organizations only because they make mistakes is unsettling—especially because educated individuals tend to join in

greater proportions. Implying that those committing errors keep contributing for years without revising their prior beliefs is even more difficult to believe. As in the two models elaborated above, the conditional membership decision replicates the initial choice to join. While there may be some exogenous forces prompting a few individuals to leave, the outcomes generally ought to be the same under all three models.

A more reasonable supposition is that the decision to join makes sense as a strategy by individuals who recognize their lack of knowledge. Members join groups to learn about them, and as they acquire knowledge, some can be expected to leave. The politics of *experiential search* offer a superior perspective for understanding retention and integrating it with the initial membership choice.

A Theory of Experiential Search

Organizational membership can be conceptualized as a search process. Citizens lack complete information about all of the alternative groups they might join and the associated costs and benefits. They presumably would like to discover an organization or organizations that will give them enough returns relative to costs—regardless of the types of benefits they seek—that they will be content to remain.

Experiential search can be distinguished from the economics literature on sequential search (for an excellent recent overview, see Mortensen 1986). In the latter, individuals typically search sequentially until they meet their reservation wage or price, where the expected marginal cost of an additional iteration equals the expected marginal return. In these models, workers or consumers gather information before taking a job or purchasing a product.

There are some models (e.g., Burdett 1978; Wilde 1979) that permit on-the-job

or experiential learning by workers within a sequential search framework. These perspectives are much closer in spirit to the experiential search theory propounded here, but there are still important differences. For example, by and large, workers must search for a job, while there is no similar compulsion to join a public interest group.

In looking for an organization to fill their needs, individual decision makers with fixed preferences have three options: they can (1) conclude that given the problems of obtaining information and the costs of membership, it is advisable to give up; (2) search over alternative associations without contributing; or (3) join an organization to learn whether membership is worthwhile. This final option, learning through exposure, can have a number of facets—developing an understanding about how a group functions, whether it is effective in achieving its goals, and whether its outputs are in line with one's preferences, to name a few. Given the low monetary cost of joining numerous voluntary associations, particularly public interest groups, many should opt for experiential learning because it is an efficient information-gathering technique.⁹

A factor predisposing searchers to join an organization is that many attributes of membership are only observable by participation. These are specific characteristics, while those that can be observed without joining are general characteristics. These two types can be thought of as opposite poles on a continuum that reflects the difficulty of acquiring information without making a commitment; most informational traits combine elements of both.

Almost every factor incorporated into the joining (or retention) calculus can be described, at least partially, as a specific characteristic. The only purely general feature of organizational membership is the dues level. If the remaining factors

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were ranked from more to less general, they might roughly be ranked as follows: costs other than dues levels, purposive benefits, divisible benefits, solidary benefits, and collective benefits.

When the costs of evaluating specific qualities are relatively low, prospective contributors will tend, *ceteris paribus*, to join, accumulate knowledge, and then decide either to quit or to stay and learn more. Since a reasonable inference is that one accumulates knowledge more and more slowly over time, the expected rate of dropping out should diminish temporally. Specific characteristics should also be more important for newcomers than for veterans.

In the organizational context, first-time joiners will have imperfect information about costs other than dues. How can the costs of phone calls asking for assistance—writing legislators, contacting other members, and so on—or appeals for monetary contributions in excess of dues be established without error in advance? As contributors spend time in the group, they will develop a growing awareness of the true price of membership and behave accordingly.

Along these same lines, the value of purposive benefits will be increasingly evident with experience. Although members will probably have some initial idea about what the group stands for, they will gradually learn whether it represents those things that provide them with consumption benefits.

Before they join, potential members will also lack complete information about the quality of the divisible benefits furnished to participants. Accurately assessing the full value of a groups' offerings without consuming them regularly is all but impossible; membership offers an opportunity to learn about their utility firsthand.

This inference has an important implication: Suppose that before engaging in experiential search, small contributors

deduce that their donations will have no impact on the level of collective goods provided. Presume too that purposive and solidary incentives play no role in eliciting contributions. Even in this extreme case where lack of knowledge is irrelevant for an individual's valuation of collective, purposive, or solidary benefits, imperfect information *still* ought to be a major factor in members' decision calculi.

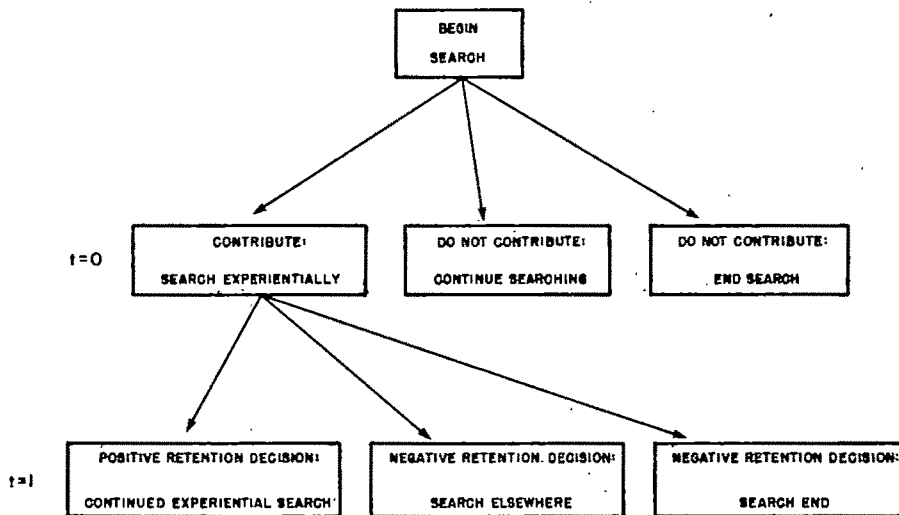
If contributors are motivated by solidary returns, it is unlikely that they will have a full idea of the value of these benefits either. Only immersion in day-to-day organizational operations will permit them to see whether the interpersonal interactions are sufficiently rewarding.

Experiential search should be especially germane for learning about collective goods. Member education should have several elements. Contributors ought to become more cognizant of what collective benefits their organization actually proffers. Furthermore, those who believe that their contributions to the provision of collective goods are nontrivial should learn about the value of these goods and the insignificance of their contributions. Members who donate resources to have an impact on the magnitude of public goods provided—and not because they gain purposive rewards from contributing to collective goods—should leave the group over time, *ceteris paribus*.

To reiterate, citizens with perfect information (or those who have imperfect information but never learn) will join those organizations that have the highest net benefits for them. While changes in exogenous conditions might lead some to exit, the strong presumption is, Once a participant, always a participant.¹⁰ But when the assumption of imperfect information is embedded within the experiential search framework, quitting becomes comprehensible.

Group membership might usefully be conceptualized as a decision-theoretic process in which members join an organi-

Figure 1. Organizational Membership as Experiential Search



zation and then reevaluate their decision in accordance with what they have learned about the costs and benefits of participating (see Figure 1). Each membership renewal period is another stage in this decision process. And each time, the contributor has better information. Learning about specific characteristics continues indefinitely, but the amount of additional information accumulated through experiential search diminishes over time. Members weed themselves out, especially during their first few years in an organization. Information updating can reinforce a propensity to remain in the group for another contribution period or lead to the conclusion that membership is less valuable than foregone opportunities. Those abdicating membership either become politically inactive or continue searching for an alternative that makes participation worthwhile. For those who stay in the organization, over-time learning should become a less and less salient factor in the decision whether to stay or go.

Experiential Search Theory: An Empirical Test

The experiential search theory implies that withdrawal is a rational response by imperfectly informed decision makers. Unlike the previous three models, under this formulation members' cost-benefit calculations should change substantially over time. This expectation has three implications that can be tested with cross-sectional data on Common Cause membership:¹¹ (1) a model of the conditional membership decision should uncover fairly strong relationships between costs and benefits, even for those individuals who have previously elected to join; (2) the longer contributors have been in the organization, the less likely they ought to be to leave, because the likely increment in knowledge declines over time; and (3) the impact of those factors that guide the retention calculus, particularly highly specific characteristics, ought to be stronger for relative newcomers.

The true test of this framework is how

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well these theoretical assertions explain behavior in the empirical world. Too often students of organizational membership have been content to utilize a few descriptive tables—largely because they were analyzing the initial membership decision with data only about members. This research focuses on testable hypotheses about membership dynamics.

The empirical analysis of Common Cause renewal decisions unfolds in four steps. First, to provide the proper context, I outline the costs and benefits of contributing to the association. Second, I provide descriptive data about the reasons members say they initially joined, to establish a baseline for comparison with their subsequent decisions. Third, I examine whether those who belong to an organization learn over time, to provide side evidence about the assertions made earlier. Finally, I make models of the conditional membership process operational and directly test them to determine which is most empirically valid.

The Costs and Benefits of Common Cause Membership

Common Cause is the most prominent of the new wave of organizations associated with the burgeoning consumer movement of the late 1960s and early 1970s (for a detailed case study of Common Cause, see McFarland 1984). This feature alone makes it a natural candidate for study. But it is the leadership's strong dependence on its more than 200 thousand members that makes it especially appealing for analysis. The members are essential because of both their financial contributions and their activism at key points during the organization's external political battles. What, then, are the costs and benefits that motivate citizens to contribute to Common Cause?

Initially joining the association is a reasonably low-cost activity. Organizational dues are predictably moderate: \$20

per year. Members are fairly well-off—their median family income in 1981 fell in the \$25 to \$35 thousand range, about 50% higher than the national average.¹² A tenable assumption is that relatively well-heeled contributors can learn about Common Cause or other moderately priced organizations through experiential search.

These members should discover new information about the value of belonging. They learn that there are additional costs to participation. Members are solicited to give more financial support, and many comply. In fact, 63% of those surveyed reported contributing more than the required \$20. Despite this generosity, Common Cause, with a staff of less than one hundred, lacks the financial resources to compete on an even footing with private interest groups. Like public interest associations generally, it employs other instruments to make up for these shortfalls. One critical resource is energizing contributors to become politically involved. Members are asked to give of their time and become part of Common Cause's activist core. They are encouraged to immerse themselves in the association's sophisticated, congressionally based, grassroots network, as well as in state and local organizations.

It is difficult to be simply a passive contributor. Those preferring only to write checks will find themselves the targets of periodic mobilization efforts when battles over issues pinpointed by the leadership come to a head. Forty-three percent of all members in the survey (32% of the rank and file and 68% of the activists) said they had been contacted by Common Cause in the past year and asked to write to Congress about some issue.¹³

Contributors also have opportunities to learn about the complete gamut of organizational benefits. Although the stereotypical individual who joins Common Cause is characterized as a liberal do-gooder, the leadership obviously believes that divisible benefits are necessary to

keep members happy. A bimonthly magazine is distributed to association members as an enticement to contribute; perhaps the consumption of this and the other political information furnished is sufficient to elicit a minimal annual contribution.¹⁴

Some may perceive another private return from participation, namely, career advancement. Suggesting self-promotion as a motivation is antithetical to the high moral ground that Common Cause tries to occupy, but it is consistent with Olson's by-product theory. An unexpectedly high proportion of respondents—23%—replied that they had political aspirations. Thirty-five percent of those surveyed stated that they either had political aspirations or had at some time sought a party position, an elected office, or an appointed office. When broken down between rank and file and activists, the percentages are 32 and 45, respectively. This discovery suggests that the participation of many allegedly altruistic liberals may be motivated by the search for a springboard onto the political opportunity structure (Schlesinger 1966) or perhaps by a desire to be educated about liberal positions. If it does the trick, membership is renewed; if not, the aspirant moves on and continues to search.

Common Cause has been involved in many celebrated battles, generally associated with "good government" issues designed to provide a host of nonexclusive collective goods. The leadership selects its policy areas carefully to maintain membership loyalty. To gather information about constituent preferences, the staff annually polls contributors for their opinions about the Common Cause political agenda. Choosing those issues over which opinions are particularly homogeneous is considered especially important (McFarland 1984).

This decision-making strategy has several interesting implications. It implies

a belief that members either think they have an impact on the provision of collective goods or get considerable value from the purposive statement that their participation in Common Cause makes to the world. Assuming that the leadership's inference is correct, this conflict-minimizing strategy should also mitigate the impact on retention decisions of members' satisfaction or dissatisfaction over the association's political actions. If those in charge selected issues exclusively on other criteria, for instance, if they mistakenly viewed political action as a by-product that has no weight in participants' decision calculi, then contributors' evaluations of Common Cause's political efforts would be more germane in conditional membership decisions.

Common Cause's decentralized structure obviously provides ample opportunities for interpersonal interactions (see also McFarland 1984). Although much of this is a product of organizational weakness—lack of funds forces the staff to rely heavily upon its other major resource, an energetic membership—it may also be a strength when it comes to keeping contributors in the organization.

Why Join in the First Place?

Information on members' professed reasons for joining Common Cause cannot explain why people belong. There are no data on those electing not to sign up. However, contributors' beliefs about why they originally joined do furnish a vantage point from which the retention process can be understood. Are participants' reasons for staying or leaving consistent with their assessments of why they initially contributed? Or do other unrelated factors enter their calculations?

Using an open-ended format, members were asked why they joined. Their responses were coded according to the type of benefits they seemed to value most. The most intriguing result is that

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the vast majority of contributors believe that they got involved for purposive reasons (Table 1). Even employing an extraordinarily broad definition of selective benefits, 72% of respondents must be classified as professing that purposive

concerns stimulated them to sign up.¹⁵ These data are consistent with the experiential search argument that members know little about the specific costs and benefits of belonging when they initially join. General reformist tendencies were

Table 1. Members' Proclaimed Reasons for Joining Common Cause

Reason for Joining	Number of Respondents	Percentage
Purposive benefits		
Supports general goals, issues, or efforts	169	14.7
Keeps government honest and fair	162	14.1
Reforms, improves government; makes government more responsive	132	11.5
Combats special interests, lobbyists, PACs, corporations, big business	79	6.9
Supports public interest and the common good	71	6.2
Is a watchdog-investigatory group	53	4.6
Is a nonpartisan group	26	2.3
Protests political power and corruption	21	1.8
Supports in general (no issue content)	20	1.7
Is concerned about democracy	18	1.6
Is antigovernment or anti-political parties	17	1.5
Appeals to sense of civic duty	13	1.1
Addresses problems in society, government policies	11	1.0
Maintains checks and balances	10	0.9
Is unique and important	9	0.8
Needs financial support	8	0.7
Is a liberal and anticonservative group	6	0.5
Subtotal	825	71.9
Selective benefits (collective or divisible)		
Makes member effective	65	5.7
Provides political information	53	4.6
Addresses specific issue(s)	35	3.0
Offers a chance for personal political activity	32	2.8
Provides collective action necessary for change	28	2.4
Supplies leadership	26	2.3
Provides representation	18	1.6
Informs citizens and encourages participatory democracy	12	1.0
Offers a chance to be mobilized on issues	11	1.0
Is effective	9	0.8
Is an activist organization	5	0.4
Subtotal	294	25.6
Solidary benefits		
Family or friends belong	11	1.0
Joining is part of life style—social reasons	3	0.3
Subtotal	14	1.3
Miscellaneous	16	1.4
Total	1,149	100.2

Table 2. Organizational Experience and Knowledge about Group (%)

Years in Group	Percentage Correct					Mean Percentage Correct
	0	25	50	75	100	
1	34.1	31.7	26.8	4.9	2.4	27.5
2	34.9	23.3	30.2	8.1	3.5	30.5
3	20.4	18.4	28.6	25.5	7.1	45.2
4	9.6	19.3	22.9	42.2	6.0	53.9
5	7.3	15.9	35.4	30.5	11.0	55.5
6	12.1	13.6	28.8	30.3	15.2	55.7
7	5.4	14.7	28.7	31.0	20.2	61.4
8	2.5	13.6	25.9	42.0	16.0	63.9
9	4.9	10.7	29.1	32.0	23.3	64.6
10	3.4	11.2	16.9	47.2	21.3	70.0
11	2.8	8.4	25.2	28.0	35.5	71.3
12	5.3	5.3	23.7	39.5	26.3	69.1
Number of Cases	116	159	307	368	205	1,115

Note: $\chi^2 = 254$, $df = 44$. Each cell gives the percentage of members who have been in group for x years (row) who get the designated percentage (column) of the answers correct.

especially evident among those with a purposive impetus. Even many whose initial participation allegedly reflects the quest for selective benefits exhibit a broad participatory impulse. They claim they chose Common Cause to be politically effective or mobilized and generally not because of specific issues or other detailed benefits.

Do Members Learn?

An assumption underpinning the experiential search framework is that members learn about the costs and benefits of participating. More extensive knowledge about these organizational features can lead to higher or lower levels of retention. But regardless of whether members stay or leave, learning is a fundamental component of experiential search.¹⁶ A prerequisite for validating this perspective is ascertaining whether contributors learn through organizational exposure.¹⁷ In Common Cause, individuals with many years of associational experience are no different sociodemographically than newcomers. They are indistin-

guishable in terms of education or income, so any variation must emanate from other sources. Findings that long-term contributors know more about the organization would demonstrate that members learn and would provide important side evidence that experiential search is fundamental for associational membership.

Several straightforward tests clearly show that new arrivals and long-time contributors are only distinguishable as a result of their organizational exposure. Members answered a battery of four basic questions about their organizational acumen.¹⁸ When respondents' knowledge is broken out by length of membership, the experiential search perspective receives strong support. Long-time contributors know a great deal more than newcomers (Table 2). Roughly one-third of the one- or two-year members missed all four of these questions; the same is true of less than 5% of the members who have been in the group for more than 10 years.

Also, as expected, the mean level of knowledge from one cohort to the next shows a clear pattern of diminishing

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**Table 3. Organizational Experience and Group Issue Opinions
(Common Cause Members without Opinions) (%)**

Issue	Organizational Experience (Years in Group)												Total Percentage	Number of Cases	X ²
	1	2	3	4	5	6	7	8	9	10	11	12			
Limit government spending	58	38	37	31	40	37	33	39	51	45	51	43	41	1,155	22*
Sunset legislation	40	23	20	21	11	13	9	11	6	8	14	10	14	1,164	54**
Campaign finance	24	24	11	10	6	7	5	1	2	1	7	3	7	1,167	74**
Equal rights amendment	28	30	26	19	24	19	13	18	17	12	15	13	18	1,162	25**
Lobby disclosure	20	20	14	8	5	9	5	1	3	1	11	4	7	1,174	56**

* $p < .05$

** $p < .01$

**Table 4. Organizational Experience and Personal Issue Opinions
(Common Cause Members without Opinions) (%)**

Issue	Organizational Experience (Years in Group)												Total Percentage	Number of Cases	X ²
	1	2	3	4	5	6	7	8	9	10	11	12			
Limit government spending	5	8	7	7	6	9	3	2	8	5	3	6	6	1,173	9
Sunset legislation	2	3	9	2	1	6	2	1	1	3	1	3	3	1,183	21*
Campaign finance	2	1	6	6	2	4	2	1	0	1	2	4	3	1,181	15
Equal rights amendment	0	3	6	1	1	1	1	2	2	1	2	2	2	1,182	13
Lobby disclosure	0	1	2	1	0	0	0	0	0	0	0	1	1	1,186	11
Defense spending	10	10	4	11	12	9	6	3	5	5	5	10	7	1,180	15
Equality of opportunity	3	16	19	12	17	6	18	10	17	17	19	12	15	1,179	18
Social service spending	5	11	6	10	1	14	6	6	10	8	8	8	8	1,167	13
Inflation	13	22	16	22	20	18	11	16	21	21	24	19	19	1,167	11
Abortion	10	1	7	5	2	4	2	4	2	1	3	3	3	1,178	16
Minimum guaranteed income	13	8	16	14	17	14	12	18	16	16	20	17	15	1,172	7
School busing	8	15	10	14	11	6	15	15	16	17	20	13	14	1,176	12
Nuclear energy	8	10	10	14	20	11	15	11	13	11	8	17	13	1,180	13
Soviet relations	33	39	25	20	32	34	32	27	29	31	29	30	30	1,180	10

* $p < .05$

marginal returns with organizational experience. A year has roughly four times as much impact for newcomers as for long-time members.

Next, consider knowledge about the collective goods that Common Cause furnishes. Members were asked about associational positions on five issues on the group's political agenda. In four out of five instances, new members are substantially more likely to reply that they do not know the Common Cause position on these policies (Table 3).¹⁹ But when

queried about their own positions on either these issues or other items not on the Common Cause agenda, differences between older and newer members are nonexistent, with one slight exception (Table 4).

While recent members are as opinionated about the public agenda as long-time contributors, they are less well versed in the organization's stances. A substantial portion of new members could not have been motivated to join the group by its overall issue positions, since they did not

know them. This finding supports the belief that general, largely purposive benefits, rather than returns from specific collective goods, are at the heart of the initial joining decision.

The learning process is also apparently fundamental for the leap from rank and file to activist status. Stated bluntly, activists are made, not born. Modal activists have been in the organization for three years when they move up from the rank and file; a scant 10% of all members are activists from the start.²⁰ This behavior is consistent with a search framework. Imperfectly informed individuals join the organization, learn about it, and decide what their next step is—whether to drop out, remain in the rank and file, or step up to the activist cadre.

The learning process provides initial side evidence that experiential search is a fundamental component of organizational membership. Contributors write checks and then refine their knowledge about the organization.

Exploring the Conditional Membership Process

Quitting is an important consequence of experiential search. This perspective requires that some members leave, but the likelihood of exit should diminish with experience.

Retention decisions should be explicable using the experiential search framework. To test this, four nested models, similar to those previously discussed, are made operational: (1) the Olsonian model in which divisible benefits are assumed to exceed costs; (2) the same model with organizational experience incorporated; (3) this second specification with collective benefits added; and (4) a complete formulation that also takes into account purposive and solidary returns. Estimation of these models permits the determination of both whether organizational search is involved

and what specific factors drive Common Cause participants to retain or revoke their membership.

Measurement. The likelihood of membership retention in the next contribution period—the dependent variable—is made operational using a seven-point scale. Scores range from one for those certain to quit to seven for those certain to remain. Roughly consistent with Common Cause's 78% renewal rate, 54% of all members queried responded that they were certain to renew (it should be remembered that the sample is weighted toward activists; only 49% of the rank and file expressed certainty that they would stay). The other 46% expressed different levels of uncertainty (scored from six to one): 26% called renewal very likely; 12% said it was likely; 3% suggested that they were not sure; and another 6% claimed that they were not very likely to, were unlikely to, or definitely would not renew their membership. Half of the organization was up for grabs to one degree or another, and a nontrivial minority was relatively certain of leaving Common Cause.

The factors posited to structure this choice are made operational in the following manner:

1. *Costs of membership* are measured as the ability to pay (family income) and the respondent's sensitivity to costs.²¹
2. The relevance of three *divisible benefits* are incorporated: (a) the importance of publications and whether contributions would cease without them; (b) the perceived value of political information; and (c) whether or not a member has political aspirations.²²
3. The lure of *collective benefits* is gauged by whether individuals (a) agree with the positions of Common Cause on key issues; (b) consider the leadership effective in providing collective goods; (c) are active in the group; and (d) believe that

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they are efficacious in the production of collective goods.²³

4. *Learning* is measured by organizational experience, made operational to capture the hypothesis of diminishing marginal returns with both a logarithmic and a linear term of the number of years in the organization. Incorporating linear and logarithmic terms is a standard means of measuring diminishing marginal returns (e.g., Maddala 1977).

5. *Purposive benefits* are tapped by whether respondents feel an obligation as good citizens to participate and whether they care about the group.²⁴

6. *Solidary benefits* are measured by whether the members value the interpersonal interactions. Common Cause provides a dummy variable on whether they have friends and colleagues within the organization.²⁵

Given these indicators, many expectations are straightforward. Others are not as clear as they might seem and will be contingent on how well Common Cause provides benefits—for instance, the degree to which the organization satisfies members who want to promote their political careers or who seek rewarding interpersonal relationships.

One clear, important expectation is that the sign for the logarithmic version of organizational experience ought to be positive (but there is no expectation for the linear term); this would reflect the diminishing marginal impact of experiential learning. Organizational experience acts as a surrogate for the respondents' level of information and their certainty about a host of factors that are correlated with time. As the previous empirical analyses demonstrate, members gradually learn about how a group functions and slowly develop an understanding of its positions. They should also become more certain about their subjective valuation of the host of benefits that the association offers. A big advantage in employing years in the organization is that it is a con-

tinuous measure, which makes it feasible to test the hypothesis of diminishing marginal returns.²⁶

It is possible to debate whether some of the other indicators measure one factor or another. Some ambiguities are inevitable, since benefit types are not empirically orthogonal to one another; and particularly in the case of collective and purposive rewards (to be discussed shortly), imperfectly informed individuals are likely to confound one benefit with another. On the whole, however, the indicators in this analysis gauge what has traditionally been meant by divisible, collective, purposive, and solidary benefits, as well as the costs of membership.

Results and interpretations. The ordinary least squares estimates for these models (Table 5) show that the retention decision is explicable.²⁷ Even when the focus of analysis is current members—a homogeneous, truncated sample of society—it is possible to separate out those prone to stay from those likely to leave. Such results are inconsistent with the informational assumptions underlying previous formulations of the joining process.

A framework incorporating various kinds of returns and learning does a superior job to more limited alternatives. A series of *F* tests comparing each model with its more restricted predecessor—model 2 with model 1 ($F_{2,1076} = 33.15$; $p < .01$); model 3 with model 2 ($F_{4,971} = 42.11$; $p < .01$); and model 4 with model 3 ($F_{4,971} = 7.93$; $p < .05$)—clearly identify model 4 as the best specification (see Pindyck and Rubinfeld 1981, 117–19, for an explication of joint *F* tests). Looking at selective incentives generated via divisible benefits is not enough; collective, solidary, and purposive benefits, as well as organizational learning, also affect the decision calculus.²⁸

The whole gamut of costs and benefits go into the retention decision.²⁹ The price

Table 5. Determinants of Retention Decisions

Variable	Model 1	Model 2	Model 3	Model 4
Constant	6.25**	5.38**	2.55**	.61**
Costs				
Ability to pay	.01	-.01	.03	.03
Low sensitivity to costs	1.11**	1.04**	.83**	.78**
Moderate sensitivity to costs	.51**	.52**	.45**	.42**
Divisible benefits				
Value of political information	.01*	.01	.01	.03
Publications' value	.15**	.15**	.09**	.08**
Low sensitivity to provision of publications	.29**	.20**	.12	.08
Moderate sensitivity to provision of publications	.24**	.21**	.12	.06
Political aspirations	-.15**	-.19**	-.18**	-.11
Learning				
Organizational experience		-.03	-.02	-.02
Natural log of organizational experience		.60**	.38**	.39**
Collective benefits				
Agreement with group's positions			.01**	.01**
Assessment of leadership's achievements			.30**	.27**
Activism in group			.08**	.10**
Feeling of personal efficacy regarding group			.05**	.05**
Purposive benefits				
Care about group				.04**
Sense of citizen duty				.07**
Solidary benefits				
Value interaction				.12**
Friends or colleagues are members				.10**
Number of cases	1,114	1,085	986	930
Adjusted R ²	.18	.22	.34	.36

* $p < .10$

** $p < .05$

of membership is an important consideration, although ability to pay, per se, is not. While all members know the monetary cost of joining, some are especially sensitive to it. Those finding that membership is not worth the opportunity costs—of foregoing participation in another organization, for instance—depart and either search elsewhere or become inactive.

The salience of divisible benefits is more nebulous. When the Olsonian model is made operational the group's publications, the political information it provides, and the opportunities it fur-

nishes to political aspirants all appear important. When other benefits are fully integrated into the decision framework, however, everything but the value of Common Cause publications is insignificant, and even the estimate of its impact is halved.

The utility of the Olsonian framework as a predictive model for retention decisions in public interest groups is questionable. As an explanatory framework, it is even less successful. This is not a direct indictment of a perspective designed to explain initial joining in economic groups. However, it does pro-

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vide some reason to doubt the assumptions underlying this decision-making model.

The effect of political aspirations, although insignificant in the final model ($t = 1.3$), is nevertheless intriguing. These individuals are less likely to remain in Common Cause than other contributors. Once they learn about the true nature of the organization—its antagonistic stance toward political parties, for example—they may decide that Common Cause is the wrong place for them and move on. Even if the politically ambitious are more likely than others to join the association (and given the percentage of members who have such motivations, this is probably the case), they can still be more prone to quit. Conversely, the relationship between future aspirations and retention will be positive in organizations that are good mechanisms for building political careers.

Collective benefits appear to be an important element in the conditional joining calculus. These findings refute the assertion that members never learn about collective goods; if the latter were the case, these estimates would be insignificant. Despite the fact that few members cited such returns as the principal reason for initially joining, they seem to be the most relevant factor for the retention choice.³⁰ Assessment of the leadership, level of activism, and feelings of efficacy all have an impact. So too does agreement with Common Cause's positions. Of particular salience are those good government issues—sunset legislation, campaign finance, and regulation of lobbyists—in which the organization has traditionally been involved.³¹

An explanation for this tension between the apparent insignificance of collective benefits in joining decisions and their critical role for retention calculi centers on experiential search. Many people are ignorant about the organization when they first sign on but gradually learn upon

joining. All they might know initially is that Common Cause is a group that deals with good government issues and for which experiential search comes cheaply. After contributing, they discover more specifically what the organization does and how much it accomplishes. These data are employed in calculating whether or not to stay in the group. Members move away from a concern about seemingly purposive benefits toward an interest in more specific collective returns.

This result implies that the traditional dichotomy between purposive and collective benefits—the former representing an adherence to the group's stated goals and the latter reflecting members' beliefs about their impacts on the production of collective goods—reflects a false distinction. The difference between these two types of rewards is overwhelmed, at least as it is tapped by survey instruments in the public interest group context, by the conditioning effect of information on the estimated policy benefits derived from membership.

Put another way, what is being interpreted as collective benefits may really be specific statements about purposive returns (this has previously been implied in Hardin 1982). Contributors' responses may reflect their perceptions of the group's, and not their personal, impact on the provision of collective benefits. Their assessment of the group's leadership and policies is important for deciding whether to exit because they think the organization can have an impact on the production of public goods. They learn about the group's efficacy and how their policy preferences correspond to the organization's and either stay or depart accordingly. Learning integrates initial and conditional membership: broad motivations are replaced by more specific ones. Group leaders have an incentive to foster the confusion between individual and associational efficacy. To the extent that they control the information contributors

employ to update their cost-benefit calculi, the elites will add to the confusion by telling members that they make a difference by acting collectively.

Explicitly purposive benefits are still germane. Common Cause is a good place for people who really care about such returns and want to be good citizens. Even broad policy attachments to the group may make participation satisfying. Consequently, contributors who have an abiding interest in being good citizens or who develop a strong identification with Common Cause tend to keep on giving.

To summarize, these findings about collective and purposive rewards support the proposition that members go from general to specific reasons for staying or leaving as they become more knowledgeable. This explanation is consistent with the side evidence about learning. All that is additionally required is the assumption that individuals recognize their informational shortfalls before joining and employ experiential search to remedy them partially. This strategy leads members to offer vaguely purposive reasons for initially joining and more specific concerns about collective goods for staying or leaving.

Similarly, solidary benefits are rarely mentioned as a major reason for joining. Yet they too are significant factors in the retention choice. For those seeking such interactions, the organization delivers the goods. Others may discover that a by-product of searching over purposive and collective benefits is a rewarding associational involvement. Again, the tension between initial and subsequent conditional membership choices stems from the fact that contributors learn over time. Those who find rewarding interpersonal relationships stay in Common Cause; those who either do not care about such interactions or decide that the organization does not provide the solidary benefits they desire, depart.

The findings regarding learning offer a

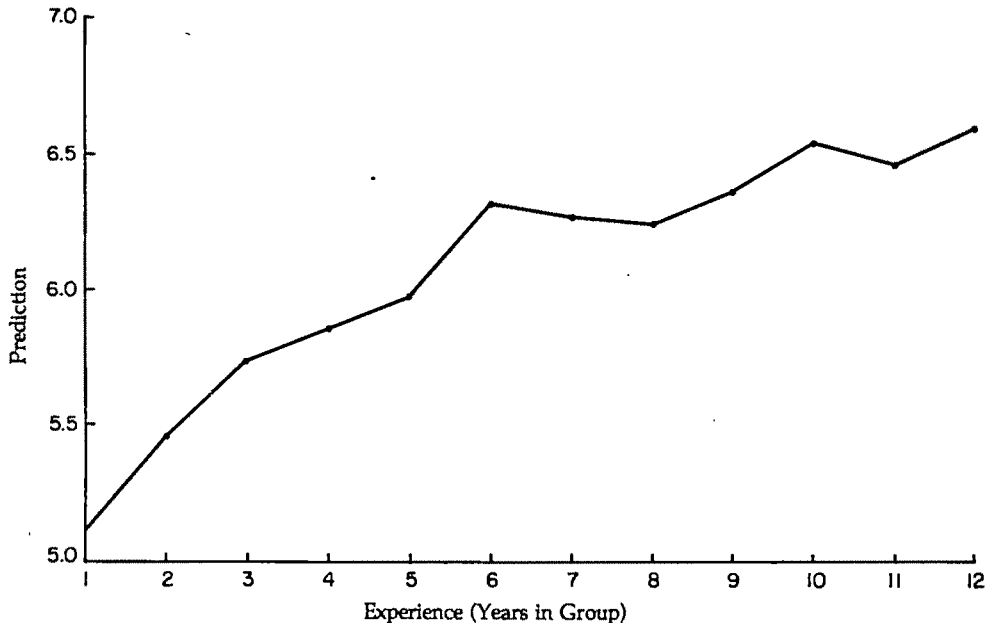
second test of the experiential search theory and, once again, provide validation. Specifically, they lend credence to the hypothesis that experiential learning yields diminishing marginal returns. Each year has a positive, yet declining, impact on the probability that members will remain committed even after all the standard costs and benefits of joining are incorporated into the model. *Ceteris paribus*, a newcomer scores three-quarters-of-a-point lower on the seven-point scale than the most veteran contributors. In other words, there is considerable vacillation among new members about their future intentions, but this uncertainty dissipates over time. By and large, departing contributors are recent converts who, upon learning about the group, become disenchanted; they are not long-term members who grow bored with Common Cause.

Similarly, the *overall* predictions from the full experiential search model (model 4) lend credence to the hypothesis that the probability of staying increases temporally but at a diminishing rate (Figure 2). The impact of the early years is roughly five times greater than the effect of the later years; and, as discussed previously, there are no obvious differences between long-term members and newcomers that might render this relationship artifactual. The only possible inference is that individuals learn and update their information. Those who like what they see stay, and the rest search elsewhere. This conclusion is consistent with Common Cause's own troubles in holding onto new members. Only about 55% of first-year members continue to contribute the following year, while roughly 90% of long-time members remain.

Finally, consider what happens when the sample is split between newcomers—those contributing for six years or less—and veterans and a revised version of model 4 is estimated (Table 6).³² Most strikingly—but predictably from an ex-

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Figure 2. Retention Predictions and Organizational Experience



Note: Predictions are based on the seven-point scale used to measure a respondent's probability of retention. Scores range from one (certain to quit) to seven (certain to renew).

periential search perspective—the model does a superior job of explaining why newcomers come and go as compared to veterans. Longer-term members more closely approximate the full information ideal and are prone to depart for idiosyncratic reasons. Also as predicted, specific characteristics loom larger in newcomers' retention decisions.

While there is no appreciable difference between the two samples in the findings for purposive benefits (no coefficient is significant), there are variations in the effects of solidary benefits and especially collective returns. In the latter the impact of these factors is stronger for newcomers than for veterans. Not only is learning about specific characteristics crucial, it is

especially salient for those who are new to the organization.³³

All three tests of the experiential search perspective support its validity as a superior framework for conceptualizing the retention choice. Members make their decisions in a systematic, comprehensible fashion. The decision to remain in the organization reflects their discovery that the group provides the benefits they are searching for. Learning is an important component in understanding how conditional membership choices are made. Overall predictions about the probability of remaining in the organization reflect the diminishing marginal returns to be expected if individuals garner information through experiential search. An individ-

Table 6. Retention Decisions of Shorter- and Longer-Term Members

Variable	Newcomers	Veterans
Constant	-.23	2.06**
Costs		
Ability to pay	.04	.01
Low sensitivity to costs	1.12**	.56**
Moderate sensitivity to costs	.58**	.23**
Divisible benefits		
Value of political information	-.01	.06
Publications' value	.12*	.07*
Low sensitivity to provision of publications	.29*	.10
Moderate sensitivity to provision of publications	.09	-.03
Political aspirations	-.19	-.10
Learning		
Organizational experience	.10**	.04*
Collective benefits		
Agreement with group's positions	.01**	.01**
Assessment of leadership's achievements	.28**	.24**
Activism in group	.16**	.08*
Feeling of personal efficacy regarding group	.02	.06*
Purposive benefits		
Care about group	.05	.04
Sense of citizen duty	.10	.04
Solidary benefits		
Value interaction	-.04	.13
Friends or colleagues are members	.20**	.06
Number of cases	370	560
Adjusted R ²	.40	.24

* $p < .10$

** $p < .05$

ual's first years in the organization are especially important—particularly for learning about highly specific characteristics.

Conclusions: Experiential Search and the Retention Choice

Understanding why people leave or remain in groups has received little attention. The retention decision should not be taken as a given, that is, something that is determined by the initial choice to contribute. The conditional membership deci-

sion is fundamental for organizational stability and is a constant source of anxiety for group leaders. It reflects an experiential search process through which contributors acquire information and make increasingly knowledgeable choices.

This perspective is very different from either a game-theoretic framework or those decision-theoretic models adopted by previous analysts of organizational membership. Here it is assumed that imperfectly informed decision makers are aware of their shortfalls and take them into account in making choices while playing against nature. Because accruing

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information is costly, there is an incentive to become informed in the most efficient way possible. If the price of membership is small and a mistake is easily rectified at the next contribution period, joining a group and learning about it experientially may well be the optimal strategy.

Common Cause members behave very much in this manner. The following picture of the average contributor emerges from this analysis: Individuals join an organization about which they are largely uninformed. They have a rough idea about what it stands for, but they lack the detailed knowledge needed to decide whether this is the best association for them. They are as informed about politics, as educated, and as wealthy as long-time members. But they lack organization-specific information, which is best gained experientially.

Consequently, the retention process is explicable by specifying a model that reflects information updating. Among other things, the findings make clear that early on when the incremental informational gains are greatest, there is a higher probability that revising prior beliefs will precipitate departures. As time progresses and the additional impact of another period diminishes, so too does the probability of dropping out. The accrual of knowledge about the organization also leads members to rely upon more specific criteria in making their conditional membership decisions than they claim motivated their initial contributions. Examining the model's overall predictions about future membership behavior simply buttresses the experiential search story further. So too does the contrast between what the model reveals about newcomers and about veterans.

Group participation ought to be conceptualized as an experiential search process in which individuals with only imperfect information are forced to make choices. They may commit calculated mistakes, but they will eventually rectify

them. This framework makes organizational quitting understandable and provides a linkage between the initial membership choice and follow-up decisions. It furnishes insights into how the ebb and flow of group membership ought to vary systematically across associations, depending upon both the costs of contributing and the variety of benefits available to association members. Any number of interesting comparisons for testing this general perspective spring to mind: for example, contrasting an organization like Common Cause which conducts much of its business through the mail and over the telephone, and one like the League of Women Voters, for which interpersonal interactions that provide solidary rewards are more central.

The empirical findings suggest that group leaders might weigh newer members' preferences more heavily than those of long-time contributors, assuming that both are equally valued. They also imply that any educational efforts should be centered on appealing to these newcomers; utilizing scarce resources to sway veteran participants is likely to be a less productive investment.

What is needed in the future is the collection of data better designed to capture the dynamics of retention choices. Without a doubt, the key flaw of the present research is that only limited temporal data are available. Future endeavors must employ longitudinal designs, including studies that follow up on individuals who begin, continue, and cease contributing (and perhaps go elsewhere); analyses that explicitly build in samples of the general public as well as group members; and research that incorporates a multiplicity of groups varying on those dimensions that should affect the amount of experiential search undertaken. Such data will provide a better, more integrated understanding of why people join and either stay in or leave organizations than the initial attempt made in this analysis.

Notes

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1. Building a core membership is extremely important for ensuring long-term organizational survival. Even those associations that receive foundation grants and support from other organizations may find that these sources of income are unreliable.

2. In her study of four Michigan groups, Cook (1984) does provide some data on whether member satisfaction-dissatisfaction is associated with intentions to remain in the organization. Moe (1980) also touches on the decision to exit in his empirical work. In an intriguing theoretical piece, Johnson (1987) demonstrates that if a median voter rule were employed in determining dues levels (contributors constitute the electorate), voluntary associations would inevitably collapse due to exit. He also correctly notes, however, that group leaders can thwart this potential instability. At Common Cause, for example, dues are set by those running the organization. In his well-known work, Hirschman (1970) acknowledges that individuals may respond to changes in organizational performance by departing. However, his model is based on the idea that deteriorating leadership performance over time may precipitate either exit or voice from individuals possessing perfect information. By contrast, the present research explores the implications of imperfect information for members' behavior.

3. *Retention* is the term utilized in studies of reenlistment decisions by military personnel (e.g., Gotz and McCall 1983, 1984).

4. This is a generic problem that all organizational leaders face. Employers, for instance, also deal with the problem of holding onto valuable workers, as well as attracting capable replacements. What sharply distinguishes interest groups from other organizations is that nonparticipation in the public arena is a far more viable alternative.

5. The Common Cause survey was conducted in the fall of 1981 by the political science department of Stanford University. It was funded by grant SES-8105708 from the National Science Foundation to Heinz Eulau in support of research by Jonathan Siegel. Many thanks to Siegel for generously sharing these data. Because this is a stratified sample, the descriptive information is frequently broken down between rank-and-file members and activists; but, to reiterate, the sample design has no impact on the hypothesis tests conducted. Only if the sample is stratified on the choice in question—for example, the decision to join—is there a problem.

6. Not all of the alternative specifications are examined here. For example, Smith (1985) develops a model of contributions to environmental organizations based on the theory of club goods. However, none of the public goods that Common Cause (or few other groups) might provide have an element of excludability to them, which is essential for translating the economic theory of clubs into one of group membership. Similarly, Hansen (1985) furnishes a context-dependent model of membership decisions based on prospect theory. Incorporating his insights would require a complete panel design.

7. Although the traditional distinction between purposive and collective benefits is employed at this point in the analysis, a different perspective on this dichotomy will be introduced later.

8. Moe takes the imperfect information assumption somewhat further, but almost strictly from the perspective of group leaders (particularly, how to structure an organization to encourage donations) rather than from the contributors' viewpoint.

9. An interesting implication is that fewer members will learn experientially in groups with expensive membership charges. Instead, potential contributors will use their resources to search from outside of the organization. Membership in interest groups with relatively high dues should therefore be less volatile than in comparatively "cheap" organizations.

10. A quintessential example of exogenous forces is found in Wilson's (1962) observation that "amateur Democrats" lose their enthusiasm to spend endless hours working for the cause once an election is over.

11. As will be discussed in more detail, the ideal means of studying membership would be through a panel study. The analysis presented here is a second best alternative.

12. Respondents were asked whether their present family income was (1) under \$10,000; (2) \$10,000-\$20,000; (3) \$20,000-\$25,000; (4) \$25,000-\$35,000; (5) \$35,000-\$50,000; (6) \$50,000-\$75,000; (7) \$75,000-\$100,000; or (8) over \$100,000.

13. Seventy-three percent of those contacted actually wrote: 60% of the rank and file and 90% of all activists. Further corroboration of the mobilization of the membership is given by the finding that 44% of those sampled (35% of the rank-and-file members contacted and 78% of the activists) reported writing in the previous year about at least one of the following six issues: a constitutional amendment limiting government spending, sunset laws, campaign finance, reapportionment, lobby disclosure, or the Equal Rights Amendment (ERA).

14. An overwhelming 98.5% of those queried reported reading the Common Cause publications they receive.

15. The 31 reasons for joining listed in Table 1 were coded from 99 different answers. Only those citing a specific issue or issues, leadership, nonpar-

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tisan group, or unique-important were subsequently difficult to classify into the four broad categories. They accounted for about 8% of the respondents.

16. Empirically there should be (and is) a positive relationship between knowledge—and organizational experience generally—and the decision to stay in the group. Those who gain information and do not like what they learn drop out.

17. A strong caveat is in order: Cross-sectional evidence is being used to draw temporal conclusions. It is nevertheless hard to tell a compelling story about how employing this cross-sectional information would confound this part of the analysis. Thus it is worthwhile to try to uncover evidence of contributor learning with these data, especially because they are probably the best currently available.

18. The four statements (with their answers), to which members could reply *true*, *false*, or *don't know* are (1) members of Common Cause elect the governing board [true]; (2) members of Common Cause elect the Common Cause chairman [false]; (3) Common Cause is a federation of state and local organizations [false]; and (4) Common Cause state organizations determine their own issue agendas [true]. A further check was performed to make sure that the relationship between knowledge and tenure is not a spurious reflection of the fact that activists have been in the group longer. The data show that even after controlling for whether respondents are activists or rank-and-file members, the association remains quite strong, although activism also is related to organizational knowledge.

19. The one exception dealt with constitutional amendments limiting government spending. This issue was relatively new on the Common Cause agenda, and long-time members themselves might not have had time to learn about it. More than 40% of the entire membership had no opinion on Common Cause's position on it, as compared to a maximum of 18% on the other four items.

20. Rather than being asked how many years they have been activists, these members were requested to choose one of five categories: (1) 8–10 years, (2) 6–7 years, (3) 4–5 years, (4) 2–3 years, and (5) 1 year or less. The three-year estimate is conservative; it is based on the greatest possible number of years—10, 7, 5, 3, or 1—that participants might have been involved relative to the number of years they have been in the group. If the means of the categories were substituted for the upper bounds, modal activists would be in their fourth year.

21. As mentioned, family income is tapped with an eightfold variable. Cost sensitivity is gauged with two dummy variables coded from a question in which members were asked if they would remain in the organization (*yes*, *can't say*, *no*) if Common Cause raised its annual dues from \$20 to \$40.

22. Members were asked two fivefold questions about the importance to them personally of (a) mag-

azines and other Common Cause publications and (b) the political information the association provided. They were also given a question parallel to the one on cost sensitivity in which they were queried whether they would stop contributing if the publications were halted.

23. Position agreement is measured as

$$-1 \left| \sum_{i=1}^5 (X_{ip} - X_{ic})^2 \right|,$$

where X_{ip} and X_{ic} are, respectively, contributors' personal preferences and views of where Common Cause stands on the following issues: an amendment limiting government spending, sunset legislation, campaign finance laws, the ERA, and lobby disclosure laws. Of course, only those respondents with personal preferences and estimates of Common Cause positions are included. Contributors without issue opinions (see Tables 3 and 4) are excluded. Leadership assessment is an additive index combining fivefold responses to questions about the legislative success of Common Cause and an explicit rating of how well the leadership and staff do their jobs. Activity is tapped by counting whether a person has (a) written or talked to Common Cause staff or leaders about a group policy or position; (b) attempted to attract new contributors; (c) attended a local Common Cause meeting in the last year; (d) voted in the 1981 governing board elections; or (e) completed the 1981 membership poll. Finally, the personal efficacy measure combines the responses to two parallel questions on how important to the success of Common Cause individuals think their own contributions and their own political activities are.

24. The former is an additive index of the importance assigned to membership in Common Cause as a means of fulfilling the responsibilities of citizenship, supporting leaders like John Gardner and Archibald Cox and helping to ensure good government. Caring is an additive scale of member interest in each of the five issues used to construct the position agreement scale.

25. The former is a fivefold response to a question on the importance contributors attribute to meeting interesting people and making new friends.

26. It might be maintained, in the spirit of the so-called garbage can theory (Cohen, March, and Olsen 1972; March 1978), that organizational experience is tapping the adaptation of preferences to what Common Cause has to offer, that is, the conventional assumption that preferences are fixed may be incorrect. Admittedly, it is impossible to distinguish definitively between changing preferences and learning given the lack of panel data. However, the fixed preference assumption is probably quite reasonable. It has been shown in this analysis that individuals do learn about the organization's operations and issue positions. It is also hard to believe

that Common Cause members suddenly develop a preference for good government, political information, or social interactions once they sign up for the group.

27. Before estimating the models, a number of preliminary steps were taken. To ensure that it is acceptable to pool the Common Cause-designated activists with rank-and-file members a Chow test (Chow 1960) was conducted. The results of the test proved to be insignificant, thus permitting the pooling. Also, Hausman tests (Hausman 1978) were utilized to ascertain whether any of the independent variables were endogenously determined (the logarithmic and linear organizational experience terms were tested jointly): none were. Additional tests were undertaken to investigate whether the standard sevenfold specification of the dependent variable was correct. A Hausman test for determining whether the slopes for a seven-ordered probit are identical to those for a threefold analog (*no, can't say, yes*) led to the rejection of the null hypothesis that $B_7 = B_3$ ($\chi^2 = 35$, $df = 19$). This finding implies that the seven-category operationalization is subject to specification error of some sort, although the ramifications are uncertain. This discovery precipitated a further investigation to uncover whether a better specification was available. The obvious alternative is a two-stage conditional structure that breaks the retention decision first into a direction (*no, can't say, yes*) and then into a strength (*certain, very likely, likely*) choice. When the log-likelihood ratios of the conditional and unconditional processes are compared (see Vuong 1986), however, the latter model is far superior. Given this strong finding, the unconditional, sevenfold specification was adopted with the caveat that there might be some superior alternative. Having decided upon this specification, both ordinary least squares (OLS) and probit estimation were employed. The results are identical for all intents and purposes; the OLS results are reported here due to their ease of interpretation. To reiterate what has been noted previously: attrition should not have had an impact on the estimates in Table 5. As long as quitting is related to exogenous factors—unhappiness with the group, evaluations of the leadership, and so on—and these factors are controlled for, the ensuing estimates are unbiased. None of the difficulties associated with choice-based sampling are relevant in this case.

28. Multicollinearity is not a major problem in these models. The linear and logarithmic learning terms are highly correlated. This intercorrelation is to be expected but, as mentioned, utilizing a logarithmic and a linear term was suggested to tap diminishing marginal returns (see, for example, Maddala 1977). If one of the two terms is excluded, the other findings do not change substantially. On examining the intercorrelations between the independent variables, only two other pairs of correlations are above the .30 level: the valuation of

political information and publications (.59) and the two dummy variables on cost sensitivity (–.60). Combining the information and publication variables results in a poorer-fitting model. The resulting variable is insignificant (whereas the publications' value is significant in Table 5), and no other substantive results change. There is also no appreciable change if the variable measuring moderate sensitivity to costs is dropped except that the coefficient for high sensitivity increases in magnitude. To explore more systematically the potential for multicollinearity, the singular-value decomposition (SVD) recommended by Belsley, Kuh, and Welsch (1980) was performed for model 4. Interestingly, the SVD for the full model shows a moderate level of multicollinearity (SVD = 60). However, a major cause of this multicollinearity is the relationship between the intercept term and other variables: this result is common when using dummy variables but does not harm the estimates. If the intercept and the logarithmic learning terms are dropped, the SVD is cut in half, to a level where experimental work shows the multicollinearity is quite small (SVD = 30). Again, the findings are robust with respect to multicollinearity.

29. Unfortunately, no good measure of nonpecuniary costs was available for the whole sample. An indicator that worked for part of it ($N = 395$) was whether or not Common Cause members responded to attempts to mobilize them. Even after controlling for all other factors, these costs are important determinants of the retention decision. This result provides evidence that those who find these additional organizational demands taxing depart, presumably either to find an association where they can simply write checks or to leave the world of organizational participation completely.

30. An increase of one standard deviation in a member's score on each of the collective benefit indicators would result in a jump of .71 on the retention scale. This change is slightly greater than the impact of membership costs (.63), followed by divisible benefits (.23), purposive returns (.21), organizational experience (.18), and solidary rewards (.17). The strength of membership costs might be a bit of a surprise. However, the measures of cost sensitivity—the willingness to quit in response to a \$20 increase in dues—are also tapping estimates of the value of benefits.

31. These findings are based on a regression using the model 4 specification, with the sums of the squared differences for each issue being substituted separately instead of cumulatively. The two relatively new issues on the Common Cause agenda—ERA and placing limits on government spending—are not relevant. These concerns represent a departure from the issues for which the organization gained its reputation and apparently have not become a prime reason for remaining in the organization (or for joining, probably).

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32. The logarithmic term for learning is now unnecessary, since the sample was split according to how many years a contributor was in the group. The definitive means of testing whether coefficients vary between newcomers and veterans is to employ interaction terms with the full sample. However, multicollinearity makes such estimates unfeasible in this instance.

33. The larger impact of cost sensitivity on newcomers than veterans probably reflects the former's greater uncertainty about the value of membership.

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MASS DYNAMICS OF U.S. PRESIDENTIAL COMPETITIONS, 1928-1936

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T*his analysis investigates the mass dynamics of competitive electoral politics with respect to the presidential vote during the 1928-36 realigning period for the United States. A formal system of three interdependent differential equations is employed to characterize the dynamic processes of the aggregate voter shifts between the Republican, Democratic, and nonvoter populations. The modeling strategy is used to locate substantial variations in the mass dynamics between large subgroups in the electorate as well as to identify both national and socially interactive components to the patterns of voter movements. The results show that the overall realignment period was quite complex. Vote switching from the Republican party to the Democratic party was the dominant characteristic of the 1932 election, whereas additional Democratic gains in 1936 came mostly from new voters.*

This study examines the roles played by new voters and former Republicans in the dramatic rise in Democratic presidential support between 1928 and 1936. The analysis focuses on the process of deinstitutionalization among voters in a setting of large-scale new voter participation. The term *institutionalization* refers to a constraining influence on electoral competitions and is linked substantively to work by Przeworski (1975) and, to some extent, Huntington (1968). The institutionalization of partisan behavior is identified by the existence of enduring partisan attachments and is viewed as a function of time and the frequency of repeated electoral experiences among voters (i.e., the development of a party "habit"). *Deinstitutionalization* refers to the process by which voters break these former partisan bonds.

There are two factors crucial to the process of deinstitutionalization as it is characterized here. First, the assumption

is made that influences that disturb partisan ties affect voters differentially, depending upon the force of the political appeals on particular groups in the population and the strength of each group's previous ties (a central theme in work by Nie, Verba, and Petrocik 1979). Second, previous electoral experiences for certain groups can produce resistance to these appeals. This second factor addresses the concept of political immunization as it has been described by McPhee and Ferguson (1962). Major social and political disturbances of sufficient magnitude to cause large-scale changes in partisan attachments among many groups should affect more strongly those who are the least immunized to alternative partisan appeals.

Critical to the dynamics of the deinstitutionalizing process is the role played by new voters, a totally noninstitutionalized and nonimmunized sector of the electorate. A number of questions on the general impact of new voters on electoral

systems bear particular relevance to this analysis. When new voters enter the electorate in substantial numbers in response to a major social disturbance (such as the Depression), do they do so equally across all subgroups in the population? In terms of the timing of their entry into the electorate, do they begin to participate at moments when there are large partisan shifts as well? Alternatively, are they drawn into the electorate following a previous election in which there was significant excitement generated by large partisan shifts? Or, perhaps, do the new voters enter the electorate in massive waves due to exogenous social and political conditions, subsequently destabilizing the existing partisan alignment and precipitating a full-scale realignment? Answers to these questions are viewed here as central to an understanding of realigning processes and are addressed with regard to the 1928-36 realignment period in the United States.

In characterizing the dynamic processes involved during periods of volatile electoral change, this analysis distinguishes between two separate mechanisms by which such mass movements may occur. The first concerns the ability of nationally distributed political appeals to effectively channel the shifting partisan and new voter movements. The second addresses the ability of localized political forces to act as an intermediating factor in determining the magnitude and direction of the electoral changes. For example, in the case of the new voters, this asks the question of whether new voters are activated by national political forces as presented to them, say, by the national media or whether they are drawn into the participatory setting by other voters and localized partisan campaign forces. Here questions associated with the identification of the relative impacts of these two different political mechanisms of the mass dynamics focus on their independent effects on the new voter and shifting par-

tisan populations.

This analysis begins with a consideration of the conversion-versus-new-voter hypotheses with regard to the 1928-36 electoral period in the United States. It presents a baseline description of the partisan strengths for the United States during the period from 1920 through 1936. It presents and investigates a formal model capturing the dynamics of the partisan competitions and new voter fluctuations and uses the model to identify contrasting patterns in roles played during the 1928-36 realigning period by certain groups in the U.S. population. Then, the magnitudes of the national as well as the more localized political influences are compared to determine the relative strengths of the underlying components of the mobilization and conversion processes.

The Conversion and New Voter Hypotheses

The literature focusing on the conversion-versus-new-voter hypotheses is quite divided. Sundquist (1983, 229-39) uses selective aggregate data to argue that Republican-to-Democratic conversion was the dominant type of electoral activity for the period. Further support for the conversion hypothesis comes from Erikson and Tedin (1981)—who use the *Literary Digest* "straw poll" data—as well as from Key (1964, 523-35), Burnham (1970), Ladd and Hadley (1978), and others. The most prominent proponent of the new voter hypothesis has been Kristi Andersen (1979b), with support for the hypothesis also coming from Converse (1975), Campbell and his colleagues (1960, 153-56), Petrocik (1981, 55-57), and others.

Arguments supporting the conversion hypothesis typically suggest that the steadily worsening national economic conditions of the time produced widespread discontent with the Republican

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Table 1. U.S. Presidential Vote As a Proportion of Total Eligibles, 1920-36

Vote	1920	1924	1928	1932	1936
Democratic	.15	.13	.21	.30	.35
Republican	.26	.24	.30	.21	.21
La Follette		.08			
Total vote	.44	.45	.52	.53	.57
New voters		.004	.079	.006	.045

administration. Moreover, Roosevelt was able to mobilize many of these discontent former Republicans. Indeed, he developed a "natural" constituency among the working class, especially coming from the economically hard-hit industrial urban areas (Sundquist 1983, 214-23). The new voter proponents argue that partisan attachments were probably as firm (or nearly as firm) then as they have been found to be in later years (e.g., as reported by Campbell et al. 1960). Thus the new Democratic support probably came from a generation of new voters rather than disenchanted Republicans. Many of these new voters were young, and perhaps also immigrants or descendants of immigrants (see Andersen 1979a, 39-52; Petrocik 1981, 55).

Arguments concerning virtually all of the above hypotheses have relied on data around whose quality and completeness there are controversies. Andersen's use of the 1952-72 Survey Research Center/Center for Political Studies data to reconstruct the partisan voting habits of the respondents in terms of how they voted in the 1920s and 1930s has been seriously challenged by Erikson and Tedin (1981), Sundquist (1983, 229-39), Niemi, Katz, and Newman (1980, 648), and Reiter (1980). Erikson and Tedin (1981) have tried to address this problem by using the only available survey data for the period. However, they acknowledge that the "straw poll" data collected by the *Literary Digest* is of a "tarnished sort" (Erikson and Tedin 1981, 952). Problems of sampling technique abound, and the authors disagree with Shively's characterization of

the nature and direction of the various biases (see Erikson and Tedin 1981, 953 and Shively 1971-72, 62). To date, users of aggregate data have hardly fared better. Previous studies have typically limited their analyses to particular geographical areas. The widespread use of "heuristic" samplings of aggregate data to study realignments goes back to Key (1955). For example, Key uses data for various towns in five states, and Sundquist (1983, 236-37) uses selected county and town data from five states. Yet there is no guarantee that the selected areas are representative of the nation as a whole. In sum, there are no reliable survey data for the period under study, and until now there has never been available for analysis a single *complete* set of electoral and census data for all of the United States using (the relatively small) county-level aggregations.¹

Table 1 presents the aggregate electoral strengths for the Democratic and the Republican parties in the presidential contests from 1920 through 1936. All of the results in Table 1 are written as proportions of the eligible electorate and thus can be understood as measures of mobilization. These measures differ from those commonly presented elsewhere. Typically, the electoral outcomes of the period are represented either as vote shares (i.e., as proportions of the total vote) or as partisan vote totals. For example, vote shares are used by Ladd and Hadley (1978, 43) and Key (1964, 523-40), while partisan raw totals (i.e., actual votes) are presented by Andersen (1979a, 29).

There are problems with the vote share and raw totals methods, however. Vote shares can produce a misleading interpretation of overtime aggregate change in situations in which the total vote is also changing. For example, it is possible for a party's share of the vote to decrease due to an expansion in the denominator (i.e., the total vote) while at the same time the party has not lost any of its previous support from the population. In a situation in which new voters are suspected of playing a pivotal role in determining the direction of a shifting partisan balance, a vote share measure can indicate partisan shift when there may have been only new voter movement. The use of actual votes, or raw totals, instead of vote shares encounters a related problem. Comparing raw partisan totals at different points in time makes most sense when the size of the electorate is stable. Such a longitudinal comparison can be misleading in situations in which the electorate (i.e., total eligibles) is expanding or contracting. Thus a party may receive 15 million votes in one election and 16 million votes eight years later, but if the eligible population has increased as well, the increased partisan total need not indicate increased partisan "power" relative to the other parties or the overall electorate. Indeed, the party's support relative to the size of the total electorate could have decreased. Both of the problems mentioned above regarding the vote share and the raw vote measures are unraveled by using the mobilization measure.

The data in Table 1 indicate that both the Democratic and the Republican parties increased the level of support they received from the pool of total eligibles from 1920 through 1928. However, from 1928 to 1932, the positions of the two parties relative to each other virtually reversed. While the Democratic party increased its share of the total electorate from 21% to 30%, the Republican party's support fell from 30% to 21%. Note also

that the total vote relative to the electorate increased only slightly from 1928 to 1932. Between 1932 and 1936, however, the total vote did increase substantially (more than four times the 1928-32 increase); while at the same time Democratic strength continued to increase, and Republican strength remained approximately constant. While conclusions regarding partisan shifts can barely qualify as tentative with such large aggregate measures, on the surface at least these results seem to suggest that new voters may not have played as crucial a role in the 1932 election as compared with the election of 1936 and that there indeed may have been an actual realignment in 1932, defined in terms of partisan conversions. The analysis that follows pursues these points more directly.²

The Structure of the Party Competition

The model of partisan competition developed below is a time-dependent system of three interconnected differential equations, which, in combination, address expectations of change in aggregated partisan electoral totals. Such models have been successfully exploited in the social-scientific literature by Coleman (1964, 1981), Simon (1957), Przeworski and Soares (1971), Przeworski and Sprague (1986), Sprague (1981), Tuma and Hanna (1984), Luenberger (1979), and Huckfeldt, Kohfeld, and Likens (1982), as well as others and have been notably useful in modeling the dynamics of military spending in competitions between nations (see Gillespie et al. 1977; Ward 1984). This type of modeling is also similar to that employed to characterize time-dependent ecological systems of biological populations within fixed environments (see May 1974). The mathematical theory underlying the analytic and dynamic properties of all such systems,

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both linear and nonlinear, is complete (Hirsch and Smale 1974; Kocak 1986; Luenberger 1979). Here we are interested in modeling the population fluctuations of three groups in the political environment: Democrats, Republicans, and nonvoters. We want a mathematical statement that corresponds to each group and describes change as a function of existing voter support for all other groups. Thus we desire three statements that when taken together describe the voter movements among parties as well as shifts between parties and nonvoters, all of which take place simultaneously at each point in time.

We begin by developing a model of the mass electoral dynamics for the Democratic party. In this analysis, we differentiate between two types of dynamic processes that could lead to growth or decay in the various partisan populations. The first type to be developed below is labeled the "uniform" component to aggregate partisan movements. In 1932 it is likely that many voters throughout the nation were energized through a national appeal to the electorate's broad social sensibilities. For example, widespread discontent with the Republican handling of the economy could have led to the development of a generalized sympathetic ear across the electorate for the Democratic message. Moreover, this increased attentiveness for the Democratic appeal could have affected many voters independently of their localized partisan environment (i.e., "uniformly" across the nation); that is, it may not have mattered whether or not there existed a strong Democratic presence within the voters' localized milieu. Rather, a certain number of Republicans, based only on the number of Republicans available within a given area, would have weighed their electoral options and decided to switch their support from the Republican party to the Democratic party.

If we use the proportion of the localized electorate that supports the Republican party as a measure of the size of the

Republican population (theoretically available for conversion to the Democratic party), then we can express the change in support for the Democratic party as

$$dD/dt = fR. \quad (1)$$

Here, R is the proportion of the electorate supporting the Republican party, D is the proportion of the electorate supporting the Democratic party, and f is a parameter of the model that corresponds to the probability of the occurrence of a Republican-to-Democratic conversion within the population at an instant in time. Throughout this analysis, this type of dynamic process is referred to as the "uniform" component of the model, due to the sense of the voter calculations assumed and the independence of the above mathematical statement from the strength of the localized Democratic presence, which might have conditioned, through social interaction with the existing Democratic environment, the rate of conversion to the Democratic party.

Yet this suggests another way in which partisan conversions may take place. The rate at which the national Democratic appeal produces new Democratic converts could have a social (i.e., contextual) component as well; that is, voters may see how others in their local environment react to the Democratic message. In areas in which there exists at least a moderate Democratic presence, pro-Republican sentiments on the part of some voters might be more difficult to defend. The availability of accepted alternative partisan perspectives within politically heterogeneous neighborhoods, combined with the institutional strength of a locally stronger Democratic party's electoral apparatus, should be enough to loosen the grip of the Republican party on some voters and thus allow for a higher level of conversion to the Democratic ranks. Thus we wish to capture in our modeling efforts

the effects of interacting partisan populations in producing electoral change.

The above argument can be included in the model for change in the support for the Democratic party over time by rewriting equation 1 as

$$dD/dt = fR + bRD,$$

where b is a parameter in the model and expresses the probability of a Republican-to-Democratic conversion at an instant of time due to the conditional strengths of both partisan populations interacting simultaneously. This is the second type of dynamic process to be included in the model, and it is referenced below as the "social" component. It addresses the dynamics of locally interacting partisan populations, and its inclusion here reflects the lessons of some recent research indicating that the impact of such localized social forces on voter responses to electoral battles can be substantial in magnitude and crucial to the specification of the properties of aggregated partisan movements (Beck 1974; Brown 1987; Huckfeldt and Sprague 1987, 1988; MacKuen and Brown 1987).

All such interaction terms, as specified above, are symmetric in the sense that interactive growth for the Democrats is low when either there are (1) few Democrats interacting with few or many Republicans or (2) few Republicans interacting with few or many Democrats. Interactive growth is greatest when there are sizable Democratic and Republican populations, which is both an expected and desirable property of the above characterization of the partisan competitions. The above specification of such an interaction is well represented in an extensive social-scientific literature on communication, contagion, and diffusion modeling (Coleman 1964; Huckfeldt 1983; Huckfeldt, Kohfeld, and Likens 1982; McPhee 1963; Przeworski and Soares 1971; Rapoport 1963, 1983; Simon 1957; Sprague 1976).

Such terms are also common in ecological models of interspecies interactions within contained biological ecosystems (Danby 1985; Haberman 1977; May 1974; Rosen 1970).

Democratic party electoral strengths can also improve due to an infusion of new voters to the party ranks. This addresses the hypothesis that the mobilization of new voters was a crucial factor in the changing Democratic fortunes between 1928 and 1936. It is possible for the new votes to be mobilized due to a national appeal to their potential partisan sensibilities, thus attracting new voters in correspondence to the size of the localized nonvoting population. Struck by the declining national economic fortunes and the possibility of change given by a new Democratic leadership, perhaps many former nonvoters made the decision to begin to participate in the partisan contests.

However, new voter movements need not have been limited to a response to a national appeal, the "uniform" component of the model. Previous nonvoters could also be motivated to vote due to interactions with partisans. Such interactions can be both direct and indirect. The effectiveness of an established partisan apparatus in mobilizing the vote by establishing direct contact with potential voters is a result that was reported long ago by Gosnell (1927) and evidenced repeatedly since. Partisan interactions with the nonvoting population leading to the mobilization of new voters can also be indirect, however. By themselves, nonvoters can witness the partisan characteristics of their communities. In some areas in which Republican norms are very strong or, perhaps, where overall voting activity in either partisan direction is very slight (i.e., turnout is traditionally low), it may be that nonvoters lack the relevant localized partisan cues that would cause them to initiate the internal processes of becoming politically involved. However,

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in areas in which Democratic partisan activity is already established, some non-voters may follow the lead of their local political environment (keeping up with the Joneses, so to speak) and begin their trek to the polling booths. This addresses the "social" component of the new voter mobilization for the Democratic party.

Both the uniform and the social components of the new voter mobilization dynamics for the Democratic party can be included in the above model as

$$dD/dt = fR + bRD + mN + aND, \quad (2)$$

where N is the proportion of the electorate which is nonvoting, and m and a are parameters of the model. The parameters m and a , respectively, reflect the probability of mobilizing a Democratic supporter at an instant in time from within the localized pool of nonvoters due to the nonvoters' consideration of the appeal of the national campaign independent of local partisan traditions, as well as to the interaction between the nonvoting population and the local Democratic partisan environment.

Finally, it is possible that none of the components of the model describing fluctuations in Democratic partisan populations will completely capture the aggregate movements between Republicans and Democrats as well as between nonvoters and Democrats. This addresses the realistic considerations involved in mapping any model to a body of data. Thus it is important to include a constant term in the above expression that allows for Democratic population change independent of the included components. Equation 2 can now be rewritten as

$$dD/dt = fR + bRD + mN + aND + k, \quad (3)$$

or, for economy,

$$dD/dt = R(f + bD) + N(m + aD) + k, \quad (4)$$

where k is the constant element of the derivative and a parameter of the model.

It is important to note that while the analysis here refers to the above components of the model in terms of voters moving from one group to another, the components are actually capturing the net movements between groups. Thus, the parameter b captures the net interactively determined change between the Democratic and Republican parties. This "net" is, of course, the difference between the total of Republican-to-Democratic conversions and the simultaneous Democratic-to-Republican conversions. This is a characteristic of all models, both statistical and formal, which rely on aggregate-level data. There is no way for independent estimates to be derived that capture the changes in both directions.

However, the question remains as to whether this potential crisscrossing movement would seriously bias one-way interpretations that one might like to draw from the analysis. This depends on the degree of crisscrossing that actually occurred. The data used in this analysis clearly suggest that any potential interpretive bias would be extremely small. Using U.S. county-level aggregations, only eight (.3%) of the approximately three thousand counties experienced a decrease in the Democratic vote in 1932 combined with an increase in the Republican vote. If there had been a significant degree of crisscrossing, surely this would have been more detectable on the national level, ecological considerations notwithstanding. The situation is similar regarding Democratic and nonvoter crisscrossings in 1932. Only 2.4% of the counties experienced a decrease in the Democratic vote combined with an increase in the nonvoter populations. In 1936 there were larger number of counties experiencing decreases in the Democratic vote combined with increases in the Republican and nonvoting populations (34% and 22% respectively). However, *all* of these

cases occurred in farm areas, which are accurately accounted for in the conditioning analysis presented below. Thus the one-way movements of the electorate clearly seem to dominate these data. Moreover, this closely corresponds to a historical reading of the politics of the times as it is presented in the vast related extant literature. Thus while a discussion in terms of net movements among parties and between parties and nonvoters more closely fits the technical realities of any model that utilizes aggregate-level data, interpretations that phrase these movements in one or another direction probably do no injustice to a historical analysis of the politics of these years.

The model describing longitudinal change in the Republican party population can be developed similarly to that for the Democratic party. Republican defections to the Democrats (or in some cases gains from the Democrats) will occur either uniformly across the nation, independently of the strength of the local Democratic presence, or through the process of local competition in which the strength of the Democratic traditions within each community will condition the rate of partisan change (again, the so-called uniform and social components of the model). Furthermore, gains or losses for the Republicans due to nonvoter volatility can be similarly directed. Given the weak economic conditions of 1932, some Republicans may have simply stopped voting, thus joining the ranks of the uninvolved. Yet some nonvoters with Republican tendencies may have felt that it was time to get involved when their favorite party was under attack. These potential gains and losses for the Republican party could have taken place uniformly throughout the United States, dependent only on the availability of nonvoters in a given community. Alternatively, the nonvoter-Republican shifts may have been dependent upon the interactive effects of the nonvoter and Republican

populations. All such partisan and nonvoter trade-offs with regard to the Republican party can be captured with the statement

$$\begin{aligned} dR/dt = D(q - bR) + N(s + wR) \\ + j, \end{aligned} \quad (5)$$

where q , b , s , w , and j are parameters of the model. Here, the parameters q and s control the "uniform" inputs to changing Republican support, whereas the parameters b and w govern the "social" inputs. Note the negative coefficient for the parameter b . This results from its appearance in the statement for change in Democratic support over time (i.e., equation 4). The negative coefficient for the parameter b maintains the accounting compatibility of both models and has implications for the procedures used to estimate the parameters. The parameter j is the constant term of the derivative and is included to identify Republican partisan change that cannot be captured by the other components of the model.

Changes in the nonvoter population of the United States are due to new voter movements to or from the Democratic and Republican parties. New voters can be carried toward a particular party on a wave of national excitement, again, independently of existing local partisan strengths. However, new voters also can be drawn toward a party due to the influence of the local party apparatus or led by the partisan directional cues sensed from the surrounding environment. These two paths address the uniform and social components of the models and can be incorporated directly in a model describing longitudinal fluctuations in the nonvoter population. Thus we have

$$\begin{aligned} dN/dt = D(g - aN) + R(v - wN) \\ + y, \end{aligned} \quad (6)$$

where g , a , v , w , and y are parameters of the model and are identified with respect

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to the "uniform" and "social" components of the model as are the corresponding components in equations 4 and 5. The parameters a and w have occurred elsewhere and are included again here to preserve the population compatibilities of the overall system. The parameter y is the constant element of the derivative and describes change in the nonvoting population that cannot be isolated by the partisan-directed components of the model.

Equations 4-6 constitute an interdependent system of three nonlinear differential equations that together describe the mass electoral dynamics between the Democratic and Republican parties as well as between both parties and the nonvoting population. Throughout, the system includes both the "uniform" and the "social" components of population trade-offs between the three electoral groups (Democrats, Republicans, and nonvoters). The system has general properties and is entirely symmetric in tracing aggregate population shifts from any one group to all other groups. (Some characteristics of the above models written in reduced form are presented in Appendix A.)

While the system can be used to estimate the partisan and nonvoter trade-offs for the entire United States without modification, additional leverage can be gained by identifying different social conditions that could change the magnitude or direction of the partisan and nonvoter shifts. For example, we may be interested in how the system of electoral competition differed in urban areas as compared with rural areas. This can be done by conditioning the system with respect to a separate variable of interest—specifically, by writing each parameter as a linear function of this additional variable (referred to as the "conditioning" variable). Thus, using the parameter f as an example, we can rewrite the parameter using the form

$$f_0 + f_1X.$$

Writing all parameters in this fashion restructures the dynamic characteristics of the overall system with respect to the conditioning variable X (see also Jackson 1987).³ The unconditioned parameters are recovered under the conditions where $X = 0$. The complete system is now

$$\begin{aligned} dD/dt = & R[(f_0 + f_1X) + (b_0 + b_1X)D] \\ & + N[(m_0 + m_1X) + (a_0 + a_1X)D] \\ & + (k_0 + k_1X) \end{aligned}$$

$$\begin{aligned} dR/dt = & D[(q_0 + q_1X) - (b_0 + b_1X)R] \\ & + N[(s_0 + s_1X) + (w_0 + w_1X)R] \\ & + (j_0 + j_1X) \end{aligned}$$

$$\begin{aligned} dN/dt = & D[(g_0 + g_1X) - (a_0 + a_1X)N] \\ & + R[(v_0 + v_1X) - (w_0 + w_1X)N] \\ & + (y_0 + y_1X). \end{aligned}$$

The Data and Estimation

Estimating the parameters in this system is a nontrivial problem. However, an extensive literature does exist that addresses such problems as well as an entire class of related issues. Standard approaches using regression techniques are of no utility here. It is not possible to solve for D , R , and N explicitly. Tuma and Hanna (1984), as well as Coleman (1981), offer linearizing techniques that are useful for analyzing simpler models. These techniques are pursued primarily to recover known statistical properties of the estimators but require algebraically approachable systems allowing the uncoupling of equations. Rather, the model must remain in derivative form and estimations must be obtained using numerical techniques to obtain longitudinal population trajectories for each group. In the social sciences, examples of the use of such techniques include estimations of systems of difference and differential equations addressing concepts of partisan competition (Brown 1987; Przeworski

and Sprague 1986), as well as systems of equations modeling arms race competitions (Ward 1984). The procedures are commonly described in the engineering literature and are often used to solve practical problems employing systems of interdependent differential equations. A lucid summary of such techniques, as well as an introduction to the broad literature on the subject, can be found in Hamming 1971 as well as Dennis and Schnabel 1983. A more complete description of the techniques as they are used in this analysis is contained in Appendix B.

The data used to estimate this system are the electoral returns for all counties in the United States from 1928 to 1936.⁴ There are approximately three thousand counties in the United States. All population measures for each group (Democrats, Republicans, and nonvoters) are written as proportions of the eligible electorate (21 years and older). Census information for all counties has been merged with the electoral data to produce an unusually complete collection of U.S. data for that time period.⁵

Three conditioning variables are used in this analysis. They are (1) urbanization (the total urban population measured as a proportion of the total population), (2) the level of farm activity, measured in terms of total county acreage under farm cultivation, and (3) the average number of wage earners in manufacturing industries during 1929, measured as a proportion of the total population. These three conditioning variables are defined as in the 1930 U.S. census, and are included here because their significance to the politics of the period has been repeatedly addressed (not always with parallel interpretations) by the topical literature (Clubb 1978, 75-79; Ladd and Hadley 1978, 66; Lubell 1965, 57; Petrocik 1981, 57; Sundquist 1983, 217).

While other conditioning variables could be used (and others were in analyses not reported here), these three variables

seem of particular heuristic value in identifying major differences in the nature of the partisan competitions along lines around which there are prior expectations. For example, contemporary Democratic support is located in large part in urban settings. However, previous to the 1930s, Republicans depended heavily on urban support. How that transfer from Republican to Democratic urban strength took place, and under which dynamic settings is a question of importance substantively. For example, it is of interest to inquire whether the onset of the depression sparked a realignment of existing urban voters in 1932 or rather, whether it caused an upsurge in new voters to swell the Democratic ranks in such areas. Similarly, farmers were among the first groups to be affected by the depression. An examination of the dynamic characteristics of the partisan competitions in farm areas will address questions of whether large sectors of the farm-related population saw the 1932 and 1936 elections as times to abandon the Republican ship, or as times to board the Democratic movement from the quiet land of the previously uninvolved. The working populations fill a similar role in this analysis. Comparing the nature of the partisan competitions in both working and farm areas allows for an examination of crucial differences between diverse populations both in the timing and the manner of their movement to the Democrats. While both the working and farm populations were heavily affected by the depression, it is of interest to know whether differences with the economic, social, and political conditions for each group encouraged dissimilar partisan dynamics.

All of the conditioning variables are standardized to a mean of zero and a standard deviation of one before being entered into the analysis. The transformation inspires the intuitive interpretation of the conditioning variables as measures of the social milieu, recognizing that small

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changes in a social environment (subjectively detectable as a shift in local ambience) can result in more dramatic electoral consequences (see also Blau 1977; Blum 1985; Simmel 1955).

The period from 1928 through 1936 is broken up into two separate time spans. Thus the model is evaluated based on its ability to explain longitudinal county-level population fluctuations for the Democrats, Republicans, and nonvoters for the period from 1928 through 1932 and then again from 1932 through 1936. We are, of course, anticipating that the model will detect and reveal underlying systemic patterns from within the data. However, the system evaluated here measures *change* between and among partisan and nonvoter populations. In situations in which little systemic change has occurred nationally, the model should perform poorly with regard to the measure of fit. In such a case, local noise would dominate. On the other hand, in those cases in which the model does discern a significant national trend in terms of the voter and nonvoter trade-offs, the measure of fit should reflect the system's success in characterizing the period's mass dynamics. Throughout all of the analysis, each case (i.e., county) is weighted by its eligible population.

This analysis examines the mass dynamics of the electoral movements during an unusual period of time in U.S. history. Since available survey data for this period are of questionable reliability, aggregate measures have been used extensively in the extant realignment literature in an attempt to unravel some of the electoral mysteries of those years. This analysis pursues an unusual treatment of the same type of data analyzed elsewhere. This analysis finds correspondence with results by Kramer and others that suggest that the analysis of dynamic aggregate data can be superior to cross-sectional survey data in determining individual-level behavior when the examined be-

havior involves a systematic longitudinal component (Kramer 1983; see also Barrilleaux 1986; Campbell 1986; Irwin and Lichtman 1976; Parent, Jillson, and Weber 1987; Powell 1986; Sprague 1976). The present analysis is formulated from the perspective of aggregate population trade-offs between groups. The expectations of the model rest with the direction of change in aggregate group memberships. Where aggregate relationships are strong, the corresponding expectations regarding individuals are buttressed from the perspective of the characterization of the mass dynamics.

Results

The results of the parameter estimations for the system are contained in Tables 2-5. The estimates are separated by time periods as well as by the conditioning variables used in the analysis. On the left sides of the tables are the estimates for the 1928-32 period. The estimates for 1932-36 are displayed on the right sides of the tables, next to the estimates for the earlier period. The fits of the models with regard to each group (Democrats, Republicans, and nonvoters) are also included in the tables. The chi-squared statistics test the statistical significance of each estimate in terms of its influence on the prediction surface generated by the overall system and are explained more thoroughly in Appendix B.

Substantive interpretations of the system's characterization of the mass dynamics of the period are difficult to obtain solely from an examination of Tables 2-5. However, while the parameter estimates are used to produce a graphical analysis of the overall system, an interpretation of a few of the estimates is useful heuristically as an introduction to how the graphs are produced. Note in Table 2 that the estimates for the parameter *b* between both periods (in the unconditioned case) change dramatically in magnitude. Recall

Table 2. Unconditioned Parameter Estimates

Parameters	1928-32		1932-36	
	Estimates	Chi-Squared (df = 2)	Estimates	Chi-Squared (df = 2)
F	.17663	552,344	.16550	261,216
B	.28495	210,164	.00220	15
M	.05874	202,611	.04801	78,357
A	.21841	218,718	.08603	44,440
G	.06158	39,570	-.07332	115,710
Q	-.04302	34,852	.05017	94,374
S	-.02308	32,020	-.00871	4,170
W	-.18302	154,735	.04190	4,571
V	-.11234	129,542	-.14990	200,499
K	-.00918	22,113	-.00349	2,369
J	-.01609	70,537	-.00461	6,737
Y	-.02275	79,659	-.00806	11,773

Note: 1928-32 models and fits: Republican, .72210; Democratic, .70980; nonvoter, .29513. 1932-36 models and fits: Republican, .10401; Democratic, .46233; nonvoter, .59062.

Table 3. Urban-conditioned Parameter Estimates

Parameters	1928-32		1932-36	
	Estimates	Chi-Squared (df = 2)	Estimates	Chi-Squared (df = 2)
F1	-.041184	73,638.4	.016983	6,436.4
B1	-.013620	1,035.9	.007756	447.3
M1	.006823	6,293.5	.017480	19,756.1
A1	.003570	154.7	.009544	1,354.1
G1	.012993	4,157.1	-.013117	9,240.0
Q1	.045386	89,041.7	-.018512	31,892.4
S1	-.014611	27,723.4	.007009	5,468.0
W1	.008769	949.4	.001171	10.6
V1	.009201	2,043.3	-.005351	638.6
K1	-.001442	1,323.4	.000179	13.5
J1	-.000850	445.5	-.000590	254.3
Y1	-.003496	4,321.9	-.001291	699.7

Note: 1928-32 models and fits: Republican, .75175; Democratic, .73101; nonvoter, .30703. 1932-36 models and fits: Republican, .16159; Democratic, .533219; nonvoter, .61885.

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Table 4. Workers-conditioned Parameter Estimates

Parameters	1928-32		1932-36	
	Estimates	Chi-Squared (df = 2)	Estimates	Chi-Squared (df = 2)
F1	-.056730	50,843.8	.018393	2,814.2
B1	-.025263	1,153.9	.008776	197.6
M1	.004884	994.3	.025507	13,335.3
A1	-.003974	57.8	.011860	611.7
G1	-.007105	380.4	-.014155	3,175.2
Q1	.081014	82,174.5	-.030970	26,567.4
S1	-.018781	13,699.8	.010886	4,112.4
W1	-.034929	5,483.0	.002252	14.8
V1	.030527	8,471.5	-.007042	402.6
K1	-.002115	908.5	.000393	21.1
J1	-.001122	241.1	-.000650	98.1
Y1	-.003614	1,518.3	-.001862	460.6

Note: 1928-32 models and fits: Republican, .76030; Democratic, .74926; nonvoter, .32356. 1932-36 models and fits: Republican, .17262; Democratic, .52578; nonvoter, .61129.

Table 5. Farm Density-conditioned Parameter Estimates

Parameters	1928-32		1932-36	
	Estimates	Chi-Squared (df = 2)	Estimates	Chi-Squared (df = 2)
F1	.049522	5,045.8	-.055609	4,333.1
B1	.035395	256.4	-.022492	183.7
M1	-.003735	259.9	-.012324	1,572.3
A1	.012268	227.9	-.013577	392.4
G1	-.039536	3,149.1	.031656	4,860.7
Q1	-.074097	20,940.7	.032871	9,013.1
S1	.017093	5,855.5	-.019620	6,467.1
W1	-.032939	1,168.3	-.002274	3.6
V1	.038948	1,789.7	.018547	447.5
K1	.001887	215.5	-.003577	602.0
J1	-.000359	8.5	.001869	265.2
Y1	.003798	506.7	.001154	58.7

Note: 1928-32 models and fits: Republican, .78489; Democratic, .73126; nonvoter, .31185. 1932-36 models and fits: Republican, .15693; Democratic, .54827; nonvoter, .63657.

that the parameter b represents gains for the Democrats due to interactive losses (i.e., the "social" component of the model) from the Republicans. These results suggest that the Democrats gained heavily from the Republicans in this fashion during the 1928-32 period but not nearly so heavily during the later period. Similarly, the estimate for the parameter q changes substantially from the early to the later period. In this case, the magnitude remains approximately the same, but the sign changes. Again, the parameter q represents the "uniform" component of the model describing aggregate group change from the Democrats to the Republicans. These results suggest that Republican "uniform" losses did correspond with Democratic gains in 1932 but not in 1936. Indeed, this one parameter value (examined in isolation from the remainder of the system) seems to indicate that there were some uniform gains for the Republicans from the Democrats in 1936. (The graphic analysis below will show that some reverse movement did in fact occur in farm areas in 1936.)

An examination of the fits for the unconditioned cases similarly provides some useful initial interpretive guidelines with regard to the entire system. For the period from 1928 to 1932, note that the models account for a considerable amount of variance in the longitudinal population fluctuations for the Republican and Democratic parties. Note also that the model that characterizes change in the nonvoting population does less well during that time period. Compare these results with those obtained using the unconditioned system for the period from 1932 to 1936. While the model describing change for the Democratic party continues to account for a reduced but still substantial degree of variance in the aggregate Democratic outcomes, the model for Republican change now performs poorly. On the other hand, the model for change in the nonvoting population does very well.

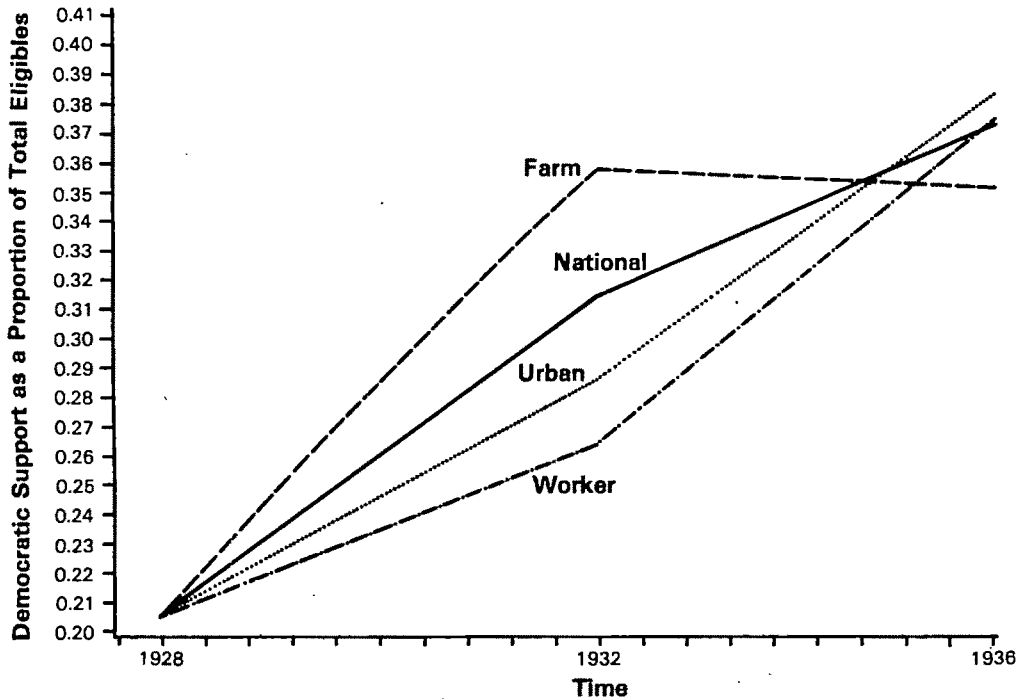
These initial results can only offer very tentative guidelines as to the interpretations of large-scale aggregate movements. However, they do suggest that during the 1928-32 period, the greatest aggregate movement on the county level was between Republicans and Democrats, as the model for nonvoter change encounters relatively less national trend and more local "noise" for that period. But in the 1932-36 period, the situation seems to have reversed. These results suggest that it is activity between the Democratic and nonvoter population groups that dominates the later period.

A more comprehensive description of the characteristics of the estimated system under the influence of the various conditioning variables can be obtained using a graphic analysis. The simplest form of such an analysis is a straightforward longitudinal plot of support for a particular party. Figure 1 presents such a plot for the Democratic party. There are four model-generated time paths presented in Figure 1. The path labeled "National" is the longitudinal trajectory for the Democratic party using the unconditioned estimates found in Table 2. This path is included in Figures 2 and 3 as well and serves as a base line from which to evaluate the other trajectories. The time paths labeled "Farm," "Urban," and "Worker" represent the system's predicted level of voter support for the Democratic party in areas that can be described as primarily farm-oriented, totally urban, and heavily working class in character.

Note that from 1928 through 1936, the Democratic party's strength continued to grow nationally (i.e., as seen using the trajectory labeled "National"). However, the greatest growth for the Democratic party between 1928 and 1932 was in the farm areas of the country and there seems to have been no additional growth in Democratic support from such areas after Roosevelt's initial election. However, Democratic support coming from heavily

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Figure 1. Democratic Support from 1928 to 1936

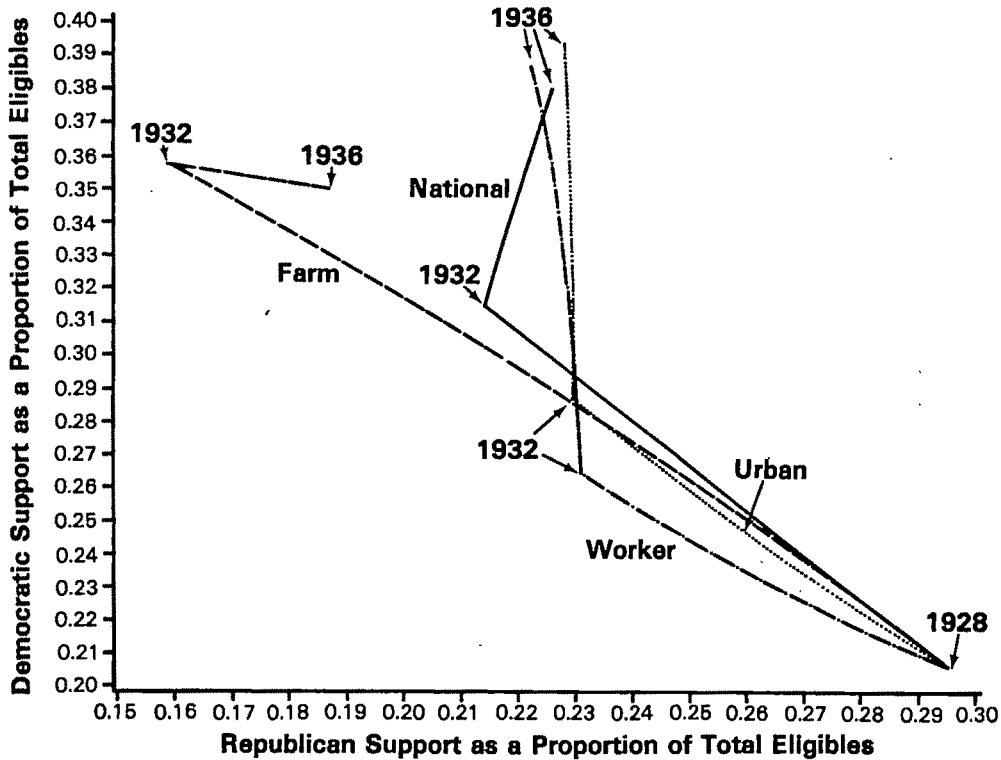


urban and "working class" areas exhibits an opposite pattern. Between 1928 and 1932, such areas contributed less to Roosevelt's first victory than did many of the other sections of the nation. Indeed, areas with high concentrations of workers produced the smallest growth in Democratic support in 1932. These results are of interest considering the focus of much of the realignment literature for the U.S. on the urban and working class basis of the Roosevelt coalition (see Degler 1971, 141; Petrocik 1981, 53). However, note that after the 1932 election, Democratic support from urban and working class areas increased dramatically, marginally surpassing the overall national strength of the party and completely eclipsing the now-stagnant growth from the farm areas. These results for 1936 seem to support arguments by Sundquist and some others (Sundquist 1983, 218-19; see also

Lubell 1965, 57-63) that the urban and working class elements of the Roosevelt coalition did not emerge in their full form until after 1932. However, the trajectories in Figure 1 suggest surprising differences in the manner and timing in which various groups turned to the Democrats. In particular, the magnitude of the differences between the farm areas as compared with the urban and working class areas for 1932 is quite striking given the claims by some authors that the growth in Democratic support in 1932 was primarily affected by a broadly based sense of dissatisfaction (cutting across all groups) with the previous Republican leadership (Ladd and Hadley 1978, 87). Moreover, the dramatic reversal of these patterns after 1936 is noteworthy in terms of its magnitude and deserves further explanation.

A problem with Figure 1 is that it does

Figure 2. Democratic and Republican Trade-offs, 1928 to 1936



not allow for an examination of the relative trade-offs between groups; that is, it is not clear whether the growth in Democratic support coincides with an increase in new voter strengths or a decrease in local Republican fortunes. These trade-offs can be seen more clearly in an analysis of a type of graph called a "phase diagram." Figure 2 is a phase diagram representing change in Democratic and Republican support from 1928 through 1936. As with the other phase diagram described below, each curve on the plot represents simultaneous change in two separate populations. In Figure 2 the horizontal axis identifies Republican support from the pool of eligibles while the vertical axis represents Democratic support.

Note that each curve on the plot is labeled with respect to whether it is an un-

conditioned ("national") or a conditioned ("farm," "urban," or "worker") trajectory. Also note that each curve on the figure is labeled with regard to the three election years 1928, 1932, and 1936. As one visually follows any one of the curves (all beginning in 1928 and originating in the lower-right-hand corner of the figure) upwards and to the left, it is possible to examine the voter trade-offs between the two groups over time. For example, follow the curve labeled "National" diagonally up from its starting point in 1928 until its sharp bend upward in 1932. This part of the curve indicates that from 1928 to 1932 there was a large drop in national Republican support combined with an approximately equal gain in Democratic support, all computed using the entire estimated system (thus simul-

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taneously controlling for all partisan-nonvoter shifts as well). Continuing up the curve from 1932 to 1936, there appears to be very little movement in Republican support (shown by the near vertical nature of the curve after 1932) and a substantial increase in national Democratic support. When a curve cuts a dramatic diagonal across the surface of a plot, this indicates a substantial simultaneous movement between the two populations represented on the figure's axes. When a curve is placed horizontally or vertically in the figure (or nearly so, relative to the other curves), this indicates little or no aggregate movement between the two populations (i.e., one population stayed constant while the other either increased or decreased).

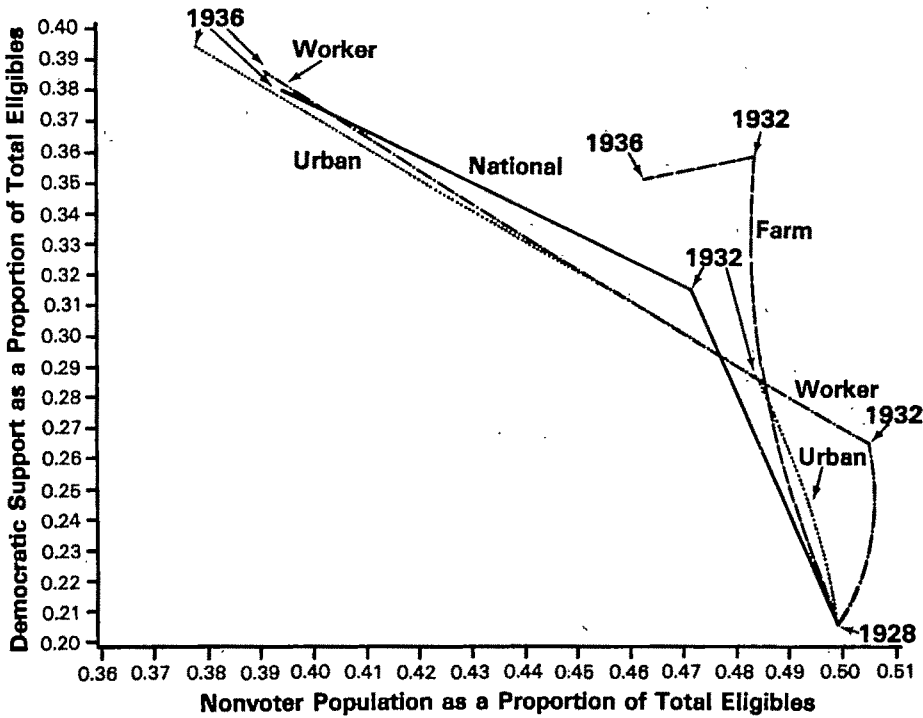
While the curve for the national-level trade-offs is included in all phase diagrams for reference, the other curves tell the more interesting story. In Figure 2, note that the curve representing the system of competition as it occurred in predominantly farming areas marks a very dramatic diagonal sweep across the surface of the plot from 1928 to 1932. This suggests that a great deal of formerly Republican support coming from farm areas in the United States abandoned the Republican party and joined the ranks of the Democratic voters. Moreover, the magnitude of the Republican loss in farming areas, combined with that of the Democratic gain, surpasses those for urban or working class areas as well as for the national average. Such a strong movement away from the Republicans and toward the Democrats in farming areas probably had its origins in historical conditions reaching back more than 10 years. Recall that the La Follette movement in 1924 registered marked discontent with Republican farm policies (Sundquist 1983, 182-91) and that Calvin Coolidge did once say, "Well, farmers never have made money. I don't believe we can do much about it" (White 1965, 344). It seems that

the onset of the Great Depression was the last straw for many farmers. While the Republicans may have maintained a substantial degree of farmer support throughout the 1930s, they do seem to have lost a good part of their earlier support in 1932. However, note that the Republicans did bounce back somewhat in the farm areas after 1932. This suggests that many of the switching farmland folks of 1932 stayed Democrats in 1936, but a few returned to their Republican moorings.

The pattern between the Democrats and the Republicans in farm areas differs remarkably from that in urban and working class areas. Figure 2 suggests that among urbanites there was a substantial Republican loss combined with a large Democratic gain primarily in 1932. However, in areas in which there were large numbers of workers, Republican support dropped between 1928 and 1932 with less than an equivalent Democratic gain. After 1932, Democrats seemed to have scored heavily from such areas without a concomitant Republican loss of comparable magnitude. This raises the question of what these working class voters did in 1932. While they seemed to have abandoned the Republican party in large numbers in 1932, they do not seem to have been as fundamental a component of the Democratic gains until after 1932.

This puzzle regarding working class trade-offs between the Democrats and the Republicans is answered in large part with Figure 3. Figure 3 contains the longitudinal trade-off population trajectories for the two groups, Democrats and nonvoters. The horizontal axis represents the non-voting population, measured as a proportion of the total electorate; whereas the vertical axis represents support for the Democrats (as in Figure 2). Note that the curve labeled "Worker" moves upwards and somewhat to the *right* after its beginning in 1928. From 1932 to 1936, the curve makes a marked change and moves diagonally upwards and to the left. These

Figure 3. Democratic and Nonvoter Trade-Offs, 1928 to 1936

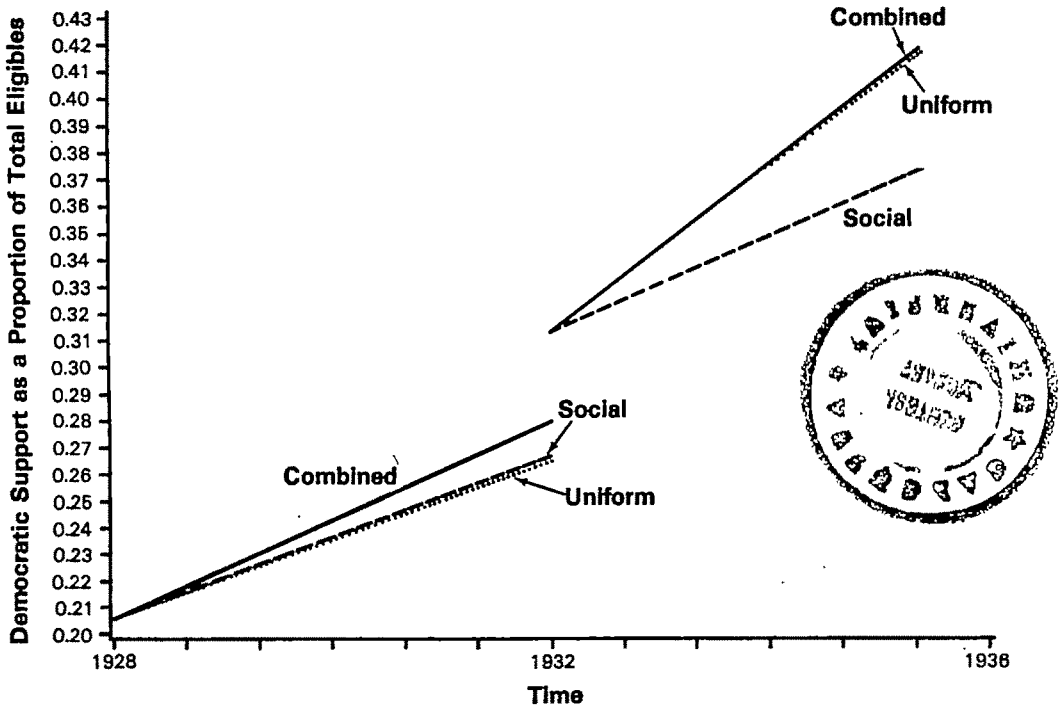


results suggest that voters in heavily working class areas experienced no increase in mobilization in 1932 (and perhaps a degree of demobilization). This result, interesting in terms of the magnitude of the later shift, is not completely without interpretation guided by some of the extant realignment literature of the period. Recall that the 1932 election was not a radical departure from other elections in the 1920s with respect to Roosevelt's campaign rhetoric (Ladd and Hadley 1978, 38; Lubell 1965; Sundquist 1983, 208-10). Indeed, it was not until Roosevelt came to power that the shape of the future was so clearly directed in New Deal terms (Petrocik 1981, 53-54). While the depression made the Republicans an unpopular party in 1932, the Democrats were not clearly identified as a party of

economic and social salvation (Key 1964, 523-24). The results of Figure 3 suggest that workers did not initially rise up and attempt to throw the Republicans out of the White House, regardless of whether or not they blamed that party for their own depression-related economic misfortunes. Rather, they remained inert, and some may have even stopped voting, withdrawing from political participation just as they had begun to withdraw from their participation in the national economy. This may have been tied to the dramatic decrease in the unionized work force at the time. They may have been struck by anger at their plight, but all were not motivated (or perhaps organized) to register their anger at the polling booths. One can only suspect that they did not really know where to turn for assistance

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**Figure 4. Simon Bounds for Democrats from Republicans
(Urban-conditioned)**



in 1932. They were conceivably more interested in their own domestic situations than in politics, and they were perhaps unsure of what any government could do for them, given decades of Republican political dominance and a previously widespread public acceptance of the principle of nonintervention in the private economy. But in 1936 they saw the difference between the parties, and partisan politics in the United States experienced a massive infusion of new as well as formerly demobilized voters from working class areas.⁶

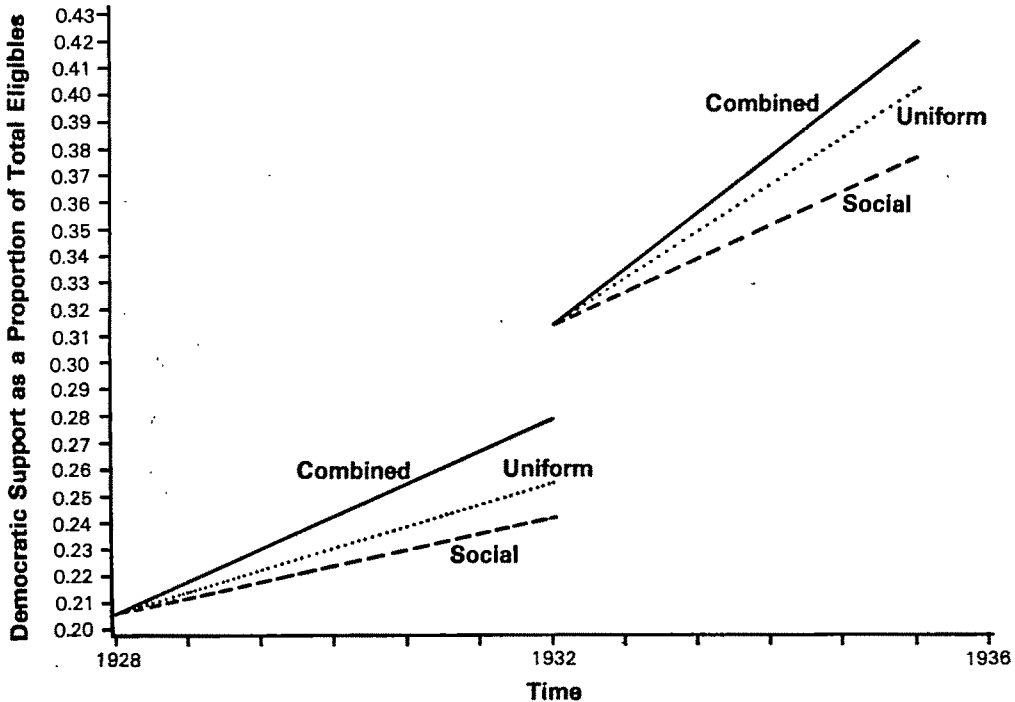
Figure 3 suggests a related story for urban voters (of whom many were workers). There was little overall movement to the Democrats from the pool of nonvoters in urban areas between 1928 and 1932, as is indicated by the near vertical course of the curve labeled "Urban" through 1932

(although there was certainly variation between many particular urban areas, of course). Yet after 1932 the diagonal movement for the curve indicates that the urban areas experienced heavy Democratic mobilization among new voters. Voters in farm areas acted in an entirely different fashion. The Democrats managed to mobilize some new voters in 1932 from such areas. However, in 1936 the Democrats had no comparable success from the farming areas. Indeed, a fraction of their 1932 farmland supporters failed to turn out at all.⁷

The Relative Impact of the Mechanisms for Change

Figures 4-7 change the focus of this analysis to ask a question based on the

Figure 5. Simon Bounds for Democrats from New Voters
(Urban-conditioned)



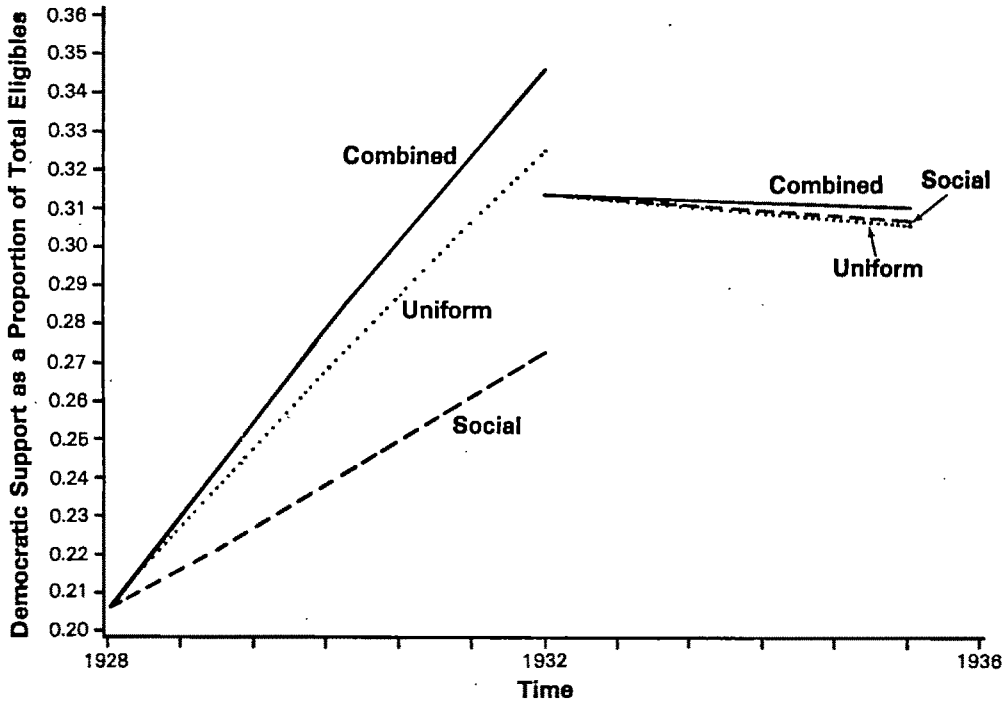
structure of the model explored here. Within each mathematical statement characterizing growth and decay for the partisan and nonvoter populations, there are both uniform and social components. It is descriptively useful as well as analytically important to evaluate the relative impact of the two components. Figure 4 displays what is titled the "Simon bounds" for aggregate partisan change for the Democrats from the ranks of the Republicans. Figure 5 contains a similar representation for partisan change for the Democrats from new voters. Both Figures 4 and 5 are drawn with respect to the urban-conditioned environment. Figures 6 and 7 are a similar representation for the farm-conditioned environment. The term *Simon bounds* references an early work by Herbert A. Simon (1957), in which

substantive meanings were explicitly tied to formal mathematical expressions of social change of the type used here. In all figures, time is on the horizontal axis and Democratic support as a proportion of the total eligibles is on the vertical axis.

To begin with an interpretation of Figure 4, the curves labeled "Combined" represent the total longitudinal gains for the Democrats in urban areas and are identical to the urban-conditioned trajectories presented under the "Urban" label in Figure 1. The lines labeled "Uniform" and "Social" are computed using only the system's uniform or social components (respectively) as inputs to the joint Democratic and Republican totals. For example, to compute the Democratic vote represented by the lines labeled "Uniform" in Figure 4, the parameter b was set to zero.

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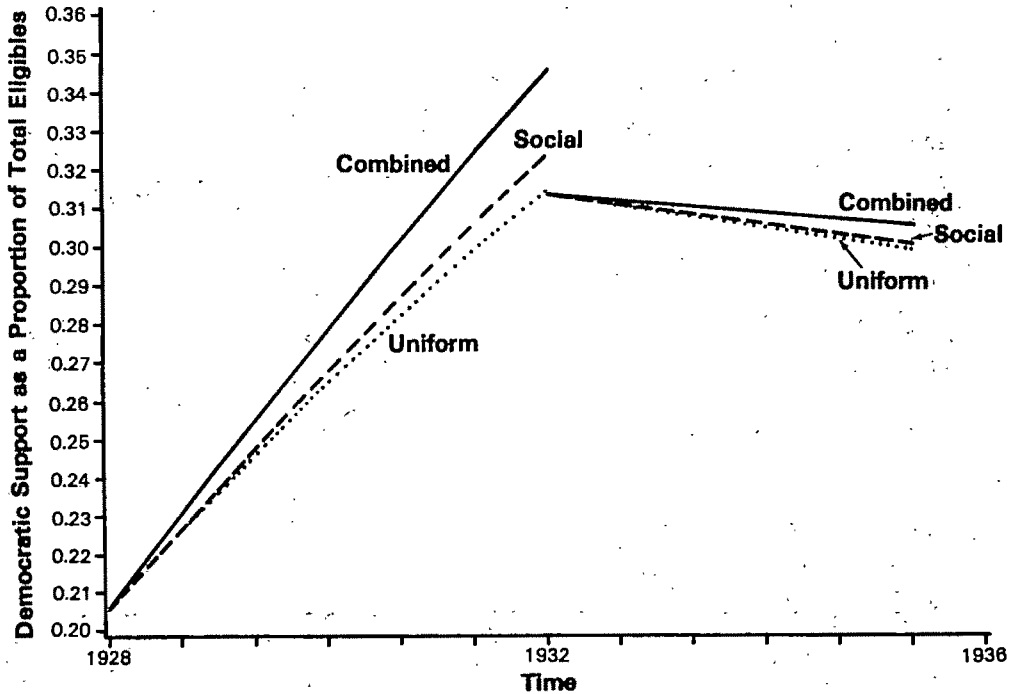
Figure 6. Simon Bounds for Democrats from Republicans
(Farm-conditioned)



This parameter represents the probability of recruiting a Republican for the Democrats at an instant in time due to the interactive influences of the two existing partisan populations. Setting this component to zero leaves only Democratic gains due to nationally uniform Republican defections (i.e., parameters f and q , mediating the uniform components of the expressions for Democratic and Republican change). Alternatively, the lines labeled "Social" are computed by setting the uniform components between the Republicans and the Democrats to zero, leaving only the social component in the model. Briefly, the lines labeled "Uniform" in Figure 4 use only the uniform components of the model as inputs to the nationally changing partisan totals, whereas lines labeled "Social" use only the social components.

For urban environments during the period from 1928 to 1932 in Figure 4, note that the social component of the model contributes only a very slightly larger share of the national Democratic vote gain than the uniform component. This is indicated by the height of the social time path relative to the height of the uniform time path. Both the uniform and the social paths are lower than the combined path since the former both contributed to the national totals, and the absence of either would produce a lower outcome. This suggests that Democratic gains from the Republicans in 1932 in urban areas were approximately equally dependent on the localized abilities of the Democratic party to compete (i.e., converting more Republicans where the Democrats are strong and fewer Republicans where the Democrats are weak) as on a more uniformly

Figure 7. Simon Bounds for Democrats from New Voters
(Farm-conditioned)



distributed sense of dissatisfaction with the Republicans.

The time paths on the right of Figure 4 (for the period from 1932 to 1936) indicate that the uniform component was dominantly responsible for any further Democratic gains from the Republicans in urban areas. This is evidenced by the much greater height of the uniform time path relative to the social time path. Recall from Figure 2 that there were very few Democratic gains from the Republicans in urban areas in 1936. The results of Figure 4 suggest that the Democrats did not have to rely on their own localized partisan strengths in order to maintain those gains.

Figure 5 contains the Democratic time paths identifying the "Simon bounds" with respect to Democratic gains in urban areas from the new voters. Comparing the

1928-32 with the 1932-36 period, note that the relationship between the uniform and social time paths is similar between periods. In both periods, Democratic gains from the new voters were relatively independent of local Democratic party strengths. Apparently these new voters (fewer in 1932 than in 1936) were driven by the national sense of crisis and (especially in 1936) the overall Democratic appeal. In other words, Democratic mobilization of new voters in urban areas was relatively less dependent on existing local Democratic partisan strengths.

Recall from the discussion of Figures 2 and 3 that Democrats gained a great deal of Republican support as well as a substantial amount of new voter support from farm areas in 1932. Figures 6 and 7 present the Simon bounds for Democratic

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gains from the Republicans and the new voters within farm-conditioned environments. The trajectories on the left side of Figure 6 indicate that the Republican-to-Democrat defections in 1932 were not a result of locally dependent partisan competitions. This is evidenced by the much greater height of the uniform trajectory relative to the social trajectory. Following 1932, there was much less of a difference between the separate components of the competition. But the Democrats did not gain much from the farm-based Republicans in 1936 (in fact, they seem to have lost support there).

Figure 7 reveals an interesting difference from the pattern presented in Figure 6. From the new voters in 1932, the dominant component of the Democratic gains came from the local partisan competitions in areas where existing Democratic organizations could successfully mobilize former nonparticipants. In this figure, the social component has a substantially higher trajectory than the uniform component. The same pattern exists after 1932, but the relative impact of farm-based new voters on Democratic gains in 1936 was slight overall (from Figure 3).

In summary, Figures 4-7 present an interesting and contrasting picture of the dynamics of partisan competitions in urban and farm environments. In the urban areas, the movement toward the Democrats by new voters was a phenomenon that occurred in large part uniformly throughout these areas, relatively independent of existing Democratic strengths. Among Republican-to-Democratic conversions in 1932, the effects of the uniform and social components were more or less equal. In farm areas the situation was also complex. Uniform movement dominated among Republican-to-Democratic conversions. However, new voters from farm areas were most easily brought into the Democratic ranks when the local Democratic presence was relatively strong.

The nature of this recruitment as described above corresponds to findings reported elsewhere by Beck (1974) regarding the environmental properties of county-level partisan competitions during periods of dealignment. In farm areas, the early Democratic success came from former Republicans across-the-board (i.e., "uniformly") who were fed up with Republican farm policies (or perhaps the lack of them) and some new voters in locations in which there was an existing Democratic presence. Thus Roosevelt's relatively "gentle" campaign, filled with farm-oriented themes, aided local Democratic mobilization efforts among new voters in rural areas. But the already-mobilized former Republicans did not need the party contact. They jumped ship more uniformly than the new voters, and their impact on the early Democratic success was much greater than that of the new voters.

It was only after 1932 that the New Deal coalition began to form around identifiable centers of Democratic strength in urban areas. When this occurred, the movement was relatively less dependent on local partisan competitive abilities than on the national momentum of the times. The campaign of 1936 did not represent the politics of the former status quo. The Democrats had a new and coherent message, a message that had direct appeal to the urban masses. It seems that the radical nature of the message transformed the campaign from one where local party organizations were a major factor in the processes of converting Republicans (as in 1932 for urban areas) to one in which the party more accurately benefited from the windfall resulting from the newly energized and volatile electorate.

These results for farm and urban areas suggest that during periods of rapid change in mass voting patterns party organizations are more likely to be victims or beneficiaries, than causes, of the

mass movements. When the movements are of smaller magnitude, the activity of the party organizations have much greater impact. However, large aggregate movements, especially involving new voter activity as with the Democrats in 1936, could also be seen as the beginning of the renaissance of local party apparatuses, reenergized by the masses after years of decay with little national-level guidance. When partisan politics settled down, the enhanced organizational strength of the Democratic party was certainly a factor in the dominance of the party in national politics until the 1950s.

Remarks

In this study I develop and explore a model of mass political behavior with respect to the realigning period in the United States from 1928 to 1936. The analysis suggests that vote switching from the Republicans to the Democrats dominated the 1932 part of the realignment. However, discontent with the Republicans was not distributed equally or even near equally across social groups in the nation. Voters in farm areas—both former Republicans and some new voters—joined the Democratic ranks in large numbers. But urbanites responded to the Democratic appeal with less enthusiasm in 1932. Republican urban voters converted at a rate lower than that for the nation as a whole, and the nonvoters in these areas generally stayed put. Moreover, these results suggest that some workers may have even withdrawn from active participation in the electorate in 1932 rather than be drawn toward an emergent Democratic party that had not yet developed a clear identity.

The election of 1936 changed the character of Democratic politics. Some voters from the farm areas returned to the Republican fold. But urbanites and workers flooded the Democratic party's ranks. Moreover, these new Democrats of 1936

were also predominantly new voters.

The model developed here characterizes the nature of the partisan competitions as a dynamic process containing both nationally uniform and locally interactive partisan ingredients (the so-called uniform and social components of the model). In a situation of national crisis combined with large-scale mass movements, the uniform components often dominate, especially given weaknesses in the local party structures of at least one party. But when the mass movements are less severe but the political climate is favorable, the social components of partisan mobilization can yield substantial gains as well. In either case, the rejuvenation of the party organization as linked to the resurgence in its national fortunes must certainly enhance the party's long-term prospects of securing the attachments of these newly oriented partisans.

Appendix A

All of the models (i.e., the entire system) can be rewritten in reduced form. For purposes of explanation (since all three derivatives have similar structural forms), the present discussion is limited to the mathematical statement for dD/dt . Taking advantage of the identity $D + R + N = 1$, solving for N and making the substitution into equation 3 produces

$$dD/dt = fR + bRD + m(1 - R - d) + aD(1 - R - D) + k.$$

This simplifies to

$$dD/dt = (k + m) + D[(a - m) + (b - a)R - aD] + (f - m)R,$$

or, in reduced form,

$$dD/dt = \beta_0 + D[\beta_1 + \beta_2 R - \beta_3 D] + \beta_4 R. \quad (A-1)$$

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All of the right-hand-side components of equation A-1 are standard features of many such reduced form models, which are described in the literature on dynamic modeling (Danby 1985; Haberman 1977; Hirsch and Smale 1974; Huckfeldt, Kohfeldt, and Likens 1982; Luenberger 1979; Nisbet and Gurney 1982). In reduced form (thus in the complete absence of the original structural equations), substantive interpretations might be proposed as follows: β_0 represents a term characterizing constant growth; β_1 describes growth based only on existent levels of D (exponential growth or decay); β_2 controls the interactive input to the model as either growth or decay; β_3 is a logistic limitation to growth, where growth in D slows as D approaches an upper bound; and β_4 represents noninteractive gain (or loss) dependent on existent levels for R . The problem with the reduced form expressions is that the above interpretations do not characterize the more complex social processes, such as are revealed through the imbedded (i.e., nonreduced) parameters found in the original structural equations. This problem is characteristic of all reduced form models, which leads to the natural desire to estimate complex systems in their structural representation. Hanushek and Jackson offer the following interpretation of the two forms: "The reduced form equations summarize the entire structural model in terms of the total changes expected in each endogenous variable from a change in any one of the exogenous variables. The structural model on the other hand 'explains' how those changes occur and describes the behavioral process underlying the predicted changes" (1977, 227).

It is not necessary to estimate the models in their reduced form. Indeed, for estimation purposes, the models are best left in their original structural form (with all of the model-to-model parameter interdependencies left explicit), thus allowing for statistical tests for all of the original

(nonreduced) parameters. This is a general problem of nonlinear parameter estimation toward which is directed a sizable body of literature in both the engineering and econometric fields (Bard 1974; Dennis and Schnabel 1983; Hamming 1971; Judge et al. 1982, 773-74; SAS Institute 1984, chap. 21). The precise methods of parameter estimation as they are used in this analysis are explained in Appendix B.

Appendix B: The Calculations

The iterative parameter estimation procedure used in this analysis has its roots in a broad literature spanning a variety of diverse disciplines. Perhaps the most lucid description of the general procedure can be found in Hamming 1971. The focus of the related literature is the merger of standard numerical approximation techniques with least squares unconstrained optimization procedures. Much of the relevant literature is identified in Dennis and Schnabel 1983 (pp. 364-70; see also Hamming 1973). Interested readers might also find useful Fletcher 1965, Powell 1964, and Dennis, Gay, and Welsh 1981a, 1981b.

The following description of the estimation technique used in this analysis is necessarily brief. A more thorough description of the technique (including software written in SAS) is available from me.

The estimation process begins by computing a trajectory using guessed (but plausible) values of the parameters (a process to be repeated often with different values). A Runge-Kutta approximation to definite integration is then used, the first step of which takes the form

$$D_{\text{NEXT}} = D + h(dD/dt),$$

to compute an overtime trajectory for each partisan population, where h is a small number. This iteration is repeated a fixed number of times to yield the next

election's predicted partisan total (in the above example, for the Democrats).

The predicted values for the Democrats, Republicans, and nonvoters are then evaluated with regard to their fit to the actual data. The fit is calculated as

$$FIT = 1 - (RSS/TSS),$$

where *RSS* is the residual sums of squares between the predicted and actual values for each group, and *TSS* is the total sums of squares measuring the total variation for each group between the two elections.

The partial derivatives of the fit surface with respect to all parameters are then computed as

(change in fit / change in parameter).

This is accomplished by disturbing each parameter around its present value and evaluating the concomitant change in fit. All of this is done for one parameter at a time.

The estimated parameter values are then moved through the parameter space in order to maximize the fit based on the directional information contained in the vector of partials for the fit surface. This movement is accomplished iteratively using the relation

$$B_{NEXT} = B + z(P),$$

where the vector of partials for the fit surface is *P*, the vector of parameter values is *B*, and *z* is a small number. The value of *z* can vary depending on the steepness of the overall fit surface and the proximity of the maximum to that surface. Movement in the parameter space continues until the maximum to the surface has been reached, as is indicated by an evaluation of the partials to the fit surface.

With any one attempt at arriving at the maximum of the fit surface, there is no guarantee that the achieved maximum is the global maximum of the surface. This is characteristic of all nonsmooth problems of this sort. The usual practice of

varying the initial parameter values as a safeguard has been followed here.

The chi-squared statistics for the parameters test the impact of each parameter on the predicted values for each group (i.e., Democrats, Republicans, and nonvoters) from which the measures of fit are derived. First, predicted values for each group are calculated using the optimal values for each parameter. Second, new predicted values are calculated after setting each parameter to zero (one at a time). The differences between the two population sets are used to compute the chi-squared statistics. Thus a low chi-squared value indicates that an estimate has little impact on the predicted surface (and thus the model) and that the null hypothesis that the parameter equals zero should not be rejected.

Interested readers can obtain a free copy of the computer program which was used for the above estimations by contacting the author at the BITNET address POLSCB at EMUVM1. The program is machine-readable and thoroughly annotated. All inquiries should include a BITNET address to which the response should be sent.

Notes

1. There are three basic reasons for the previous unavailability of a usable complete collection of U.S. county-level aggregate data. First, the electoral data are available from the Inter-University Consortium for Political and Social Research data collections in what amounts to a scattering of over two hundred separate data files. County-level returns for all states and all years have not been organized into a few accessible, large, data files. Second, the available data sets, although scattered, are still terribly complete. There are data for literally hundreds of parties, covering all U.S. congressional, presidential, and many state elections. The bottom line is that the variable names in the separate data sets are not comparable. One cannot simply merge the data sets since the variable names do not correspond in year or party. Third, the information that is necessary to merge the various data sets is contained in the variable labels. There are often hundreds of variable labels for the many data sets, each of which needs to be read individually, and from which new

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variable names need to be constructed. Thus the process of identifying particular partisan returns for all counties in the U.S. has been unusually difficult, given normal constraints of staff, budget, and personal wear and tear.

2. Since this analysis focuses on the time period from 1928 to 1936, some questions regarding some of the long-term aspects of the realignment remain unanswered. It is not clear whether a vote-switching realignment in 1932 implies a permanent reorientation of what would be thought of as the "normal vote" for many voters. Some have suggested that many of the switchers of 1932 returned to their earlier Republican habits by 1940 (Petrocik 1981, 57). Survey responses (were they available) could have been used to query the voters' inner psychological commitments to particular partisan labels and attachments. However, in the absence of such data, reliance is made on an examination of the voting patterns of the early period, identifying vote switching and nonvoter interactions but leaving open the question of whether or not both the 1932 and the 1936 elections represented a deviating electoral period for some voters in which previous partisan allegiances were subsequently reestablished.

3. Writing each parameter in this fashion is different from simply estimating the nonconditioned system separately, using select subsets of the aggregate data for each estimation. Writing the parameters as a linear function of a conditioning variable allows the use of all of the data, as well as an interpretation of the system based on progressive changes in characteristics of the social environment.

4. The data utilized here were made available by the Inter-University Consortium for Political and Social Research. Neither the original collectors of the data nor the consortium bear any responsibility for the analyses or interpretations presented here.

5. The problem of organizing the data described in n. 1 was accomplished by first printing (on magnetic disk) a listing of the many data sets. A Basic program was then constructed which "read" each of the listings. This program also wrote computer code in SAS based on the information from these listings. The result was a program written in SAS that recoded all variables in all data sets to have common names containing embedded information that had been extracted from the original variable labels. All of the data sets were then merged to produce the manageable set used here.

6. It seems unlikely that the results for the worker populations would be influenced to a large degree by an ecological aggregation effect, although such a possibility does exist with any analysis of aggregate data. The farm and urban results would appear less susceptible to such aggregation effects due to the relative homogeneity of these areas. However, the areas with large worker populations also tend to be heavily urban, and the results for the workers tend to have similar dynamic properties to that of the

urban areas. Moreover, subsequent checking with alternative statistical strategies ranging from simple weighted correlations to more complicated logistic structures indicates that the present description of worker behavior is not a product of the particular model specification used here but rather a real structural characteristic of these data.

7. A conditional analysis similar to the above was also performed to test for differences in the partisan and new voter dynamics for the southern states as compared with the nonsouthern states. This analysis is not included in the body of the text only for reasons of space. However, the basic result is that there were no large differences in the directions of partisan and nonvoter change between southern and nonsouthern states (see also Shively 1971-72). This is not to say that the magnitudes of the static partisan totals for each election were similar, for obviously they were not. But the characteristics of change did not vary according to this division of region. In the southern states as well as the nonsouthern states, vote switching from the available pool of Republicans (admittedly small in the South) to the Democrats still dominated the 1932 election, whereas the new voters had their greatest impact in the 1936 contest.

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CLASS COMPROMISES IN INDUSTRIAL DEMOCRACIES

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The stability of democracies has been of critical interest to students of comparative politics. One question that has received limited empirical analysis is, How do we integrate class conflict into the broader concept of democratic stability? We extend the work of Przeworski and Wallerstein. Our principal thesis is that as the material bases of class exchange diminish, the state intervenes economically to expand the resources conducive to class consent. A tripartite coalition thus emerges, with the state facilitating class compromise between labor and capital through economic policies. We explore this thesis in a cross-national analysis of data collected from 20 advanced industrial democracies during the time period 1965-83, and explore the essential conclusions and the political-economic implications of our findings.

The stability of democracies has been of critical interest to students of comparative politics (Lijphart 1984; Powell 1982). One question that has received limited empirical analysis, particularly within a cross-national framework, is how class conflict can be managed so as to promote democratic stability.

Of course, this question presumes a special importance for class conflict in the organization of authority and the distribution of resources within contemporary democracies. Pluralist theory has essentially deemphasized class conflict, while the neocorporatist logic has broadened the concept through the introduction of social concertation, institutional cooperation, and interest coordination into the lexicon of class analysis (Korpi 1983). Others have argued that traditional class differences may be less critical to some democracies than religious, ethnic, regional, or racial concerns (Lijphart 1984), while a more recent body of litera-

ture has focused on the centrist tendencies in advanced industrial democracies (Jackman 1986). However, Hibbs has concluded that "trends in the strike activity tend to complement other streams of evidence contradicting macro-sociological arguments about the 'integration' of labor in advanced capitalist societies and the embourgeoisement of workers in the face of unprecedented affluence" (1976, 1057; see also Cameron 1984; Crouch 1985a; Korpi and Shalev 1979).

How, then, do we integrate class conflict into the broader concept of democratic performance? Przeworski (1980) and Przeworski and Wallerstein (1982) provide an answer. In contravention to the Marxian interpretation of class conflict as the product of two parties—labor and capital—unable to fashion agreement, Przeworski (1980) and Przeworski and Wallerstein (1982) formulate an alternative logic of class relations based on the capacity and willingness of labor and

capital to consent to disagreement. Their research emphasizes the bases of class compromise and hence the preservation of democratic governance within capitalist economies. Class consent is made rational by the possibility of reproducing positive-sum—rather than zero- or negative-sum—relationships. Przeworski (1980) and Przeworski and Wallerstein (1982) draw attention to the economic, organizational, and political conditions conducive to reproducing class compromise.

What lies at the core of the process of renewing capitalism? Przeworski (1980) and Przeworski and Wallerstein (1982), as well as Lange, Ross, and Vannicelli (1982) argue that exchange is at the heart of stable (i.e., nonrevolutionary, nonviolent) class relations. Specifically, capital and labor exchange material benefits with the long-term objective of increasing the overall economic pie available for allocation. If this exchange ceases to be mutually beneficial, class compromise will falter, thereby threatening the replacement of democracy with capitalist dictatorship or labor autocracy. By illuminating the environmental and institutional factors critical to shaping class consensus, Przeworski (1980) and Przeworski and Wallerstein (1982) highlight the challenge public officials continually face, namely, as Powell reminds us, that of finding solutions to "major policy disagreements, as well as to recognized common problems, if the democratic system is to thrive" (1982, 227).

We attempt to extend the work of Przeworski (1980) and Przeworski and Wallerstein (1982) by examining the conditions that impel state interventionism in order to secure class stability. Our principal thesis is that as the material bases of capitalism diminish, the state intervenes economically in order to expand the resources conducive to class consent. A tripartite coalition thus emerges, with the state facilitating class compromise be-

tween labor and capital through economic policies. We explore the thesis in a cross-national analysis of data collected from 20 advanced industrial democracies during the 1965–83 time period.¹ For reasons discussed below, we use public employment as a measure of state intervention.

We first examine the logic behind the state's participation in economic affairs through the allocation of employment. We identify the aggregate resources available to capital for investment and the earning power of labor as proximal conditions motivating a tripartite (state, labor, and capital) coalition. Then we examine the dynamics behind changing the nature of such coalitions through expanded state involvement. The process of change is arguably different from the basic logic of state intervention in economic affairs. Finally, we examine a reduced-form model of the determinants of public employment and discuss its implications regarding class sovereignty. We also discuss the bases of manifest class consent, or—more accurately—the withdrawal of consent.

The Logic of Class Compromise and Tripartite Coalitions

In their recent extension of theories of class conflict, Przeworski and Wallerstein (1982) offer a powerful and elegant argument for conceptualizing class conflict in contemporary industrialized democracies. Challenging the neo-Marxist literature, their framework stresses the prominent role of an exchange system within class relations. The currency of exchange is material in nature. Capital agrees to withhold a portion of its savings for present wages and consumption, while workers agree to consent to capital's primary claim to profit.

While the consent of both labor and capital is necessary in order to achieve a system of exchange, Przeworski (1980,

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33) notes that such a system is nevertheless "hegemonic." The system depends upon the reproduction of profit. Thus the interests of capital within industrial democracies "represent the future universal interests while interests of all other groups appear as particularistic and hence inimical to future developments" (p. 27). Elaborating further, Korpi (1983, 16) distinguishes between the rather limited "power resource" available to labor (human capital) and the principal resource of capital (control over the means of production), which has "a large domain, wide scope and high concentration potential, as well as high scarcity and convertibility," all dimensions found lacking in human capital. Nonetheless, capital, in spite of its hegemonic advantage, must acquiesce in part to the interests of labor in order to prevent "those whose labor is extracted at any moment" from rebelling against the political-economic order (Przeworski 1980, 28). Further, labor organizes collectively in order to countervail capital's recognized power. Unions form for this purpose. This in turn provides the foundations upon which the "outcomes of conflict [between labor and capital] are within limits uncertain, and in particular, [foundations] in which these outcomes are not uniquely determined by class positions" (p. 28).

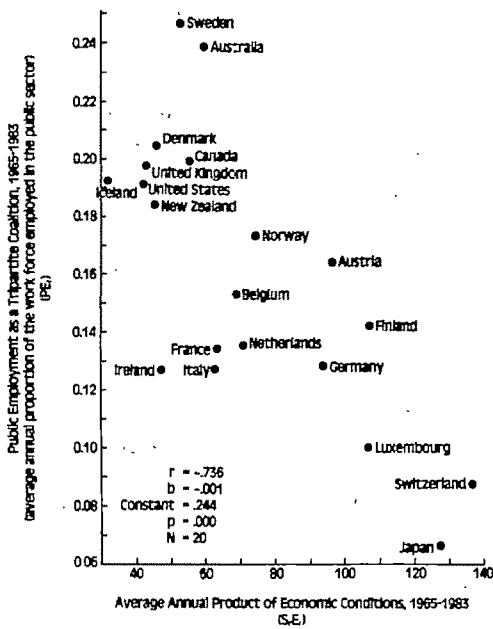
Przeworski and Wallerstein's (1982) theory identifies the role for the state in class compromise. In particular, the state is an instrument—a provider of economic resources to reproduce the material bases of capitalism in such a way as to assure labor's continual assent to the hegemony of profit. More specifically, they argue that "class compromise implies a particular organization of political relations, a particular relation between each class and the state, a particular set of institutions, and a particular set of policies" (Przeworski and Wallerstein 1982, 236). Therefore, state policies both manifest

and support class compromise:

These policies—and the state itself—now appear as an expression of a compromise: they are quite instrumental with regard to the interests of a class coalition that includes both capitalists and organized workers. When workers pursue strategies that lead to a compromise, the state does what appears necessary to reproduce capitalism because this is the choice of the workers as well as the capitalists. The organization of the state as an institution and the policies pursued by this institution constitute an expression of a specific class compromise. (p. 136)

A question thus becomes, What economic conditions lead to expanded state involvement (i.e., expanded public employment) and hence the material expression of a tripartite alliance? The answer lies in the logic which is behind any political coalition. For labor and capital, the logic of participating in a three-party coalition including the state is the imperative need to obtain additional resources that promote a capitalist system of exchange and thereby protect capital's sources of investment while also affording labor wages at sufficient levels to satisfy each. State involvement through public policies is a political-economic strategy to increase the material interests of both labor and capital. It is especially relevant to organized labor's continued assent to capitalism because it enables its leaders to assure members that public authority is being exerted to expand their purchasing power and, implicitly, to prevent the undue appropriation of profit by capital. As Edelman (1971, 144) observed, "Workers are likely to acquiesce without wide or deep dissent in the continued incumbency of their union officials as long as the economic benefits of an affluent society continue to roll in and as long as the union officials present an appropriate dramaturgical performance suggesting they are furthering the interests of their followers. In supplying both kinds of benefits, government can help union officials." Labor's continued confidence in the state as an instrument of economic expansion is

Figure 1. Public Employment as a Tripartite Coalition and the Product of Economic Conditions, 1965-83



arguably of more political importance than capital's support because of organized labor's electoral base (Edelman 1971; Przeworski and Wallerstein 1982). State involvement in class compromise through economic expansionism is therefore, as Przeworski and Wallerstein (1982, 236) point out, consistent with the role envisaged by Keynes (1936, 378): "It is not the ownership of the instruments of production which it is important for the state to assume. If the state is able to determine the aggregate amount of resources devoted to augmenting the instruments and basic rate of reward to those who own them, it will have accomplished all that is necessary [to assure class compromise]." In addition, the state itself, to the extent that it manifests capital's economic dominance, has a direct interest in intervening in behalf of class compromise.

The material bases of capitalism can be

found, at least partly, in the product of two aggregated economic measures: $S_t E_t$. In our cross-national analysis, S_t is the average ratio of gross savings to the gross domestic product in an industrial democracy for the 1965-83 time period, and E_t is the average annual ratio of the rate of growth in real weekly earnings in manufacturing to the rate of growth in the consumer price index in an industrial democracy for the same period.² S_t indicates the resources available to capital for reinvestment, and E_t suggests how labor is benefiting within economic systems in terms of growth in their real capacity to consume goods and services.³ Our expectation is that the larger the product of $S_t E_t$, the more likely labor and capital will be able to fashion a positive-sum exchange and class compromise. The incentives for the state to expand its economic interventionist policies are accordingly less. Conversely, a smaller product implies weakened material bases of class consent and hence greater need for state involvement. For instance, declining growth or negative growth in real earnings may cause workers to have less confidence in their future earning power, thereby generating political pressure for governmental intervention. We therefore expect the product $S_t E_t$ to be negatively correlated with public employment in this model: $PE_t = f(S_t E_t)$, where PE_t is the average annual proportion of the workforce of an industrial democracy employed in the public sector for the years 1965-83.⁴

Figure 1 plots the relationship between PE_t and $S_t E_t$ across 20 democracies.⁵ The data generally support the expected negative relationship. Public employment is generally higher in countries with lower material bases, as expressed in the $S_t E_t$ product.

At least two other factors, however, are likely to effect the participation of labor, capital, and the state in tripartite coalitions to support class compromise through public employment. One is the extent to

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which class relationships are formally organized or institutionalized. Crouch (1985b), Lange (1984), Lange and Garrett (1985), and Przeworski and Wallerstein (1982) provide a theoretical basis for this expectation. The presence of institutionalized procedures for resolving economic differences (over how to slice the economic pie) implies a spirit of compromise between labor and capital—or at least a willingness to mitigate the potential for class warfare. This problem-solving spirit might spill over into political relations (Goldthorpe 1984, 328). To the extent that capital and labor benefit from these institutional arrangements, they have an incentive to promote state policies that reinforce the bases of compromise.

The stability of institutionalized relations (manifested, for example, in the form of collective bargaining) depends substantially on the institutional position of organized labor in society. Lange (1984) and Lange and Garrett (1985) have argued that strong, centralized labor organizations provide an incentive for capital to work toward a compromise with labor and labor to reduce its militancy relative to capital. Such institutional arrangements enhance capital's confidence that organized labor will guarantee the compliance of its member organizations in accordance with the terms of any wage and benefit contracts agreed to by organized labor. Furthermore, centralized labor organizations enhance wage earners' confidence that organized labor will be able to extract the necessary concessions from capital in industrial negotiations. Przeworski and Wallerstein have suggested that in a more general sense the capacity of labor and capital to agree upon a system of exchange in order to achieve class consent will be enhanced when capital is certain that "particular groups among [labor] would not conclude their own agreements with their respective employers at the cost of other workers" (1982, 220). One may

infer from the potential significance of these institutional arrangements that both classes have a vested interest in supporting state policies that reinforce confidence in the extant consent and that do not sow discord between and within labor and capital.

Although a variety of operationalizations of institutionalized class relations exists, Cameron (1984) provides the most sophisticated index. The measure essentially captures the centralization of organized labor and its integration within the capitalist structure. The components of it are (1) the level at which labor bargains, (2) the unity of labor's organization, (3) the style of political-economic decision making within which labor and capital coordinate their relationship, (4) the strength of labor unions within the work place itself, and (5) the density of labor union organizations within the national economy. The first four variables comprise a separate subindex of organizational interaction for each country in our sample. The value of this separate subindex was then combined with the union density value for each country, and from this an index of institutionalized class relations (ICR_i) was devised.⁶

A second factor may also affect the incentives of the classes to compromise and the basic capacity of a state to invest in public employment. Broadly speaking, Przeworski and Wallerstein (1982) have alerted us to the role that international economic developments might play in shaping the parties' confidence in the positive-sum aspects of class compromise. Such compromise depends at least partly on the "ordinary risks . . . owing to domestic and international fluctuations, domestic and international competition" (Przeworski and Wallerstein 1982, 221).

More specifically, Alt (1985) has identified a society's openness (i.e., the extent to which a country relies upon foreign trade, particularly exports) as an important factor in determining a state's capac-

**Table 1. Determinants of Variation in State Intervention
via Public Employment, 1965-83**

Independent Variables	Unstandardized Regression Coefficient (<i>b</i>) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)
Economic conditions (S_tE_t)	-.001** (.000)	-.745** (-5.828)
Institutionalized class relations (ICR_t)	.083* (.024)	.450* (3.397)
Exports/GDP (EXP_t)	-.078* (.037)	-.279* (-2.100)

Note: Dependent variable = average annual proportion of the work force employed in the public sector. Adjusted $R^2 = .699$ ($F = 15.694$)**. Constant = .228 ($T = 11.646$)**. Number of cases = 20.

* $p \leq .05 > .01$.

** $p < .01$.

ity to influence employment through public policies.⁷ Openness might also affect the incentives on the part of labor and capital to compromise in the first place and to support state interventionism. It is not possible, however, to predict a priori the direction of this effect. It depends on the conditions of international economies in general and trading partners in particular. For instance, "If the economies of a country's major trading partners slow down, demand for that country's exports will decline" (Alt 1985, 1020), thereby raising unemployment and reducing public "resources."⁸ At the same time, it automatically triggers some spending in the form of unemployment assistance, and this naturally further shrinks the monies available for other purposes. To compensate for these effects, government must either increase taxes (by expanding existing rates or taxing new sources) or incur debt. At the same time, however, this situation might generate additional political pressures to stimulate economic activity through expanded public employment and the like. In short, openness acts as a conditional factor, affecting—albeit ambiguously—the incentives to expand or stabilize public employment.

Table 1 presents the results of an empirical analysis of the impact of ICR_t and exports (EXP_t)—in addition to S_tE_t —on public employment. The results support the S_tE_t and ICR_t hypotheses. The S_tE_t result indicates that the aggregate economic bases of class compromise are negatively associated with public employment levels across industrial democracies. ICR_t creates conditions favorable to higher levels of such employment, whereas the EXP_t result suggests that openness tends to restrict public employment. Altogether, the multivariate analysis gives us more confidence in the plausibility of the $S_tE_t - PE_t$ relationship.

Changing Nature of Tripartite Coalitions

The logic of a tripartite coalition reflects the basic economic need for the state to reinforce the bases of class consent through public policies. Intercountry differences in the size of public employment, however, depend not just on the material bases of capitalism (profit and purchasing power) but also economic openness and the institutionalization of

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class relations. It is arguable, however, that the dynamics involved in changing public employment at any given point in time may somewhat differ.

The capacity and willingness of capital, labor, and the state to change public employment (i.e., to adjust the rate of growth in such employment) rests on at least two related conditions. The first is the political complexion of a society (Alt 1985; Cameron 1982; Lange and Garrett 1985; Przeworski and Wallerstein 1982). In this regard, the ideological center of gravity becomes relevant. Generally speaking, labor's political influence is somewhat tied to the power of leftist parties. Leftist governments presumably respond more favorably to organized labor's public policy preferences, especially to the extent that it is a key part of a winning electoral coalition. Capital is in a weakened position to offer resistance to labor and its leftist allies lest it might thereby engender more militant actions. Therefore, to the extent that organized labor generally seeks an expanded public sector, greater rates of growth in public employment should be exhibited in nations where the ideological center is on the left of the continuum.

The parties' confidence that an increase in the rate of growth in public employment will translate into greater benefits in the future is the other consideration. The predictability and stability in class relations will obviously influence their confidence. Unpredictable and unstable relations make capital and labor each uncertain as to whether the other's industrial behavior will be adjusted in order to promote mutually beneficial exchanges within an expanding public sector economy. Both parties are arguably interested in containing excesses in such behavior in order to reproduce the material bases of capitalism. Here the institutionalization of class relations assumes much practical import. As previously noted, such institutionalization formalizes and regularizes

relations. An encompassing, centralized labor movement may be better able to contain short-term benefits (Maitland 1985; Reshef 1986). With this condition in place, the resistance to a change in public policy may wane, *ceteris paribus*. Thus if, as before, we define the institutionalization of class relations as ICR_t and if we define labor's political success as the extent to which the center of ideological gravity is leftist, we may test the following model: $\Delta PE_t = ICR_t + \bar{I}_t$, where ΔPE_t is the average annual rate of growth in the proportion of the total work force employed in the public sector for a country between the years 1965 and 1983, and I_t is the average annual ideological center of gravity within the lower chamber of the national legislature between years 1965 and 1983.⁹

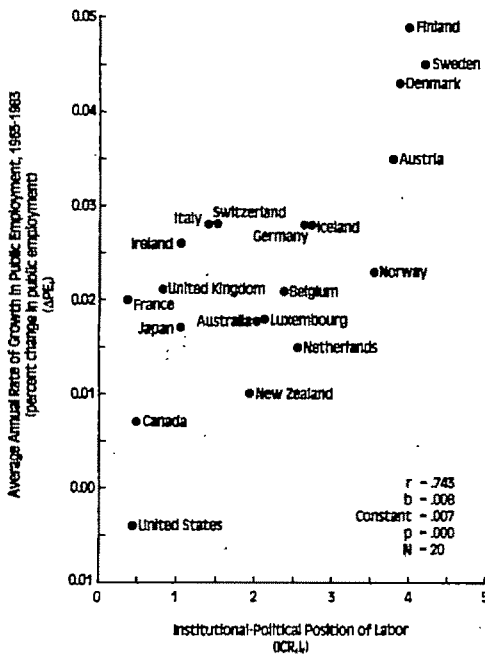
Based on the data, we derive the following estimates:

$$\begin{aligned} \Delta PE_t = & .022 + .024(ICR_t) \\ & (.010) \\ & + .008(I_t) + e, \\ & (.005) \end{aligned} \quad (1)$$

where the standard errors are indicated in the parentheses below each unstandardized regression coefficient. While the model can account for nearly half the variance ($R = .701$) in ΔPE_t , the presence of institutionalized class relations (ICR_t) is slightly more influential than the ideological center of gravity (I_t) within a country. Equation 1 suggests that the increase in I_t , denoting a more pronounced leftist orientation within the legislature, does tend to increase the expansion of tripartite coalitions, yet not as significantly as the institutionalization of class relations (ICR_t).

Rather than conceptualizing institutional class relations and labor political power as two separate and independent influences on the state, it would seem that they are interdependent. To some extent, institutional and political power go hand

Figure 2. The Rate of Growth in Public Employment and the Institutional-Political Position of Labor, 1965-83



in hand (Reshef 1986; Lange and Garrett 1985), mutually reinforcing each other. As Lange and Garrett (1985) argue, "The organizational and political power of labor both strongly affect economic growth. The impact of each, however, is heavily contingent upon the relative presence or absence of the other" (p. 820). A strong labor political presence, coupled with a well-established institutional framework for handling industrial disputes and differences, may multiplicatively affect a state's policy determinations, especially to the extent that such determinations must "win popular support in elections" (Przeworski and Wallerstein 1982, 236). In a similar vein, such a combination should lessen capital's incentive to resist expanded public sector involvement by making the expected costs of

resistance more predictable (i.e., increased labor militancy in economic and political affairs).

This interactive proposition may be formulated as

$$\Delta PE_t = f(ICR_t, I_t), \quad (2)$$

where ICR_t, I_t reflects the combined impact of labor's institutional and political position within a country. Figure 2 plots this relationship across the 20 industrialized democracies. The model improves the total explained variance of ΔPE_t , from 49.1 in equation 1 to 55.2 in equation 2. Where class relations and political representation are reinforcing, offering capital and labor greater certainty as to their position within a country's political economy, barriers to expanding the public sector become significantly more manageable and surmountable than in those democracies where the political and institutional class representation have been less mutually reinforcing.

An examination of Figure 2 suggests the possibility, however, that the relationship between ICR_t and ΔPE_t may be the result of the means by which interests in general are channeled into the political arena. Employing Lijphart's models of democracy (1984, 21-45), we can see that 5 of the 10 countries (Australia, Canada, New Zealand, the United Kingdom, and the United States) that fall at or below the median point (.021) for ΔPE_t exhibit predominantly majoritarian characteristics with respect to the style of executive-legislative relations and issue representation. Based upon an exclusionary principle common, although not exclusive, to Anglo-American democracies, the majoritarian style of representation tends to reward those interests within the mathematical majority, while largely excluding those interests that fall outside the range. Above the median point of ΔPE_t , we find 3 of the 10 countries (Denmark, Finland, and Switzerland) to be those that exhibit

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the consensual style of executive-legislative relations and issue representation. Consensual executive-legislative relations are more likely to preserve the specificity of competing interests while seeking accommodation through "grand coalitions" (Lijphart 1984). Majoritarian executive-legislative relations, by contrast, would tend to force interests into an aggregated bundle, thereby inhibiting reform and change. The implication follows that the expansion of political-economic coalitions may have less to do with ICR_t per se, than with institutional arrangements that have been sustained to allow a variety of interests to find assistance and attention through public policies. Issues are defined, presented, and eventually implemented through three critical institutions in democratic societies: parties, legislatures, and executives. Party polarization and sharp issue cleavage tend to intensify political conflict and more clearly distinguish political interests within democracies (Lijphart 1984; Powell 1982). This condition is further aggravated when the principal chamber of the legislature is fractionalized among several different political parties, thus tending to disaggregate and extend the number of distinct interests represented within the legislative arena of modern democracies (Rae 1971). Finally, if either of these two conditions are complemented by an institutionalized executive formula that departs from a simple majoritarian model, wherein the executive is not concentrated in one person or political party controlling a majority of seats in the principal chamber of the legislature or where the executive does not consist of a minimum winning coalition, the tendency toward consensus and disaggregation is increased (Dodd 1976).

While it is customary to examine one—or possibly two—of these three factors when operationalizing the degree of issue disaggregation within democracies, we have opted for a weighted index that concentrates explicitly upon those three fac-

tors critical to the style of executive-legislative decision making. Therefore, for each democracy within our sample we have separately determined for the years 1965–83 (1) the degree of dimensional polarization within the party system,¹⁰ (2) the degree of fractionalization within the lower chamber of the national legislature,¹¹ and (3) the extent to which the power of the executive is diminished relative to the legislature.¹² Having obtained these individual measures for the three separate indicators of issue disaggregation within the 20 democracies, we then derived separate weighted index scores for each democracy by factor-analyzing the data and obtaining standardized factor scores from this procedure. The larger the value of the factor score, the more the style of executive-legislative relations assume the characteristics of the consensual model of democratic representation and departs from a style characteristic of the majoritarian model of democratic representation.¹³

A second characteristic of these democracies that may have an independent impact upon the incentives to change public employment is the degree of political-economic instability within the industrialized democracy. Instability within the political environment is tantamount to an increase in the insecurities of both labor and capital regarding their future material interests as served by the present configuration of tripartite coalitions. As Lange notes, "Whatever the political composition of the government, if the state is not perceived to be able to implement its policy intentions, workers' insecurities about the future prospects are likely to be increased" (1984, 111). We presume that the same concerns influence capital. Thus partisan alignments upon which stable policies are built should be a critical factor in defining these coalitions. We would expect that the more unstable such partisan alignments or the more volatile the political environment, the more resistant

Table 2. Rate of Growth in Public Employment, 1965-83

Independent Variables	Unstandardized Regression Coefficient (<i>b</i>) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)
Institutional-political position of labor ($ICR_t I_t$)	.006** (.001)	.633** (4.232)
Style of executive-legislative relation	.005* (.002)	.374* (2.400)
Political-economic instability ($RV_t \Delta UR_t$)	-.204* (.101)	-2.91* (-2.101)

Note: Dependent variable = average annual rate of change in the proportion of the work force employed in the public sector (ΔPE_t). Adjusted $R^2 = .641$ ($F = 12.284$)**. Intercept = .013 ($T = 3.223$)**. Number of cases = 20.

* $p < .05$ > .01.

** $p < .01$.

the parties will be to the transformation and modification of state intervention in economies through expanded public employment.

The effect of political volatility on labor and capital's ability to reach accord on expanding coalitions should, however, be conditioned by their respective evaluations of the economic environment and how it will affect the future material interests. Again, Lange (1984) underscores this point when he suggests that labor and, by inference, capital "can be expected to evaluate the political 'guarantees' about future benefits from [class coalitions] in the context of their perceptions of the more general prospects for future economic performance as determined by domestic and international conditions [and] how government can help reduce workers' [and capital's] uncertainties about the future" (p. 111). Therefore, political volatility and economic instability should have a decisive and interdependent influence on the nature of tripartite coalitions. The greater the presence of political-economic instability, the less we should expect capital and labor to reach the kind of consensus necessary to alter public policy regarding the role of public employment. To test this hypothe-

sis, we have defined political-economic instability as $RV_t \Delta U_t$. RV_t is the average annual volatility of representation within a democracy between 1965 and 1983, and ΔU_t is the average annual rate of growth in the proportion of the work force unemployed for the years 1965-83.¹⁴

Table 2 presents the estimations of the rate of growth in public employment across industrial democracies. The results of this analysis confirm the proposition that $ICR_t I_t$ is the proximal source of the rate at which coalitions expand ($b = .006$). Nonetheless, the style of executive-legislative relations and political-economic instability are both significant—though secondary—factors in determining this rate of expansion ($b = .005$ and $b = .204$, respectively). Both coefficients are in the predicted directions. We may conclude therefore that the rate at which tripartite coalitions expand through the enlargement of public employment is dependent primarily upon the institutional and political position of labor and the assurances of restraint and cooperation that these mutually reinforcing conditions imply. This relationship holds in spite of nature of executive-legislative relations within industrialized democracies—and to the degree to which

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the political-economic environment of the democracy is both unstable and uncertain.

The Logic and Nature in Perspective: Class Sovereignty and Conflict

Class Sovereignty

Up to this point, our analysis has underscored the conclusions of Przeworski (1980) and Przeworski and Wallerstein (1982): there is a strong material basis (S_tE_t) to class compromise, a process additionally conditioned by the political, institutional environment, especially the institutionalization of class relations. There is, however, a fundamental consequence that lies in the core of tripartite coalitions: the decline of class sovereignty. If we define class sovereignty as the extent to which labor and capital restrict the class coalition to only two parties—labor and capital—we may understand this sovereignty to represent the capacity and willingness of capital and labor to structure their exchange system and organize their relationship according to a more exclusive set of preferences, primarily those of their memberships and organizational leadership.

With public authority comes an expansion of the issues that consume public leadership. Schattschneider (1975) identified this process as the contagion of conflict, which spreads as greater numbers of interests find their expression in the arena of political struggle. Private conflicts become public conflicts, and policy debate becomes socialized through the inclusion of public authority. Schattschneider (1975), though speaking with reference to the U.S. system, underscored the resulting decline in class sovereignty when he observed, "It is the function of public authority to modify private power relations by enlarging the scope of conflict. Nothing could be more mistaken

than to suppose that public authority merely registers the dominance of the strong over the weak" (p. 40). Across democracies in general, the growth of public authority expands rule-making and bureaucratic procedure, which further diminishes the ability of private groups to define the issues of political contest (Aberbach, Putnam, and Rockman 1985; Dogan 1975; Page 1985b; and Suleiman 1974).

The logic and nature of class compromise, reinforced by tripartite coalitions involving state intervention, reveal the principal factors that determine the scope (or relative size) of class sovereignty in advanced industrial democracies. This scope will be no larger than the evident need to revitalize and supplement the exchange system of capital and labor through the addition of public resources as measured by PE_t . This need is reflected in investment and earnings resources (S_tE_t). However, this scope will be no smaller than the combined institutional and political power of labor (ICR_tI_t) allow. Furthermore, given a predetermined or extant need for government involvement, the flexibility to change the rate of involvement will similarly depend on ICR_tI_t .

These propositions are explored in Table 3. While ICR_tI_t clearly both constricts the scope of class sovereignty by increasing the role of public authority—as measured by PE_t —in class relations ($b = .014$) and reduces the uncertainties that can encumber change in public employment ($b = .007$), S_tE_t is the principal determinant of the actual configuration of tripartite coalitions ($b = -.001$) and therefore the scope of class sovereignty. The findings reported in Table 3 confirm the central dynamic and the predominate logic of class compromise and tripartite coalitions: regardless of the degree to which capital and labor have over time forged a sound and adherent relationship within the political and economic arena of

Table 3. Estimates of PE_t and (ΔPE_t) , Employing Only Proximal Determinants of the Logic and Nature of Tripartite Coalitions, 1965-83

Independent Variables	Model 1: Estimating (PE_t)		Model 2: Estimating (ΔPE_t)	
	Unstandardized Regression Coefficient (b) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)	Unstandardized Regression Coefficient (b) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)
Economic conditions ($S_t E_t$)	-.001** (.000)	-.760** (-5.118)	.000 (.000)	.154 (.952)
Institutional-political position of labor ($ICR_t I_t$)	.014* (.005)	.379* (2.554)	.007** (.001)	.733** (4.539)
Adjusted R ²	.609 (F = 15.007)**		.537 (F-statistic = 11.433)**	
Constant	.217 (T-statistic = 10.716)**		.003 (T-statistic = .529)	
Number of cases	20		20	

* $p \leq .05 > .01$.

** $p \leq .01$.

capitalist democracies, the final configuration of government's role in class compromise rests ultimately on how the material interests of labor and capital cumulatively affect the relations on which the system of class exchange is built.

Class Conflict

We pursue the implications of our analyses by examining the relationship between the salient determinants of public employment—regarded as a tool of a tripartite coalition to reinforce class compromise—and the presence of class conflict in industrial democracies. We have argued, with respect to public employment levels per se, that a deterioration in the material bases of capitalism ($S_t E_t$) should contribute to greater state interventionism. Such a deterioration does indeed seem to be associated with a presumably corrective public policy response, but at the same time it might strain class relations by lessening confidence in the positive-sum aspects of class compromise. Therefore, weakened economic conditions, as reflected in lower

levels of $S_t E_t$, might contribute to out-breaks of class conflict.

Based on our analysis of both public employment levels and change in public employment growth rates, however, such conflict should be modified by the combined institutional and political position of labor ($ICR_t I_t$). The institutionalization of class relations gives labor the right and power to obtain benefits without having to engage rampantly in open conflict with capital. Favorable political representation gives unions greater confidence that the state will intervene in order to expand the material bases of class compromise and thereby increase the real earning power of workers.

Although public employment may be a direct manifestation of class compromise and a tripartite coalition, its impact on class conflict, especially in the short run, is problematical. In this vein, state interventionism through public employment represents a response to underlying economic variables, conditioned by institutional political considerations. Public employment is therefore arguably endogenous in the short run, and its expan-

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Table 4. Regression of Estimates of the Withdrawal of Class Consent, 1965-83

Independent Variables	Model 1: Estimating (WC_t)		Model 2: Estimating (WC_t)		Model 3: Estimating (WC_t)	
	<i>b</i> (Standard Error)	β (T-Statistic)	<i>b</i> (Standard Error)	β (T-Statistic)	<i>b</i> (Standard Error)	β (T-Statistic)
Economic conditions (S_tE_t)	-.005* (.002)	-.573* (-2.021)	-.005 (.001)	-.534** (5.212)	—	—
Institutional-political position of labor (ICR_tI_t)	-.122* (.061)	-.581* (-2.000)	-.088* (.034)	-.443* (-2.586)	—	—
Public employment (PE_t)	-.070 (1.679)	-.012 (-.039)	—	—	1.328 (1.211)	.244 (1.096)
Growth in public employment (ΔPE_t)	3.813 (5.555)	.190 (.600)	—	—	-7.563 (4.360)	-.380 (-1.731)
Adjusted R^2 (F-statistic)	.426 (4.341)**		.480 (9.299)**		.105 (2.057)	
Constant (T-statistic)	1.173 (2.995)**		1.170 (9.223)**		.624 (2.743)**	
Number of cases	19		19		19	

* $p < .05 > .01$.

** $p < .01$.

sion as a matter of policy may coincide with conflict to the extent that the seeds of conflict also spawn corrective or ameliorating public policies.

If we define open industrial conflict as a proxy measure of the extent to which labor and capital withdraw their consent, we can test the following models:

$$WC_t = f(S_tE_t) + (ICR_tI_t) + (\Delta PE_t) + (PE_t)$$

$$WC_t = f(S_tE_t) + (ICR_tI_T)$$

$$WC_t = f(PE_t) + (\Delta PE_t).$$

Operationalizing the withdrawal of class consent (WC_t) as the average annual volume of strike activity (logged) in a democracy for the period 1965-83, Table 4 confirms our expectations.¹⁵ Three conclusions emerge from the empirical analyses. First, the principal contributor to conflict is the product S_tE_t , which suggests, as predicted, that a deterioration in the material bases of capitalism engenders class conflict (but also, as the logic of class compromise indicates, the public policies suited for amelioration). Second, the

combined institutional and political position of labor, as reflected in well-established class relations and favorable political representation, apparently reduces conflict, suggesting that labor is confident of benefiting enough in the long run from compromise and consent to limit short-term hostilities. Third, public employment, as well as the rate of growth in public employment, does not significantly affect industrial conflict across industrial democracies.

Conclusions and Implications

We have attempted to extend the works of Przeworski (1980) and Przeworski and Wallerstein (1982) in particular. These scholars have laid the theoretical basis for class compromise, arguing that it is in the material interests of capital and labor to reinforce the bases of their exchanges. Our principal thesis is that there is an economic logic, based on the material conditions of exchange, that provides the incentive for the state to intervene in economic affairs in order to stimulate the

bases of capitalism and class compromise. Through its intervention, the state essentially enters into a tripartite coalition with capital and labor, a coalition logically serving the interests of the two classes, at least in the longer term. Focusing on public employment as a measure of state intervention, our empirical analyses across 20 advanced industrial democracies support the logic. They also show that the institutional and political position of organized labor affects the capacity and willingness of the state to expand the tripartite coalition: greater rates of growth in public employment are associated with greater levels of institutionalized class relations and prolabor political representation.¹⁶

Three main conclusions may be derived from our analyses. First, economics lies at the heart of class compromise and class conflict, and at establishing the means of resolving conflict through expanded state interventionism. Tripartite coalitions are economically induced phenomena aimed at reproducing the positive-sum aspects of economic exchanges between two competing classes—labor and capital.

Second, politics are relevant to the degree to which the parties will assent to an expanded level of state involvement. It registers both the degree of confidence the parties, especially labor, have in the likelihood that their interests will be fairly and adequately protected. It also manifests the degree to which one party can resist the public policy preferences of another. Where labor dominates politically, capital is obviously less able to forestall its preferences. Indeed, recalcitrance might breed worker militancy, thereby threatening the very foundation of capital. Ironically, organized labor's political power may be positively associated with capital-labor cooperation in politics. In a related vein, institutional and political position, when positively correlated, interacts synergistically.

Third, the institutionalization of class

relations, part of which involves the degree of union penetration in the labor force, facilitates state intervention through public employment and reduces manifest class conflict. Formal procedures through which labor is given the opportunity and right to represent its economic interests secures rather than destabilizes capitalism. In other words, rather than undermining capitalistic democracies, a free and powerful labor movement may well add a degree of certitude and confidence essential to stability in democratic performance.

Two principal implications follow. The first is that the state should evaluate the merits of its macroeconomic policies at least partly in terms of the impact they will have on economic conditions that affect the willingness of capital and labor to compromise. Governments should look on these policies as means of influencing confidence in the parties' belief that they will benefit in the long term from consensual behavior. This implies that unstable, erratic policies may erode such confidence and thereby precipitate conflict.

The second is that the logic of class compromise and consent developed by Przeworski (1980) and Przeworski and Wallerstein (1982) is a powerful theoretical argument. The inherency of class conflict built into Marxist thought is certainly called into question: economic and political conditions may either provoke or stymie conflict. This is not to suggest, however, that class differences are perpetually evanescent. The essence of compromise is the exchange of resources and authority required to contain different interests. Nonetheless, compromise and its reproduction are clearly attainable.

Notes

1. The countries in our analysis are Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, Japan,

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Luxembourg, the Netherlands, New Zealand, Norway, Sweden, Switzerland, the United Kingdom, and the United States.

2. Data for S_iE_i were collected from International Labour Office 1965–85 (various volumes) and Organization for Economic Cooperation and Development (hereafter OECD) 1982b, 1984b, and 1986 (p. 158). The mean value of S_iE_i across the sample was 71.66 (standard deviation = 29.922), with a minimum value of 32.12 (Iceland) and a maximum value of 136.75 (Switzerland).

3. We focus on S_i and E_i in particular, rather than an aggregate measure of economic performance (e.g., gross domestic product growth rates), because we are interested in tapping the resources available to competing classes. Savings are essential to the capacity of capital to reinvest. Factors that threaten savings undermine a society's ability to reproduce capitalism (Musgrave and Musgrave 1976; Przeworski and Wallerstein 1982). We believe that growth in real earnings is especially critical to organized labor's political-economic calculations regarding the merits of consensual behavior. In the present, workers sacrifice income, and hence consumption, in the hope of expanding the future base of their rewards by allowing capital greater capacity to expand. To some extent, however, they must rationally discount future benefits, and it is arguable that their discount rates rise in relation to the confidence they have that future rewards are forthcoming (Przeworski and Wallerstein 1982). Rising real income would seem to root such psychological confidence in tangible economic performance (McCallum 1986).

4. We need to explain why we chose public employment as our dependent indicator of state interventionism over more common measures based on public finance (see, for instance, Cameron 1978). First, as Rose (1985) and many others have documented, public employment has grown significantly in most industrial democracies since World War II. Many factors explain this growth, but governments can and do expand public employment for the sole purpose of stimulating economic growth (Musgrave 1982). Capital and labor will often combine to push for such an expansion, as indicated by the agreement reached between capital and labor in the Spanish shoe industry in 1980 to seek state intervention to protect employment in that industry (International Labour Office 1980, 441). (We should note that we reviewed the *Social and Labour Bulletin* for the 1965–83 period, and identified numerous examples of the state intervention in employment.)

Furthermore, governmental programs aimed at shielding workers and their families from the harshness of industrial life—programs in such areas as education assistance, health care, and job training—entail employment. As these programs expand, so does governmental employment.

Second, public employment "is not as fungible as money." It is a more permanent form of public com-

mitment and, by implication, a more significant intervention in economic affairs. Third, public employment is easily conceived of in terms of expenditures. For many governments, personnel is the single largest cost. Public employees are paid salaries and benefits—public expenditures.

The measure of public employment is the total work force employed in the various departments and agencies at central, state, provincial and local levels of government that "produce non-market goods and services" (OECD 1982a, 9). While the collection of public employment data is known to vary somewhat across countries (Economic Commission for Europe 1979; Haveman and Halberstadt 1982; Heller and Tait 1984; OECD 1982b; Page 1985a; and Rose 1985), the data provided by OECD (1982b), supplemented by equivalent data from OECD (1984a, 1985), Rose (1985), and Martin (1982) provide a reasonable cross-nationally equivalent measure of public employment at the level of general government. The mean value of PE_i in the sample is .159–15.9% (standard deviation = .048) with a minimum value of .066 (Japan) and a maximum value of .246 (Sweden).

In subsequent analyses, we focus on the average annual rate of change in public employment (ΔPE_i) between 1965 and 1983, arguing that the dynamics of change may be somewhat different from the explanation of rates of public employment across nations. The average annual rate of change in public employment across nations between 1965 and 1983 is .024 (standard deviation = .013) with a minimum value of $-.004$ (United States) and a maximum value of .049 (Finland). Our theoretical model and empirical analyses are intended to explain inter-country variation in PE_i and ΔPE_i .

5. The unit of analysis in this study is a country-time period (1965–83). Choosing a longitudinal time period was made necessary partly because several variables in the subsequent portion of our study are not available across our sample on a continuous annual basis (e.g., public employment and strikes); others do not vary on an annual basis (e.g., elections, trade union density, institutionalized class relations, ideological center of gravity within legislatures, and executive-legislative relations). Although a continuous time series analysis is impossible, we can capture the relationships between variables averaged over several years, thus avoiding the problem introduced by yearly aberrations within year-specific cross-sectional analyses.

6. Bargaining level is assigned a value of one in countries where bargaining occurs at a regional or national level, and zero is assigned to those countries where bargaining is decentralized at the enterprise or work place level (Blanpain 1985; Blythe 1979). The unity measure assigns one to countries in which a confederation dominates the labour movement and zero to countries in which there is no singular dominant confederation (Blanpain 1985; Blythe 1979).

Countries that exhibit a style of political-economic decision making that actively and systematically integrates labor, capital, and government (neocorporatist) are assigned a value of one; and those not exhibiting such a style of political-economic decision making are assigned a value of zero (Crouch 1985a, 117). Works councils are assigned a value of one in countries where workers have the statutory right to participate in works councils, and zero is assigned to all other countries (Cameron 1984, 164-65). The value of each of these four indicators was constant for a country over the entire time period 1965-83, as annual estimates are both highly inaccurate or incomplete across the sample. Whereas Cameron (1984, 164-65) has measured these variables at the interval level, we have opted for a dichotomous measure because of the ambiguity associated with some of the measures for countries not included in Cameron's sample. We have summed these four dummy variables for each country and divided by four to derive the subindex of organizational interaction for each country. This value is then added to union density (.001), and this sum halved to calculate the final measure of institutionalized class relations (ICR_i). For a similar procedure, see Lange and Garrett 1985. Union density is a measure of the proportion of the work force in labor unions. These are estimates that vary in accuracy and frequency across the 20 democracies (Blanpain 1985; Crouch 1985a, 114-15). The value of union density for each country during the period 1965-83 is based upon the average annual estimates available for that country. Data on union density are taken from numerous sources, the principal ones being Troy and Sheflin (1985), Blanpain (1985), Coldrick and Jones (1979), and Bain and Price (1980). The mean value for institutionalized class relations for the sample is .498 (standard deviation = .259), with a minimum value of .107 (France) and a maximum value of .917 (Sweden).

7. Data on exports were taken from OECD 1986. The mean value in our sample is .317 (standard deviation = .171) with a minimum value of .072 (United States) and a maximum value of .828 (Luxembourg).

8. Unemployment reduces the income base from which states may generate revenues for public employment.

9. We have followed the sample of Gross and Sigelman (1984, 466-67) and employed the following formula to obtain the value of a country's ideological center of gravity:

$$\sum_{i=1}^n (TiCi)/19$$

where T is the proportion of seats of a particular political party in the lower chamber of the legislature, C is the party's position along a 10-point ideological left-right continuum, and i is the particular

legislature between 1965 and 1983. Between general elections for the legislature within a particular country, the value of (I_i) for the last general election was assigned for each year. During those years when general elections to the legislature occurred, the value of that year's ideological center of gravity was assigned its value based on that year's general election if the election occurred before 30 June; the value of the year prior to the general election was assigned if the election occurred after 1 July of the year. We have employed Gross and Sigelman's (1984, 469) example and relied on the 10-point left-right ideological scale as provided by the *Britannica Book of the Year* (for the years 1970-83). The scale ranges from 0 (fascist) to 9 (communist), with 4.5 as the center. Where codes changed over the time period—as a few did—these were incorporated into our calculation of (I_i). In the case of the United States, the House of Representatives was used as the proxy "principal" chamber. Data on proportions of seats in the legislature were taken from Mackie and Rose (1982) and *Keesing's Contemporary Archives* 1965-84 (for the later years). The mean value of (I_i) for the sample is 4.218 (standard deviation = .514) with a minimum value of 3.440 (United Kingdom) and a maximum value of 5.274 (Italy).

10. Dimensional polarization is an index designed to measure the extent of party ideological polarization within the context of the number of issues which divide the party between 1965 and 1983. The calculation of dimensional polarization is based on the product $P_i D_i$, where P_i is the extent of party ideological polarization (along a standard left-right spectrum) between 1965 and 1983, and D_i is the number of issue dimensions that have divided the country between 1965 and 1983. Following the example of Dodd (1976, 105-6), P_i is calculated according to the formula

$$\sum_{i=1}^n (\sum_{c=1}^n V_{ci}) / 19,$$

where V_{ci} is the variance of party i 's position on a standard 10-point left-right scale in the legislature. V_{ci} is defined further as

$$\sum (f_i \%) (X_{ij} - \bar{X})^2,$$

where X_{ij} is party i 's position on the Britannica's 10-point left-right scale and \bar{X} is the mean of all X_{ij} s, each weighted by the percentage of legislature seats associated with party i in the legislature ($f_i \%$). As with I_i , for each year between general elections, the value of P for the last election is assigned for a given year. During general election years, the value of P is decided according to when the election is held. The number of issue dimensions for a country P_i is taken from Lijphart 1984 (p. 130). The difference between P_i and $P_i - D_i$ can be dramatic. In the case of Austria, for instance, $P_i = 1.181$; and in Sweden $P_i = 1.846$. In Austria, there are 2 issue dimensions, in

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Sweden 2.5. The respective index values ($P_i D_i$) are 2.362 and 4.615, once P_i is weighted by the issue environment within which the left-right polarization exists. Thus the differences in terms of each country's ability to resolve political conflict through a majoritarian form are more sharply highlighted when examining dimensional polarization ($P_i D_i$). The mean value ($P_i D_i$) across the sample is 4.286 (standard deviation = 2.597), with a maximum value of 8.323 (Finland) and a minimum value of .344 (Ireland).

11. Fractionalization is defined according to the standard formula

$$1 \sum_{i=1}^n (T_i)^2 / 19$$

where T_i is party i 's proportion (.001) of seats in the lower chamber of the legislature (Rae 1971). The larger the value of fractionalization, the greater the probability that any two seats in the legislature will be held by different political parties. The mean value across the sample is .673 (standard deviation = .103). The high value was Finland (.814), and the low value New Zealand (.489).

12. Executive power is defined according to the measure developed by Dodd (1976, 117),

$$(N_i / wa) / 19$$

where wa is the proportion (.001) of seats in the legislature of those parties whose addition to the existing cabinet would make the cabinet either minimum-winning or majoritarian or whose exclusion from the cabinet would render the cabinet minimum-winning or majoritarian. As there are more than one set of parties that can exist that will render the cabinet minimum-winning or majoritarian, we have followed the example of Dodd (1976, 118) and chosen the set whose wa is either largest (if the cabinet is oversized, i.e., more parties than necessary) or smallest (if the cabinet is minority status, i.e., too few parties to obtain a majority of seats in the legislature). N_i is the number of parties required to produce the necessary wa . The larger the value of the index, the less the power of the executive relative to the legislature (Dodd 1976; Lijphart 1984). Data for cabinets were taken from the authors' coding of cabinet formations as reported in *Keesing's Contemporary Archives* (1965-84), according to the definition of cabinet formation as employed by Dodd (1976) and Robertson (1984). The values of executive leadership ranged in our sample from 0 (the United States, Luxembourg, and New Zealand) to .259 (Switzerland). The mean value was .67 (standard deviation = .077). Having obtained these individual measures for the three separate indicators of issue disaggregation within the 20 democracies, we then derived separate, weighted, index scores for each democracy by factor-analyzing the data and obtaining standardized factor scores from this pro-

cedure. The larger the value of the factor score, the more the style of executive-legislative relations assumes the characteristics of the consensual model of democratic representation and departs from a style characteristic of the majoritarian model of democratic representation.

13. Following oblique rotation, one prominent dimension was identified with the following estimated communalities for the respective three variables comprising the weighted index: dimensional polarization (.500), legislative fractionalization (.659), and executive power (.548). The factor scores derived from this analysis are very similar to those reported in Lijphart's (1984, 216) "effective number of parties" dimension. The mean value for the standardized factor score measuring the style of executive-legislative ratio is 0 (standard deviation = 1) with a minimum value of -1.645 (New Zealand) and a maximum value of 1.409 (Switzerland). While our index is designed to capture only the legislative-executive dimension of these democracies, our derived factor scores nonetheless correlate highly with Lijphart's ($r = .855$).

14. Representational volatility is defined according to the formula of Pederson (1983, 32-33),

$$.5(TNC_i) / 19$$

where TNC is the total net change in party seat representation in a given legislature (i), as defined according to the formula

$$\sum_{i=1}^n |P_{ij} - P_{ij}| = 1$$

where P_i is the proportion of seats (.001) held by party i in the legislature (j). Thus, the volatility of representation (RV_i) is the cumulated gains (or alternatively losses) in seats for all parties represented in the lower chamber of the national legislature. Data for (RV_i) are drawn from Mackie and Rose 1982 and *Keesing's Contemporary Archives* 1965-84 (for the later years). Data for (ΔU_i) are from OECD 1986.

15. The average annual volume of strike activity is the product of average annual frequency of strikes (number of strikes per 1,000 employees 1965-83), the average annual duration of strikes (number of days lost per strike 1965-83), and the average annual size of strikes (number of strikers per strike 1965-83). This measure follows that developed by Hibbs (1976, 1035-36) and employed by Cameron (1984, 176), Korpi (1983, 161-62), and Crouch (1985a). However, to remain consistent with the unit of analysis employed throughout this study, we have calculated WC_i as the product of the three components measured at the level of the broader time period (1965-83) rather than at the level of the individual year. Several of the countries do not have complete data for all three components during a given year, thus necessitating the computation of

WC_t at the broader time period level. Indeed, Luxembourg is deleted from this portion of the analysis because of insufficient and inequivalent data. Data were drawn from the International Labour Office 1965–85 (various volumes). Because of skewness in our sample, WC_t was log-transformed, a procedure employed by both Korpi (1983) and Crouch (1985a). Both the log-transformed data and the raw data correlate highly with the data on strike volume reported in Korpi (1983, 161–65), Cameron (1984, 153), and Crouch (1985a, 120). It is important to note that the International Labour Office data make no distinction between work stoppages and lockouts. The value of WC_t should therefore be properly considered as an indicator of class conflict, not merely employee strikes, or employer militancy (Crouch 1985a, 112–13).

16. It is possible to conceive that our results might be a function of specific time period effects, especially those associated with the relatively turbulent 1974–83 time period. In an analysis unreported here, we estimated the relationship between $S_t E_t$ and $ICR_t I_t$ on the one hand and PE_t and ΔPE_t on the other for those years. The results, which are available upon request, are consistent with the broader period. $S_t E_t$ is negatively and statistically significantly correlated with PE_t , but positively and non-significantly correlated with ΔPE_t . $ICR_t I_t$ is positively and significantly associated.

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THE RENAISSANCE OF POLITICAL CULTURE

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The publics of different societies are characterized by durable cultural orientations that have major political and economic consequences. Throughout the period from 1973 to 1987, given nationalities consistently showed relative high or low levels of a "civic culture"—a coherent syndrome of personal life satisfaction, political satisfaction, interpersonal trust and support for the existing social order. Those societies that rank high on this syndrome are much likelier to be stable democracies than those that rank low. Economic development and cultural change are linked in a complex pattern of reciprocal influence. Originally, Protestantism may have facilitated the rise of capitalism, leading to economic development, which in turn favored the emergence of the civic culture. But in those countries that attained high levels of prosperity, there eventually emerged postmaterialist values that tended to neutralize the emphasis on economic accumulation that earlier characterized Protestant societies.

It is time to redress the balance in social analysis. Since the late 1960s, rational choice models based on economic variables have become the dominant mode of analysis; while cultural factors have been deemphasized to an unrealistic degree. This approach has made major contributions to our understanding of how politics works; nevertheless, it underestimates the significance of cultural factors, if only because while economic indicators are readily available for these models, cultural data generally are not.

The incompleteness of models that ignore cultural factors is becoming increasingly evident. In Catholic societies from Latin America to Poland, the church plays a major role despite the demise often predicted by economic determinists. In the Islamic world, Muslim fundamentalism has become a political factor that neither East nor West can ignore. The Confucian-influenced zone of East Asia manifests an economic dynamism that

outstrips any other region of the world. By economic criteria one of the *least*-favored regions on earth, it is virtually impossible to explain its performance without reference to cultural factors. Even in advanced industrial societies religion not only outweighs social class as an influence on electoral behavior (Lijphart 1979) but actually seems to be widening its lead: while social class voting has declined markedly in recent decades, religious cleavages remain astonishingly durable.

There is no question that economic factors are politically important, but they are only part of the story. I argue that different societies are characterized to very different degrees by a specific syndrome of political cultural attitudes; that these cultural differences are relatively enduring, but not immutable; and that they have major political consequences, being closely linked to the viability of democratic institutions.

After flourishing in the 1960s, the con-

cept of political culture came under attack. In 1963 the fountainhead of political culture research, Almond and Verba's *The Civic Culture*, represented a tremendous advance. Previous works that attempted to deal with the impact of culture on politics relied on impressionistic evidence. Cultural influences on the distinctive political behavior of a given people were interpreted in terms of vague but presumably indelible characteristics such as "national character." By providing a well-developed theory of political culture based on cross-national empirical data, Almond and Verba moved from the realm of literary impressions to that of testable propositions.

In subsequent years, it was often charged that political culture was a static concept and that Almond and Verba had ethnocentrically asserted the (presumably permanent) superiority of Anglo-Saxon culture over that of other nations. For though their theoretical interests concerned possible changes in political culture, their analysis was based on data from a single time point and was therefore necessarily static. Empirically, the political culture of a given country could only be treated as a constant. The British and U.S. citizens were, as hypothesized, found to rank higher on interpersonal trust, pride in their political institutions, and feelings of political competence than the publics of Germany, Italy, or Mexico. But since these variables were in fact constants for each country, it was impossible to analyze their relationships with other macrophenomena or to trace changes over time.

The political culture literature argues that the evolution and persistence of mass-based democracy requires the emergence of certain supportive habits and attitudes among the general public. One of the most basic of these attitudes is a sense of interpersonal trust. Almond and Verba (1963) concluded that interpersonal trust is a prerequisite to the forma-

tion of secondary associations, which in turn is essential to effective political participation in any large democracy. A sense of trust is also required for the functioning of the democratic rules of the game: one must view the opposition as a *loyal* opposition, who will not imprison or execute you if you surrender political power but can be relied upon to govern within the laws and to surrender political power reciprocally if your side wins the next election. Almond and Verba found that their German and Italian respondents ranked relatively low on interpersonal trust. With data from only one time point it was impossible to determine whether these findings could be attributed to short-term factors—perhaps the harsh conditions of the post-war era—or whether they reflected more enduring differences. There was some reason to believe that the Italian findings, in particular, might reflect the heritage of long historical experiences (Banfield 1958).

The relationship between (1) a culture of distrust and (2) the presence or absence of modern social structures has the causal ambiguity of the chicken-versus-egg question: Does southern Europe have low levels of trust because it has not yet developed modern organizational structures? Or (in a variation on Weber's protestant ethic thesis) did southern Europe industrialize and develop modern organizational structures later than northern Europe because its traditional culture was relatively low on interpersonal trust? We cannot answer this question conclusively with the data now available. Banfield's interpretation implies that low levels of trust are a persisting feature of given cultures or regional subcultures, which may inhibit economic and political development in those areas. His critics emphasize the impact of economic development on cultural patterns. In our view a reciprocal causal relationship seems likely.

Important though it is, interpersonal

trust alone is not sufficient to support stable mass democracy. A long-term commitment to democratic institutions among the public is also required, in order to sustain democracy when conditions are dire. Even when democracy has no reply to the question, What have you done for me lately? it may be sustained by diffuse feelings that it is an inherently good thing. These feelings in turn may reflect economic and other successes that one experienced long ago or learned about second-hand as a part of one's early socialization. Evidence presented below indicates that the publics of certain societies have much more positive feeling toward the world they live in than do those of other societies. One of the best indicators of this orientation is satisfaction with one's life as a whole. This is a very diffuse attitude. It is not tied to the current performance of the economy or the authorities currently in office or to any specific aspect of society. Partly because it is so diffuse, intercultural differences in this orientation are remarkably enduring and may help shape attitudes toward more specific objects, such as the political system.

From their 1959 fieldwork, Almond and Verba found that (unlike the British or U.S. citizens) few Germans expressed pride in their political institutions. But one of the few aspects of their society in which they *did* express pride was the way their economic system was working. In the short run, this is an inadequate basis for democratic legitimacy. But in the long run, such feelings may contribute to the evolution of broadly favorable orientations toward the institutions under which one lives. Such feelings may play an important role in sustaining the viability of these institutions even when favorable economic or political outputs are not forthcoming. For cultural patterns, once established, possess considerable autonomy and can influence subsequent political and economic events. To demon-

strate this fact, let us now turn to the analysis of data from cross-national surveys carried out during the past 15 years.

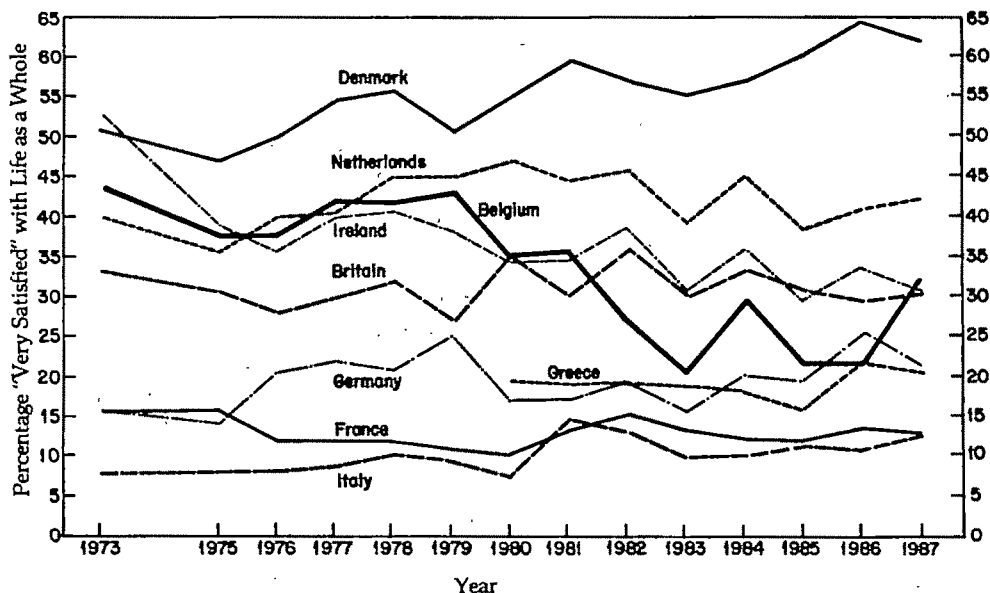
Cross-cultural Differences in Overall Life Satisfaction and Their Political Significance

The study of political culture is based on the implicit assumption that autonomous and reasonably enduring cross-cultural differences exist and that they can have important political consequences. Intuitively, these assumptions seem plausible. But critics of cultural explanations have questioned them, and indeed very little empirical evidence has been presented to support them so far. Since they are crucial assumptions underlying a controversial topic, let us examine a substantial body of evidence in order to see how well these assumptions hold up in longitudinal perspective.

I will start with one of the most basic and central attitudes of all: whether or not one is satisfied with the way things are going in one's life. Figure 1 illustrates the cross-national differences in response to the question, "Generally speaking, how satisfied are you with your life as a whole? Would you say you are very satisfied, fairly satisfied, not very satisfied, or not at all satisfied?" This question has been asked repeatedly in the Euro-Barometer surveys carried out from 1973 to the present. Figure 1 sums up the results from the over 200 thousand interviews in more than two hundred representative national surveys of the publics of nine European community nations.

I find large and remarkably stable cross-cultural differences. And these differences do not reflect objective economic conditions in any direct or simple fashion. Year after year the Italian public shows the lowest level of satisfaction; from 1973

Figure 1. Cross-national Differences in Levels of Satisfaction with Life, 1973-87



Source: Euro-Barometer surveys.

through 1987 they rank last in every year but one (when they rank second-lowest); at no time during this fourteen-year period do more than 15% of the Italian public describe themselves as "very satisfied." The French manifest only slightly higher levels of life satisfaction than the Italians, ranking in second-to-last place in all but two years (rising one rank above this level in one year and falling one rank below it in the other year). At no time do more than 17% of the French public describe themselves as "very satisfied."

At the opposite extreme, the Danes manifest the highest level of overall life satisfaction in every year but one (when they rank second); at no time do less than 47% of the Danish public describe themselves as "very satisfied." On the average, the Danes are six times as likely as the Italians to describe themselves as "very satisfied." The Dutch also rank high consistently, throughout the period from

1973 to 1987; at no time do less than 36% describe themselves as "very satisfied" with their lives as a whole.

The other nationalities maintain their relative positions in remarkably stable fashion, with a sole exception: the Belgians, who consistently ranked among the three most satisfied nationalities throughout the 1970s, show a substantial and protracted decline in the 1980s, falling to sixth place by 1986. In the 1970s, 40-45% of the Belgians consistently described themselves as "very satisfied"; in 1986, the figure had fallen to 21%. This drop of 15-20 points is not immense when compared with the gap of 50 points that separate the Danes from the Italians, but it does represent a substantial decline in the subjective well-being of the Belgian public and stands in dramatic contrast to the overall stability of the cross-national differences manifested throughout this period. The cultural differences are

reasonably stable but not eternal. Short-term fluctuations are present and, as the Belgian case illustrates, significant changes can occur in the relative positions of given nations.

On the whole, the stability shown in Figure 1 is truly remarkable. For one must bear in mind that this was a period of sharp economic upheavals; the crises that occurred in the mid-1970s and the early 1980s constituted the two most severe recessions since the 1930s. Moreover, these crises had a much more severe impact on some societies than others. From 1973 to 1987, life satisfaction declined significantly in both Belgium and Ireland, two of the three countries most severely afflicted by unemployment and inflation during this period. Conversely, life satisfaction showed a modest but perceptible upward trend in Germany, the country with the *lowest* inflation rates from 1973 to 1987. Thus I find a fairly good fit between short-term fluctuations in life satisfaction and the economic experiences of the respective societies.

But a far more impressive aspect of Figure 1 is the cultural continuity that persisted *in spite of* these short-term fluctuations. Despite dramatic economic upheavals from year to year and despite large differences between the experiences of the different countries, I find remarkable stability in the relative positions of these publics. Through thick and thin, the Italians and French remain near the bottom and the Danes and Dutch near the top. And despite the fact that the German economy ranks high both in absolute terms and in relative performance, the life satisfaction levels of the German public consistently rank relatively low. Conversely, both the Irish and the Dutch have much lower per capita incomes than the Germans, and they experienced considerably higher levels of inflation and unemployment during this era than the Germans did, yet they continued to manifest higher levels of life satisfaction

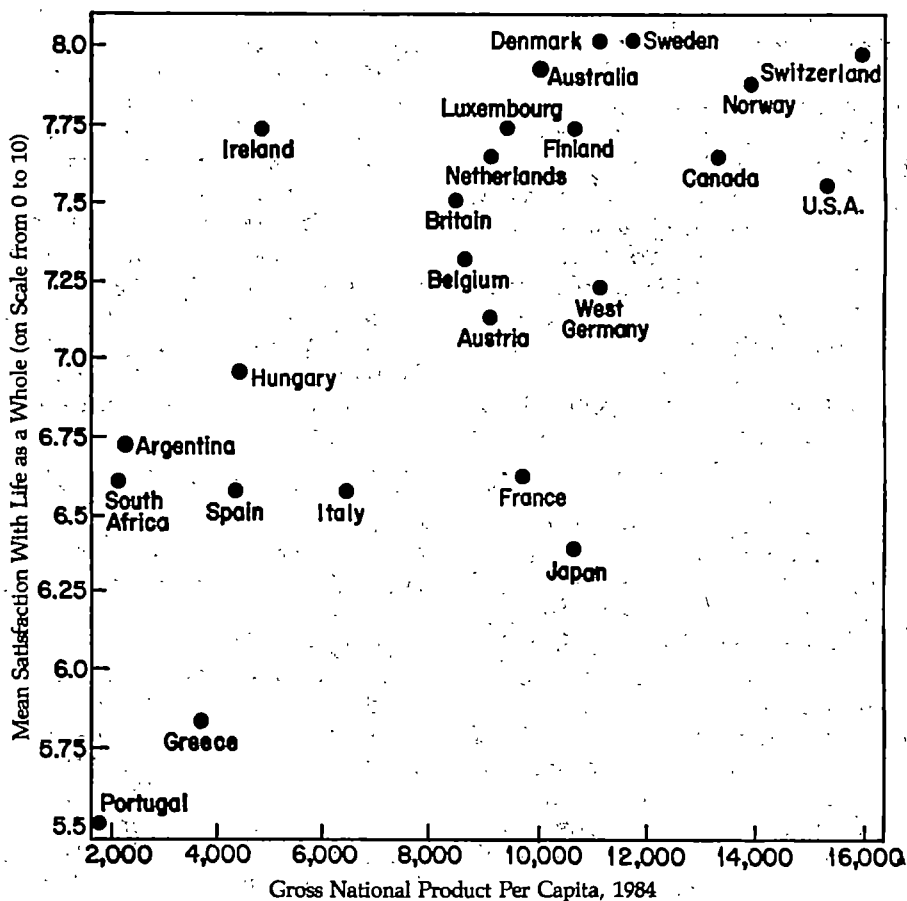
than the Germans. Though cross-cultural differences in life satisfaction respond to economic changes, they do so only with a great deal of inertia.

My conclusion is very simple but very important: there is a durable cultural component underlying these responses. Virtually any survey response is influenced to some extent by the context in which it is asked and this question is no exception: responses reflect both short-term fluctuations (resulting from immediate economic, social, and political events) and a long-term cultural component. Through statistical procedures it is possible to distinguish between the underlying cultural component and the short-term disturbances. In the present case, the long-term cultural differences are so pronounced that one can readily perceive them by mere visual inspection. Enduring cross-cultural differences exist and can be measured.¹

I suggest that the cultural component of these cross-national differences reflects the distinctive historical experience of the respective nationalities. Long periods of disappointed expectations give rise to dissatisfied attitudes. These orientations may be transmitted from generation to generation through preadult socialization. In so far as early learning is relatively persistent, this contributes to the stability of distinctive cultural patterns. The fact that one can to some extent identify the historical causes of given cross-cultural differences does not of course make them disappear. They remain distinctive cultural characteristics with important behavioral consequences.

Is it true that economic security tends to enhance the prevailing sense of life satisfaction in a society, gradually giving rise to a relatively high cultural norm? Empirical evidence supports this supposition. First, as we have seen, there is a tendency for life satisfaction levels to rise or decline gradually in response to short-term economic fluctuations. But one

Figure 2. Mass Life Satisfaction, by Level of Economic Development



Note: $r = .67$.

might suspect that the observed cross-cultural differences reflect long-term historical experiences over generations or even centuries, not just the past dozen years or so. We would need survey data covering the past century or two in order to test this hypothesis directly. They are not available, but we can use the cross-sectional pattern to provide a surrogate test: if economic security is conducive to relatively high levels of life satisfaction, we would expect the publics of prosper-

ous nations to show higher levels of satisfaction than those of poorer ones. The data in Figure 2 tests this hypothesis.

The overall correlation between gross national product per capita and life satisfaction in Figure 2 is .67: prosperity is linked with relatively high levels of life satisfaction among the 24 nations for which we have data from the 1980s. This point has been a matter of controversy in previous studies. Cantril (1965) analyzed data gathered in the 1950s from 14 coun-

tries and found that the publics of richer nations *did* show relatively high levels of subjective well-being. Easterlin (1974) re-analyzed the Cantril data and concluded that the correlation was actually rather weak. Emphasizing the fact that some poor nations (such as Egypt) showed higher levels of life satisfaction than some relatively wealthy ones (such as West Germany), he argued that economic development had little impact on subjective well-being. In a more recent study based on a broader range of nations Gallup (1976) found a relatively strong correlation between economic development and life satisfaction. He concluded that the two *are* linked. The present data also show a relatively strong correlation between economic development and life satisfaction. How we interpret this depends on one's theoretical expectations. If one approaches the topic with the expectation that subjective well-being will be found almost entirely a matter of economic factors, as Easterlin apparently did, then the crucial finding is that this is not the case. With the data in Figure 2 a nation's economic level explains less than half of the variance in life satisfaction. In the data Easterlin analyzed economic factors explain an even smaller share of the variance. From Easterlin's perspective, economic determinism was clearly discredited.

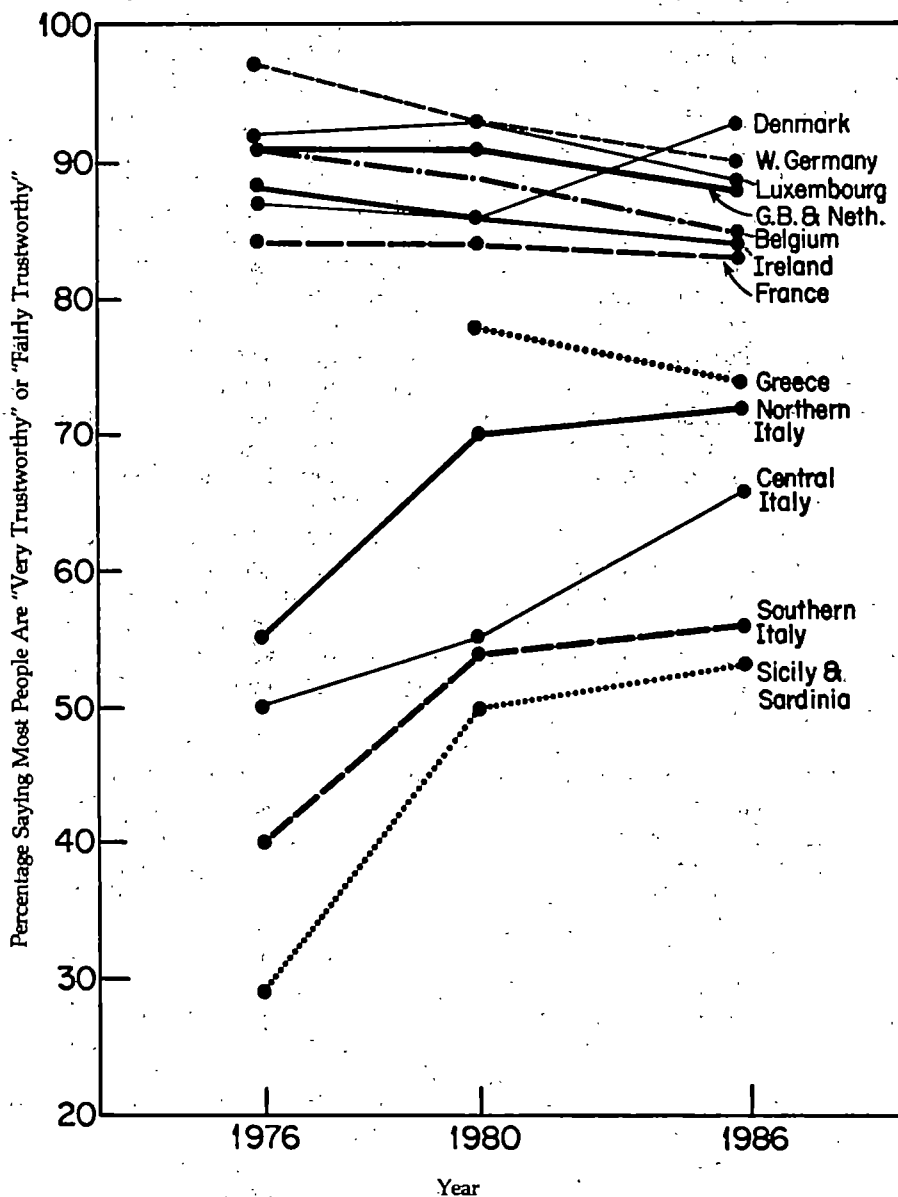
However, if one approaches the question with the expectation that a nation's level of economic development is only one of a number of historical factors that influence cross-cultural differences in life satisfaction, then the data clearly do support the hypothesis. Here, as in Easterlin's analysis, one can point to some striking deviant cases. Nevertheless, the overall pattern is clear: wealthier nations tend to show higher levels of life satisfaction than poorer ones.

The Euro-Barometer surveys also provide regular readings on political satisfaction, which shows much more short-term

fluctuation than life satisfaction, for it explicitly refers to the political system and accordingly behaves like an indicator of governmental popularity, fluctuating from one month to the next in response to current economic conditions and political events. When a conservative government is in office, those who identify with the Right show higher levels of political satisfaction; when a government of the Left gains power, those on the Left show higher levels. Political satisfaction fluctuates in response to current economic and political events. But it is clear that a significant cultural component *also* is present underneath these fluctuations: the publics of some countries are consistently more satisfied than others. Moreover, these differences reflect the now-familiar pattern found with life satisfaction and happiness: the publics of France and Italy almost always rank lower on political satisfaction than those of other nations: at the national level, the correlation between life satisfaction and political satisfaction is .41.

Both overall life satisfaction and political satisfaction are correlated with stable democracy. I will examine this relationship below. For the moment let me simply note that political satisfaction levels are only weakly linked with the number of years that democratic institutions have persisted in a given nation ($r = .21$). But *despite* the direct and obvious political relevance of political satisfaction, life satisfaction is far more strongly linked with stable democracy ($r = .85$). Democratic institutions seem to depend on enduring cultural traits such as life satisfaction and interpersonal trust, more than on relatively fluctuating variables such as political satisfaction. The latter may well be a better predictor of the popularity of a given government at a given moment. But precisely *because* it fluctuates with short-term conditions, it is less effective in maintaining the long-term stability of democratic institutions.

Figure 3. Interpersonal Trust among European Publics, 1976-86



Source: Data from Euro-Barometer surveys 6, 14, and 25.

Interpersonal Trust, Economic Development, and Democracy

Following Banfield, Almond and Verba, Wylie, and others, I hypothesized that interpersonal trust is part of an enduring cultural syndrome that is conducive to the viability of democracy. The first question one must answer is, Do enduring intercultural differences exist in interpersonal trust? Unless they do, any argument concerning its long-term political impact is on shaky ground.

The evidence indicates that given societies are indeed characterized by distinctive levels of interpersonal trust. This even seems to be true of specific regions *within* given countries. Euro-Barometer surveys in 1976, 1980 and 1986 asked, "Now I would like to ask about how much you would trust people from various countries. For each country, please say whether, in your opinion, they are generally very trustworthy, fairly trustworthy, not particularly trustworthy, or not at all trustworthy."

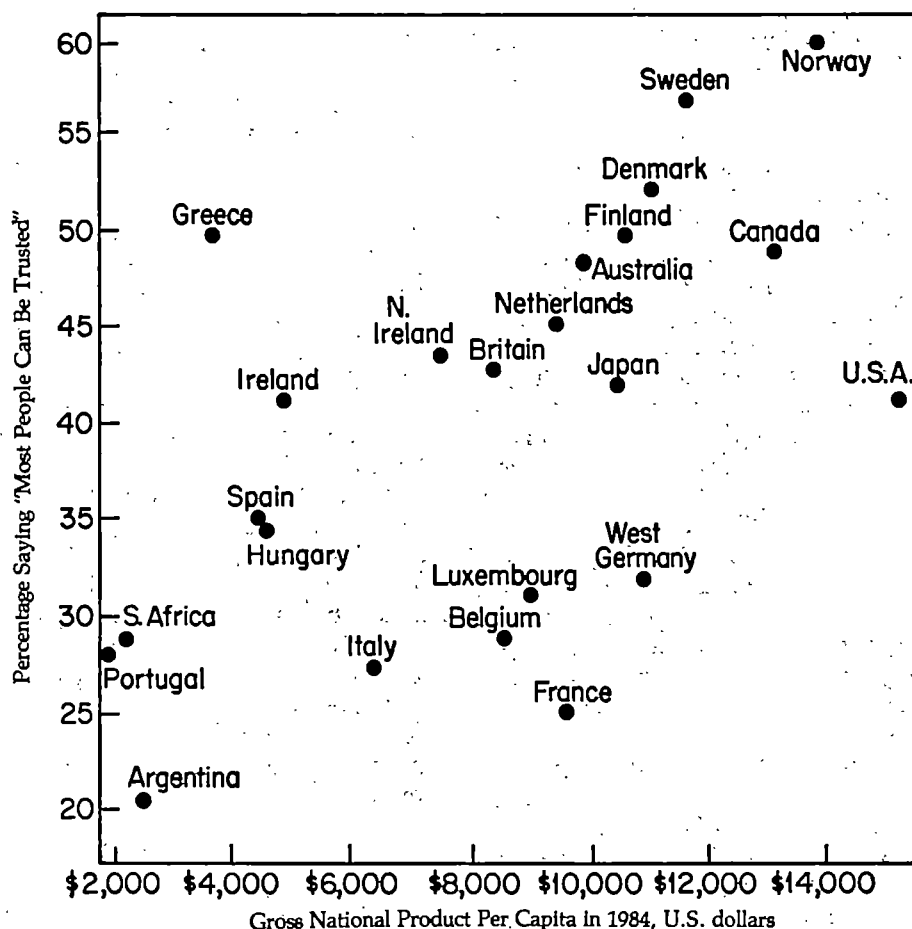
Figure 3 depicts the relative levels of interpersonal trust expressed during this 10-year period toward people of one's *own* nationality. As is immediately evident, trust levels in given countries are extremely stable. In a pattern that is becoming increasingly familiar, the Italian public ranks lowest at every point in time (with four major regions of Italy each retaining its relative position). The Greeks rank next, followed by the French. The other nationalities are clustered close together, with relative rankings consistently remaining within the band between 85% and 95%. One line in Figure 3, labeled "G.B. & Neth.," depicts the levels of *both* the British and the Dutch, which are identical at all three points. Remarkably pronounced and durable differences in trust exist among the various regions of Italy. Precisely as Banfield (1958) found many years ago, southern Italy seems to be characterized by lower levels of in-

terpersonal trust than northern or central Italy and than any other Western society for which we have data.

These findings of stable regional differences in Italian political culture are consistent with earlier findings by Putnam and his colleagues (1983) based on ecological data covering a much longer period of time. In an imaginative and elegant analysis, these authors utilize various indicators of social involvement and political mobilization (such as membership in mutual aid societies, union membership, and electoral turnout) to derive a measure of "civic culture." This variable manifests remarkable stability at the regional level: their index of civic culture as measured in the 1970s correlates at $r = .91$ with the strength of mass parties in 1919-21 and at $r = .84$ with the strength of mutual aid societies from 1873 to 1904. Civic culture in turn proves to be strongly correlated with the relative success of the new regional governments established throughout Italy in 1970. The authors conclude that political success or failure largely reflects the impact of autonomous cultural factors, independent of underlying economic variables:

Contrary to any simple-minded economic determinism, these regional continuities in political culture are strikingly greater than continuities in economic structure or social well-being. For example, the agricultural share of the regional work force in 1970 correlated $r = -.02$ with the same figure in 1870; the equivalent statistic for infant mortality is $r = .01$. Those regions with a relatively agricultural economy in 1970 had not been the more agricultural regions a century earlier, and the regions with good public health in 1970 had not been the healthier ones in 1870. But the regions characterized by political activism and social solidarity in the 1970s were essentially the same regions that had been so a century earlier. In short, we can trace with remarkable fidelity over the last hundred years the historical antecedents of just those aspects of regional political culture—mass participation and civic solidarity—that in turn provide such a powerful explanation for contemporary institutional success." (Putnam et al. 1983, 69-70)

Figure 4. Economic Development and Interpersonal Trust



Sources: Data from World Values Survey, 1981 and for Greece, Portugal and Luxembourg Euro-Barometer Survey 25 (April 1986).

Note: $r = .53$.

Despite evidence of impressive stability in the propensity to trust others, trust is not a fixed genetic characteristic: it is cultural, shaped by the historical experiences of given peoples and subject to change. Fieldwork carried out for *The Civic Culture* found that the publics of the two long-established English-speaking democracies had markedly higher levels of interpersonal trust than those of Mexico, West Germany, or Italy. The Italian

public, in particular, manifested trust levels that were phenomenally low in 1959. But as our theoretical framework implies, the economic miracles that took place in both West Germany and Italy during the 1950s and 1960s eventually had an impact on the political culture of these countries. Though the Italian public still remained relatively low on trust in 1981 and 1986, absolute levels of trust had more than tripled by 1981 and almost

Political Culture

Table 1. Support for Radical Change, Gradual Reform, or Defense of Present Society, in 20 Societies, 1981 (%)

Society	Radical Change ^a	Gradual Reform ^b	Defend Present Society ^c	Number of Cases
N. Ireland	1	74	25	287
Norway	2	50	49	1,146
W. Germany	3	59	38	1,149
Denmark	3	69	28	1,026
Japan	3	71	26	707
Netherlands	3	70	26	1,063
Sweden	4	80	16	797
Ireland	4	76	20	1,084
Luxembourg	5	69	25	760
Canada	5	74	21	1,135
Britain	5	73	23	1,136
U.S.A.	5	73	22	2,101
Belgium	7	73	21	897
Italy	8	73	19	1,270
Spain	8	82	10	2,091
Greece	9	57	26	2,000
France	9	73	19	1,123
Mexico	12	77	11	1,610
Portugal	14	74	12	813
S. Africa	25	54	21	1,182

Sources: World Values survey, 1981; except that data for Greece and Luxembourg are from Euro-Barometers 15 and 16 (carried out in April and October 1981), and Portuguese data are from Euro-Barometer 24 (carried out in November 1985). These are the earliest such data available from Portugal.

^a"The entire way our society is organized must be radically changed by revolutionary action."

^b"Our society must be gradually improved by reforms."

^c"Our present society must be valiantly defended against all subversive forces."

are reasonably strong grounds for thinking that while it may work both ways, the former of the two processes is mainly responsible for the relationship. First, knowing the relatively modest place that politics has on most people's intellectual horizons, it seems far more plausible that people vote Communist or neo-Fascist because they are dissatisfied and distrustful than that they are dissatisfied and distrustful because they vote Communist (or as a right-wing fantasy might have it, that communism spreads unhappiness and distrust among the public). Secondly, low levels of trust, satisfaction, and happiness are broad cultural characteristics of the French, Greek, and Italian publics, only marginally more prevalent among the electorates of the

Communists and extreme Right than elsewhere. If the Communists sow unhappiness, they sow it broadly, not just among their own supporters. Finally, there are indications that gradual cultural quadrupled by 1986—a phenomenon that parallels the rise in Italy shown in Figure 3. The growth of interpersonal trust among the West German public was proportionately smaller, because they started from a higher level. But by 1986 the Germans had actually surpassed the British in interpersonal trust. The other side of the coin is the fact that interpersonal trust had shown a long-term decline in the two English-speaking democracies.

Like life satisfaction and happiness, high interpersonal trust goes with relatively high levels of economic develop-

ment, as Figure 4 illustrates. The cross-sectional correlations is .53. The available data do not enable one to determine whether this is because interpersonal trust is conducive to economic development or because economic development leads to an enhanced sense of security that is conducive to trust or whether (as we suspect) the two processes are mutually supportive. It is interesting that in the two countries from which I have evidence of a dramatic rise in interpersonal trust (West Germany and Italy) this phenomenon took place after a period of dramatic economic recovery. But it is clear that economic factors alone are not decisive; for the publics of both Britain and the United States were wealthier in the 1980s than they were in 1959-60, but both experienced an erosion of interpersonal trust from 1960 to 1981.

As we will see shortly, high levels of interpersonal trust are also linked with stable democracy. But before analyzing this relationship, let us consider the implications of a recurrent finding in the data. With remarkable consistency, the publics of France and Italy rank lowest on the syndrome of attitudes that might loosely be called the civic culture. Among those societies for which time series data from 1973 to the present exist, the French and Italian publics nearly always rank lowest on life satisfaction, happiness, political satisfaction, and trust. And these are precisely the countries that have been characterized by the largest antisystem parties: in France since the end of World War I the Communists have normally polled a fifth or more of the total vote in national elections. More recently, a party of the extreme Right, the National Front, won about 10% of the vote in nationwide elections in 1984 and again in 1986, recalling the sudden mushrooming of support for another extreme Right party, the Poujadists, that took place in the 1950s. Similarly, in Italy the Communists have generally won about a third of the vote

since 1945, while the neo-Fascist Italian Social Movement has often won as much as 10% of the vote in national elections.

Data from Greece are available only since 1980, but they fit the pattern just described. To be sure, the Greek public has shown relatively high levels of satisfaction with the way democracy is functioning since democratic institutions were restored in the 1970s. But as I have already noted, this variable fluctuates with short-term events. On most of the long-term civic culture indicators such as life satisfaction, happiness, and support for the existing social order, the Greek public ranks relatively low. In keeping with this pattern, support for antisystem parties has been relatively high in Greece. The Communist vote has been higher than in any of the other societies for which time series data exist, apart from France and Italy; and there has also been significant support for parties of the extreme Right.

I believe that relatively low levels of diffuse satisfaction and trust make one more likely to reject the existing political system and support parties of the extreme Right or Left. Again, we have the chicken-versus-egg question: Does a culture of dissatisfaction and distrust give rise to an extremist vote or do extremist parties produce distrust and dissatisfaction? The available empirical data do not allow me to provide a conclusive answer; but there change *precedes* changes in the vote. Table 1 shows the cross-national pattern of responses to the following question:

On this card (*show card*) are three basic kinds of attitudes concerning the kind of society we live in. Please choose the one which best describes your own opinion.

1. The entire way our society is organized must be radically changed by revolutionary action.
2. Our society must be gradually improved by reforms.
3. Our present society must be valiantly defended against all subversive forces.

As Table 1 demonstrates, there is wide cross-national variation in response to this question. In the early 1980s, support for the revolutionary option ranged from a low of 1% or 2% in Northern Ireland and Norway to highs of 12%-25% in Mexico, Portugal, and South Africa. Conversely, support for defense of the status quo ranged from highs of 49% and 38% respectively in Norway and West Germany to lows of 10%-12% in Mexico, Spain, and Portugal. The revolutionary option was most likely to be endorsed in societies with a relatively low per capita GNP ($r = -.68$), while the conservative option was most likely to be endorsed in societies with a relatively high per capita GNP ($r = .58$). And, as one might expect, support for the revolutionary option is negatively correlated with life satisfaction ($r = -.52$); while support for the conservative option shows a positive correlation with life satisfaction ($r = .31$).

Responses to this question among the nine European Community nations from which we have data from 1976 through 1986 are quite stable, apart from a gradual decline in support for the revolutionary option. In 1976 it was supported by 9% of the public in the European Community as a whole but in 1986 by only 5%. This decline was gradual, never falling by more than one percentage point per year and was pervasive, with most nations showing declining support for revolution. But the phenomenon was especially concentrated among the publics of France and Italy, where prorevolutionaries constituted 14% of the public in both countries in 1976 but only 7% and 8% respectively in 1986. In part, this reflects the deradicalization of the Communist electorates of France and Italy, which preceded the electoral decline experienced in the mid-1980s by the Communist parties of these two countries (and to some extent, throughout Western Europe). Moreover, it may be a harbinger of favorable prospects for the persistence

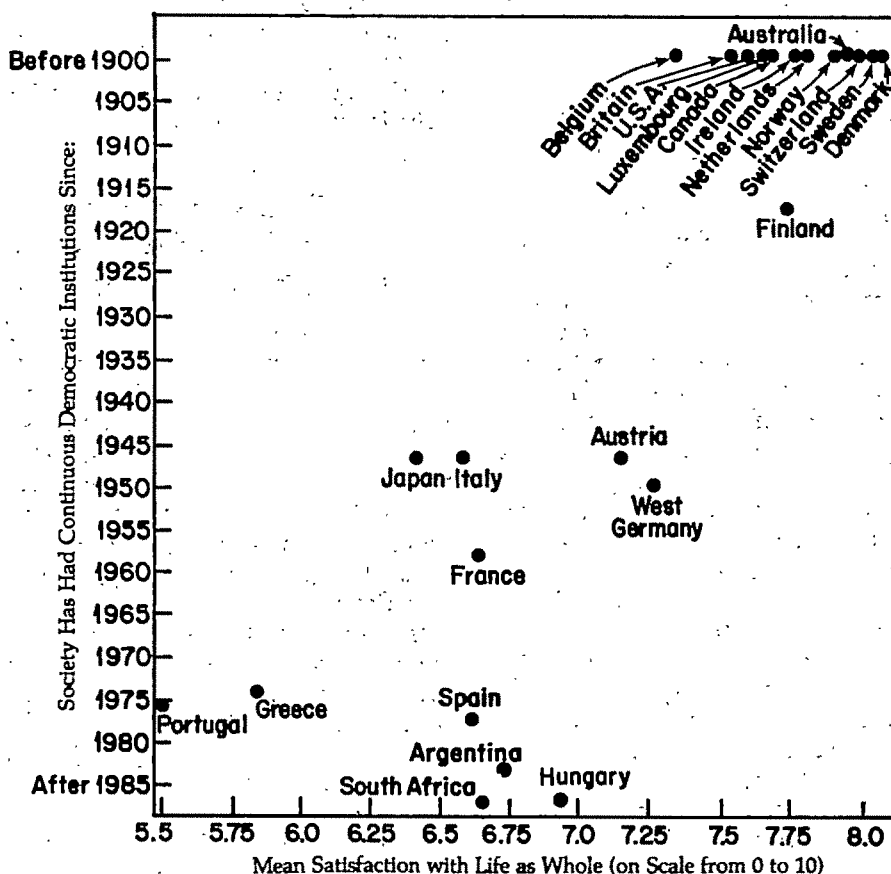
of democratic institutions in these countries, for my data show a correlation of $-.73$ between support for the revolutionary option, and the number of continuous years that democratic institutions have functioned in a given nation.

The Consequences of Political Culture: Some Speculations with Data

Let us sum up what we have learned so far. I find a broad syndrome of related attitudes that show substantial and consistent cross-cultural variation, with certain societies being characterized by satisfied and trusting attitudes to a much greater degree than others. The cross-national differences show impressive stability over time. Though they can vary (and the variations are of great substantive interest), they tend to be relatively enduring cultural characteristics. Finally, this syndrome is linked with the persistence of democratic institutions.

Life satisfaction, political satisfaction, interpersonal trust, and support for the existing social order all tend to go together. They constitute a syndrome of positive attitudes toward the world one lives in. And this syndrome goes with enduring democratic institutions. Is the linkage a causal one? Such causal linkages are difficult to demonstrate conclusively. To do so would require political culture data from a large number of nations, some of which became democracies during the course of a long time series, while others did not. My interpretation implies that those societies characterized by high levels of life satisfaction (as well as interpersonal trust, tolerance, etc.) would be likelier to adopt and maintain democratic institutions than those whose publics lacked such attitudes. Conversely, democratic institutions would be more likely to flounder in societies with low levels of life satisfaction, trust, and so on.

Figure 5. Mass Life Satisfaction and Stable Democracy



Sources: Data from World Values survey and Euro-Barometer survey.

Note: $r = .85$.

Such data are not now available and will be difficult to obtain, both because it will require a long-term data-gathering process in many countries during many decades and because the governments of nondemocratic societies usually make it difficult to carry out survey research. In principle, however, it is possible to acquire such data, and this is a goal worth striving for. We may not attain the optimum, but we can certainly improve on what we have now. In the meantime, let

us examine the cross-national pattern: do democratic institutions seem to have emerged earlier and persisted longer in societies with high levels of overall life satisfaction, than in those characterized by relatively low levels?

As Figure 5 demonstrates, the answer is yes. There is a remarkably consistent tendency for high levels of life satisfaction to go together with the persistence of democratic institutions over relatively long periods of time. Among the 24

societies depicted in Figure 5, the overall correlation between life satisfaction and the number of continuous years a given society has functioned as a democracy is .85. Needless to say, the causal inference would be on firmer ground if I had survey data on life satisfaction levels from some much earlier point in time, such as 1900, but such data is not available. I use *ex post facto* data from 1981 as an indicator of the relative rankings earlier in history. The evidence indicates that these rankings are pretty stable, but this procedure undoubtedly introduces some error in measurement (which tends to work *against* my hypothesis). Since our focus is on the effects of domestic political culture, I code democracy as having failed to survive only when it collapsed through *internal* causes and not when it did so as a consequence of foreign conquest. By this definition, literally *all* of the 13 societies that have maintained democratic institutions continuously since 1920 or earlier, show relatively high life satisfaction levels (above 7.25 on a scale from 0 to 10). Among the 11 societies in which democratic institutions have emerged only since 1945 or which are not yet fully democratic all but one show mean life satisfaction levels below 7.25 and in some cases far below it. Some data are also available from Mexico, but because they are not based on a representative national sample, they have been omitted from these quantitative cross-national analyses. For what they are worth, these data show surprisingly high levels of life satisfaction, paralleling earlier findings by Almond and Verba that the Mexican public showed anomalously high levels of satisfaction with their political system despite relatively negative conditions.

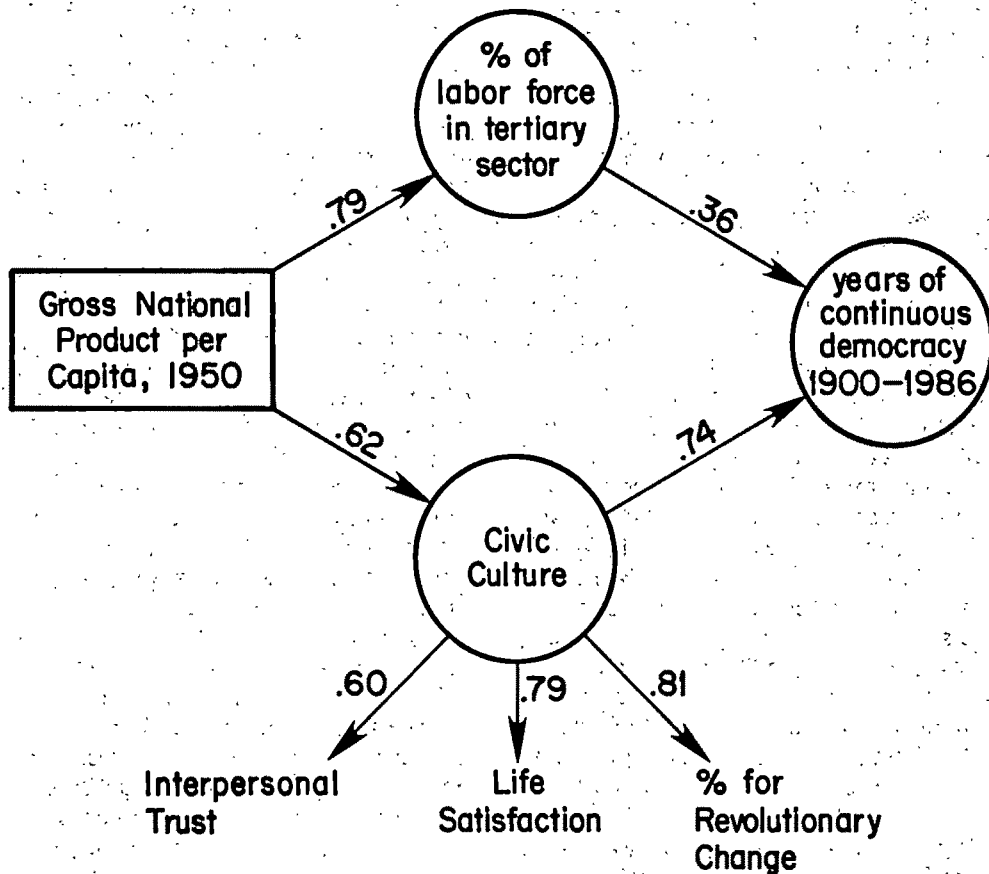
The coding of France might seem questionable. It is coded as having continuous democratic institutions only since 1958, since the military uprising that brought the Fourth Republic to an end and brought DeGaulle to power was not of

foreign origin but was carried out by the French Army. It is true that the last president of the Fourth Republic invited DeGaulle to form a government. But he did so only under pressure from the French Army. Free elections were held shortly afterward, so the suspension of democracy was very brief; but as the subsequent military uprising of 1960 and 1962 testify, democracy in France was for a time on shaky ground.

It is conceivable that we have the causal arrow reversed. Perhaps many decades of living under democratic institutions produces greater life satisfaction. We don't rule this factor out. Indeed, we think it *does* contribute to overall life satisfaction somewhat. But theoretical considerations suggest that the process mainly works the other way around. It seems more likely that a global sense of well-being would also shape one's attitudes toward politics than that what is experienced in one relatively narrow aspect of life would determine one's overall sense of satisfaction. In keeping with this reasoning, Andrews and Withey (1976) find that political satisfaction has only a relatively modest impact on most people's overall life satisfaction; satisfaction with one's job, home, family life, and leisure time all make larger contributions.

Overall life satisfaction is part of a broad syndrome of attitudes reflecting whether one has relatively positive or negative attitudes toward the world in which one lives. Life satisfaction, happiness, interpersonal trust, and whether one supports radical social change or defends one's existing society, all tend to go together in a cultural cluster that is closely related to whether or not democratic institutions have persisted for a long time in a given society. These attitudes seem to be a deep-seated aspect of given cultures, constituting a long-term component underlying absolute levels of satisfaction with governmental performance at any given time. Though political

Figure 6. Economic and Cultural Prerequisites of Stable Democracy



Source: LISREL analysis of data from the societies shown in Figure 5.

Note: Adjusted goodness of fit index = .88.

satisfaction shows sharp fluctuations from one month to the next, the publics of some societies consistently manifest higher levels of satisfaction than the publics of others.

This syndrome is also linked with a society's economic level. The more developed societies tend to rank relatively high on life satisfaction, trust, and the other components of the syndrome. Are both it and liberal democracy simply joint consequences of economic development

or does political culture make an autonomous contribution to the viability of democratic institutions, as Almond and Verba have argued? Let me emphasize that we do not yet possess a data base that would enable us to answer such questions conclusively. We have established the presence of an enduring and cross-nationally distinctive syndrome of basic cultural attitudes and demonstrated that this syndrome is much stronger in those societies that have been stable democra-

cies since 1900 than in those that have been nondemocratic or intermittently democratic. But we do not yet have sufficient data to sort out the causal linkages between political culture, economic development, and democracy in any conclusive fashion. To do so would require regular measures of the relevant cultural variables throughout the past century. At present, they are simply not available. Nevertheless, the patterns shown in Figures 2, 4, and 5 are so striking and their implications are so important that they virtually cry out for further exploration. The remainder of this article will be devoted to some analyses that cannot be conclusive, but which suggest that cultural factors may play an extremely significant role in both political and economic development. I hope these analyses may stimulate further historical analysis and the development of the longitudinal data base necessary for any definitive analysis of the role of culture.

Figure 6 shows the results of a LISREL analysis in which the dependent variable is the number of consecutive years since 1900 that democracy has functioned in a given society. The results indicate that economic development *per se* does *not* necessarily lead to democracy. Only in so far as it brings appropriate changes in social structure and political culture does it enhance the viability of democratic institutions. To take an extreme illustration, such nations as Saudi Arabia, Kuwait, and Libya are quite wealthy, but neither their social structures nor their political cultures seem favorable to democracy.

Clearly, I would prefer to have data gathered at earlier time points for some of my variables, especially the cultural indicators. I use gross national product per capita in 1950 as an indicator of a given nation's level of economic development, this being the earliest time for which I have reliable data from all 24 societies. Theoretically, an even earlier time point might better tap the impact of economic

development on the growth of the tertiary sector. However, the 1950 data were measured about 30 years before the political culture indicators (based on 1981 surveys), providing a time-lag of about one generation between economic and cultural indicators, which is appropriate for present purposes.

The results suggest that political culture is a crucial link between economic development and democracy. Although GNP per capita has a strong zero-order correlation with stable democracy, its effects are almost entirely due to its linkages with social structure and political culture, with the latter being the more important of the two. Earlier versions of the LISREL model allowed for a direct linkage between economic development and democracy, but its empirical importance was so insignificant ($r = .08$) that the final model shown in Figure 6 omits it. A given nation's level of economic development is closely linked with a set of characteristics that we label the "civic culture" (though it is only roughly equivalent to Almond and Verba's construct). This political culture syndrome is tapped by three indicators: (1) interpersonal trust, (2) life satisfaction, and (3) support for revolutionary change, the latter being negatively correlated with the "civic culture." All three variables are good indicators of this underlying cultural dimension, with the second and third indicators showing almost identical correlations, despite the fact that in face content, overall life satisfaction has no obvious relationship to politics, while support for revolutionary change clearly does. Earlier versions of the model also included a fourth indicator of the civic culture, satisfaction with the way democracy is functioning in one's country. Though in face content this variable has the most obvious reference to democratic institutions, it manifests a relatively large amount of short-term fluctuation. This variable seems to be a better indicator of governmental popularity at a given time than of

long-term support for democracy. And though it has a significant correlation with the civic culture dimension ($r = .54$), it taps it less accurately than the other three indicators. The same is true of a given public's rate of political discussion. With the small number of cases at my disposal, I obtain a better fitting model by omitting these variables.

The underlying "civic culture" tapped by these three indicators shows a strong linkage with the number of years that democratic institutions have functioned in the given society: the regression coefficient is .74, controlling for the effects of social structure. This suggests that over half of the variance in the persistence of democratic institutions can be attributed to the effects of political culture alone.

The size of the tertiary sector also has a significant linkage with the persistence of democracy. A measure of the relative size of the middle class as opposed to those with blue collar or agricultural occupations, it is also an indirect indicator of the strength of commercial elites. The importance of this variable provides empirical support for the arguments of theorists from Marx to Lipset, who have emphasized the significance of the development of a commercial-industrial middle class. In part this variable reflects purely economic factors: a less polarized distribution of income and the presence of a middle majority tend to make political conflict less ruthless and less desperate. But historically, democratic institutions emerged long *before* the redistributive welfare state and the middle majority: democracy emerged when power passed into the hands of the bourgeoisie. Moore (1966) takes it as axiomatic that there is an inherent affinity between democracy and the bourgeoisie. But if one presses farther and inquires *why* this is so, one is likely to arrive at a cultural explanation as a crucial intervening variable. Preindustrial agrarian societies are typically dominated by landed aristocracies exercising a

military function or by priestly elites, both of which are accustomed to social relations based on a hierarchical chain of command structure. Commercial elites, by contrast, necessarily become accustomed to bargaining. In market relationships the buyer and seller may have diametrically opposed interests, the seller seeking the highest possible price and the buyer the lowest, but unless they can reach a compromise that is acceptable to both sides, neither can do business.

It seems clear that viable democracy does *not* depend on economic factors alone. Specific cultural conditions are crucial, and they in turn are related to economic and macropolitical developments. Long-term economic success can help provide legitimacy for any type of regime in industrial society. Thus it can help maintain the viability of democratic institutions once they are established. But unless economic development is accompanied by certain changes in social structure and political culture, liberal democracy is unlikely to result. Moreover, external constraints can prevent the emergence of democracy even when internal factors are favorable. Huntington (1984) argues that Czechoslovakia and (probably) Poland and Hungary have social and cultural conditions that would favor increasing democratization, in the absence of direct or indirect Soviet intervention. To what degree various East European societies now have political cultures that would support democratic institutions if the Soviet Union permitted them to emerge is a fascinating but unanswered question. It would be as gross an oversimplification to believe in sheer cultural determinism as to believe in economic determinism. Stable democracy reflects the interaction of economic, political, *and* cultural factors. While economic development does not automatically bring about democracy, it does seem to be linked with sociocultural changes that enhance its chances.

Cultural Change and Economic Development

I have established that certain societies are characterized relatively strongly by a durable set of orientations that roughly corresponds to the "civic culture" discussed by Almond and Verba and that this cultural pattern shows a strong empirical linkage with stable democracy even when I control for related aspects of social structure and economic development. In other words, a body of evidence that not only is much larger than that available to Almond and Verba but also extends over a number of years tends to confirm the basic thesis of *The Civic Culture*.

But the civic culture itself is only one aspect of a still broader cultural syndrome that seems to reflect a long-term process of social and economic change. To understand this process, one must see it in the context of historical developments that have taken place over the past few centuries. Though we do not have survey data that would enable us to trace directly the long-term cultural changes linked with this process, some available indicators enable us at least to suggest the broad outlines of what has been happening.

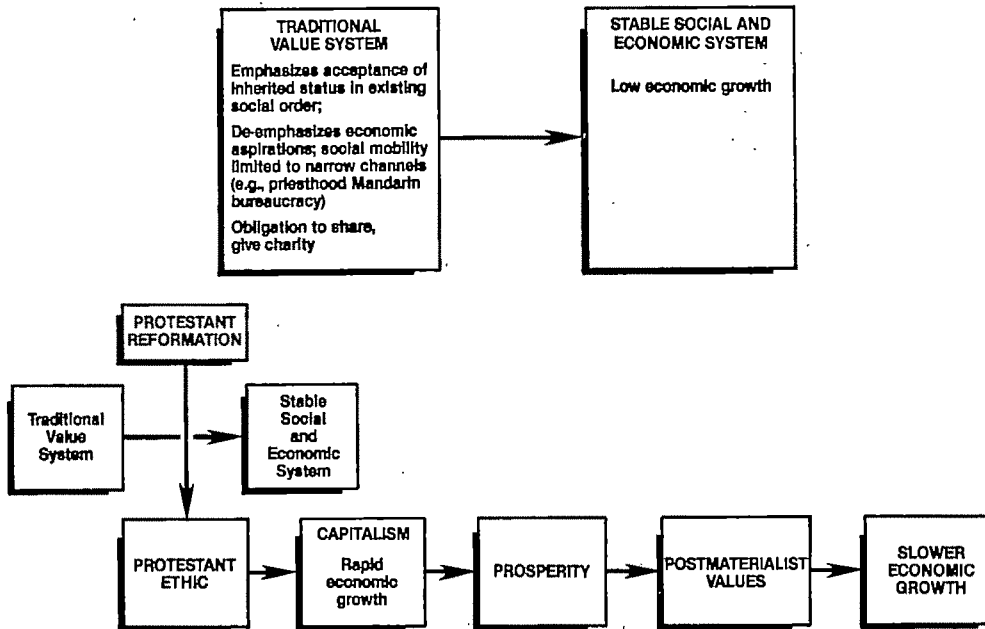
Max Weber (1958) argued at the turn of the century that the rise of capitalism and the subsequent rapid economic development of the West were made possible by a set of cultural changes related to the emergence of Calvinist Protestantism. His Protestant ethic thesis gave rise to a controversy that endured for decades. Some of the criticisms seem well founded; and the thesis that economic achievement was linked with Protestantism may seem unconvincing today, when predominantly Catholic countries have higher economic growth rates than Protestant ones. Nevertheless, though I would not defend Weber's thesis in its entirety, I believe that important aspects of it were correct, provided his work is viewed as an analysis of

a specific historical phenomenon (as was clearly Weber's intention) and not as asserting an immutable relationship between economic achievement and Protestantism. Particularly crucial is Weber's insight that culture is not simply an epiphenomenon determined by economics but an autonomous set of factors that sometimes shape economic events as well as being shaped by them.

I utilize the dominant religious tradition of a given society as an indicator of its preindustrial cultural heritage. This is, of course, an oversimplified indicator, though not as oversimplified as it may seem from today's perspective. Contemporary social scientists tend to underestimate the historical importance of religion, both because they are social scientists, habituated to viewing the world from a secular and scientific viewpoint, and because they are contemporary and live in societies in which the functions of religion have diminished drastically. In most agrarian societies, religion is an overwhelmingly important force, filling the functions that educational and scientific institutions, the mass media, art museums, and philanthropic foundations, as well as religious institutions, now fill in advanced industrial societies. In modern societies, religion is a far less adequate indicator of the culture as a whole, which becomes more differentiated and subject to more rapid change. But my interest here is on the impact of the *preindustrial* cultural heritage of given societies. Though religion is only a rough indicator of this heritage, I use it for want of more refined measures of the value systems prevailing at given times and places in the past.

Figure 7 diagrams a long-term process of economic and cultural change that led to the emergence of democracy in the West as one consequence. As this figure suggests, the relationship between economic and cultural change is one of complex reciprocal causality, with

Figure 7. Long-Term Economic Consequences of Rise of Protestant Ethic



Note: With varying time lags, the impact of the Industrialized nations serves as a functional equivalent to the Protestant Reformation for the rest of the world, though the value system that emerges is shaped by the traditional value system that precedes it.

cultural factors not only being influenced by economic change, but also influencing it. Weber argues that Calvinist Protestantism gradually evolved into a value system that viewed the accumulation of wealth for its own sake (and not as a means to survive or acquire luxuries) as a sign of divine grace and encouraged an ascetic self-control conducive to the accumulation of wealth. This led to an entrepreneurial spirit and an accumulation of capital that facilitated the Industrial Revolution in the eighteenth and nineteenth centuries, which in turn had immense consequences for global economic development in the twentieth century.

I suggest that the Protestant Reformation was only one case of a more general phenomenon: the breakdown of traditional cultural barriers to economic

modernization. For as the top half of Figure 7 suggests, one feature common to traditional value systems is that they emerge in, and are adapted to, economies characterized by very little technological change and economic growth. In this situation, social mobility is a zero-sum game, heavily laden with conflict and threatening to the social system. In a society undergoing rapid industrialization and expansion, by contrast, social mobility may be widespread. But in traditional agrarian societies, social status is hereditary, except when an individual or group forcibly seizes the lands and social status of another. To preserve social peace, virtually all traditional cultures discourage upward social mobility and the accumulation of wealth. These cultures perform an integrating function by providing a ra-

tionale that legitimates the established social order and inculcating norms of sharing, charity, and other obligations that help to mitigate the harshness of a subsistence economy.

By their very nature the traditional value systems of agrarian society are adapted to maintaining a stable balance in unchanging societies. Accordingly, they tend to discourage social change in general and the accumulative entrepreneurial spirit in particular. One of the important functions of the Protestant Reformation was to break the grip of the medieval Christian world view on a significant portion of Europe. It did not accomplish this alone. The emergence of scientific inquiry had already begun to undermine the anthropocentric cosmos of the medieval Christian tradition. But it is difficult to avoid the conclusion that Weber's emphasis on the role of Protestantism captures an important part of reality. For prior to the Protestant Reformation, southern Europe was economically more advanced than northern Europe. During the three centuries after the Reformation, capitalism emerged, mainly among the Protestant regions of Europe and among the Protestant minorities of Catholic countries. Protestant Europe manifested a subsequent economic dynamism that was extraordinary, moving it far ahead of Catholic Europe. Shifting trade patterns, declining food production in southern Europe, and other variables played a role in this shift, but the evidence suggests that cultural factors were also important.

As capitalism led to industrialization and eventually to historically unprecedented levels of prosperity, emulation became more and more attractive, and increasing amounts of cross-cultural diffusion took place. But to a truly remarkable degree, throughout the early stages the Protestant cultural zone was markedly more receptive to industrialization and economic development than any other

part of the world. The Industrial Revolution began in England, spreading rapidly to predominantly Protestant Scotland and Wales but leaving Catholic Ireland largely untouched except for the Protestant region around Belfast. Industrialization spread from England to nearby France but lagged there in comparison with its rapid implantation in more distant but more receptive areas such as the United States and Germany, both of which soon became far more industrialized than France. At the start of the twentieth century, the correlation between Protestantism and economic development was still remarkably strong. In Europe, the economically most dynamic nations were Great Britain, Germany, Sweden, Denmark, Norway, The Netherlands, and Switzerland, all of which were predominantly Protestant at that time. The only non-Protestant countries that had attained even roughly comparable levels of economic development were Belgium and France, both of which were geographically near the original core area from which the Industrial Revolution spread and in both of which Protestant minorities played a disproportionately important role in the process of economic development. In the New World, the United States and Canada had also emerged as developed industrial societies, while virtually all of Latin America remained almost totally unaffected by the Industrial Revolution. Even within Canada, the predominantly Catholic region developed much less rapidly than the rest of the country. Economic development seemed wedded to Protestantism.

But culture is not a constant. It is a system through which a society adapts to its environment. Given a changing environment, in the long run culture is likely to change. One major change that took place was the secularization of Catholic (and other non-Protestant) cultures. In much of the world, the role of the merchant and the profit-making entrepreneur

became less stigmatized. In some settings the entrepreneur even became the cultural hero, as the captain of industry had been in the United States of the late nineteenth century.

A contrasting process of cultural change began to take place in the more advanced industrial societies during the second half of the twentieth century. The lower half of Figure 7 diagrams this process. Precisely in those regions that had earlier been most strongly influenced by the Protestant ethic, the long-term consequences of economic development began to be felt, as generations emerged that had been raised in unprecedented prosperity and economic security and were characterized, increasingly, by the presence of postmaterialist values. The theoretical reasons underlying this process of value change—and a large body of supporting evidence—have been presented in detail elsewhere (Dalton 1977; Dalton, Flanagan, and Beck 1984; Deth 1984; Flanagan 1982; Inglehart 1971, 1977, 1981, 1985; Ike 1973; Kaase and Klingemann 1979; Lafferty and Knutsen 1985; Marsh 1975; Thomassen et al. 1983).

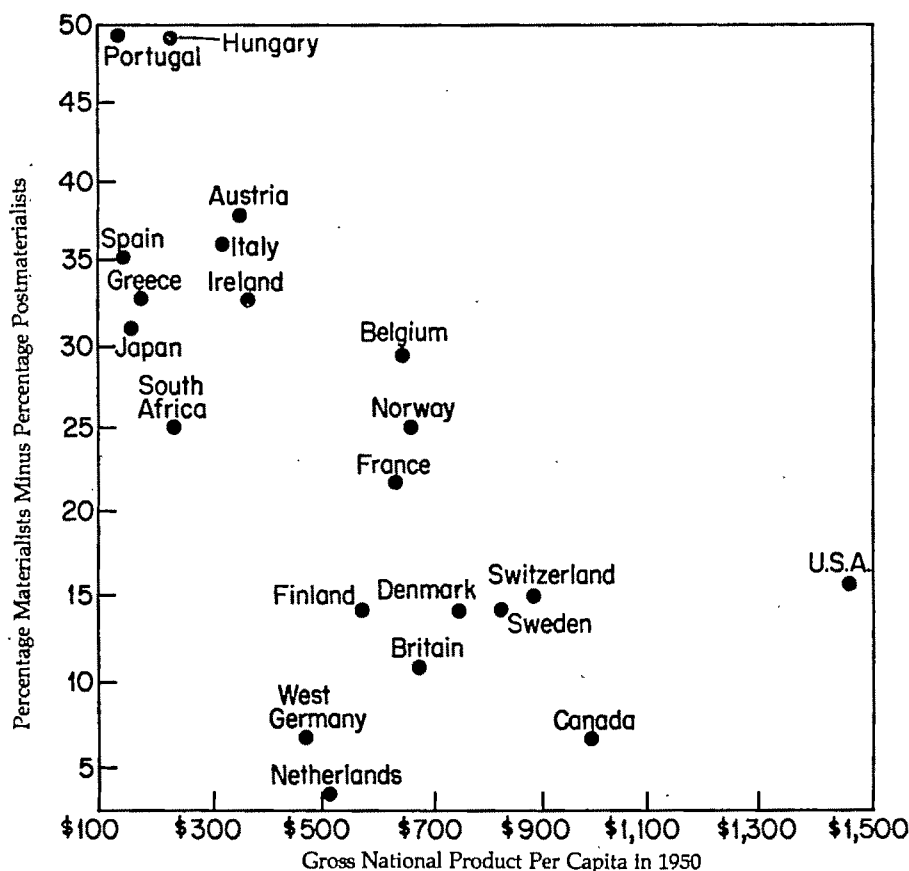
This thesis implies that as a result of the historically unprecedented prosperity and the absence of war that has prevailed in Western countries since 1945, younger birth cohorts place less emphasis on economic and physical security than do older groups, who have experienced a much greater degree of economic insecurity. Conversely, the younger birth cohorts tend to give a higher priority to non-material needs, such as a sense of community and the quality of life. Cohort analysis carried out from 1970 through 1987 in six Western countries confirms the presence of substantial differences in the basic societal priorities of younger and older generations (Inglehart n.d.). Moreover, it demonstrates that as intergenerational population replacement has occurred, there has been a gradual but pervasive shift in the values of these publics

from predominantly materialist priorities toward postmaterialist goals. One consequence of this shift has been a diminishing emphasis on economic growth in these societies together with increasing emphasis on environmental protection and preserving the quality of life—if necessary, even at the expense of economic growth. Postmaterialists place markedly less emphasis on economic growth than do those with materialist or mixed values. And they emphasize a high salary and job security less than working with people they like or doing interesting work (Inglehart 1977). Conversely, postmaterialists place more emphasis on protecting the environment and are far more likely to be active members of environmental organizations than are materialists. Finally, postmaterialists are economic under-achievers; that is, *controlling* for the fact that they come from more prosperous families and receive better education, postmaterialists earn significantly lower incomes than those with materialist values (Inglehart n.d.). All this suggests that as societies become increasingly influenced by the growing postmaterialist minority, they will tend to give economic growth a lower priority. Figures 8 and 9 test this prediction at the societal level.

Evidence from a cross-national perspective converges with evidence from the individual level, pointing to a long-term cultural process of negative feedback linked with economic growth. On one hand, as Figure 8 demonstrates, the publics of relatively rich societies are least likely to emphasize materialist values and most likely to emphasize postmaterialist ones. Since one's values tend to reflect the conditions prevailing during one's pre-adult years, I allow a lag of about 30 years between the independent variable (level of development in 1950) and the dependent variable (mass value priorities in 1981–86). Since the median age in my adult sample is about 45, my economic indicator taps conditions when the median

Political Culture

Figure 8. Economic Development and the Decline of Materialist Values



Sources: Value priorities data from World Values survey, 1981; Euro-Barometer surveys 19-25 (1982-1986); and Political Action study, 1974. Gross national product per capita calculated from *UN Statistical Yearbook, 1958* (New York: UN, 1959).

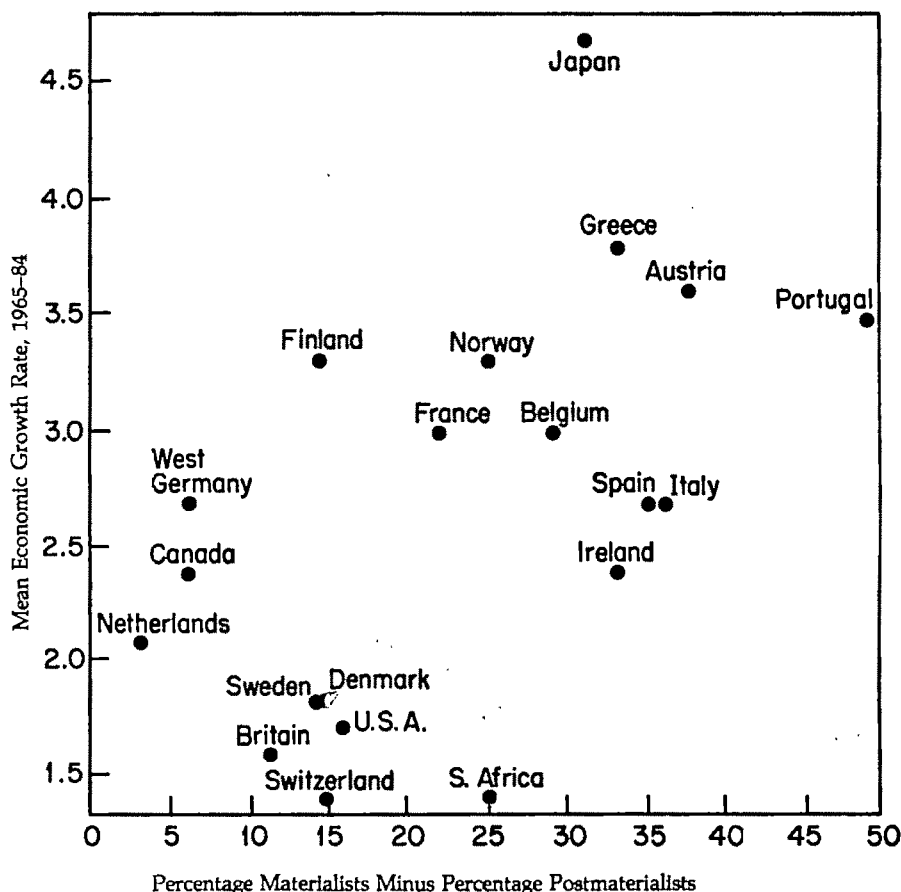
Note: $r = -.63$.

individual was about 15 years of age. I find a correlation of $-.63$ between a society's per capita GNP in 1950 and the proportion of materialists among that society's public in the 1980s. Not only does this result have the predicted polarity and significant strength, but it is stronger than the correlation obtained when I use GNP per capita in 1980 as the independent variable. The time lag assumed to exist between economic cause

and cultural effect seems to reflect reality.

Figure 9 is a mirror image of Figure 8. The wealthier societies are least likely to produce materialist publics, but materialist publics seem to produce high economic growth rates. Or, to reverse labels, though wealthier societies are most likely to produce postmaterialists, after an appropriate time lag the more postmaterialist societies have the lowest growth rates. The long-term result is that high growth rates even-

Figure 9. Materialist Values and Economic Growth, 1965-84



Sources: Growth rates 1965-84 from *World Development Report, 1986* (Washington: World Bank, 1986). Growth rates 1870-1913 from Angus Maddison, *Economic Growth in Japan and the USSR* (London: Allen & Unwin, 1969).

Note: $r = .51$ (correlation between 1870-1913 growth rates and 1981-86 values = $-.38$, i.e., the countries that are most postmaterialist today had relatively high growth rates in 1870-1913).

tually lead to lower growth rates. Prosperity engenders a cultural shift toward postmaterialist values, which eventually leads to a less intense emphasis on economic growth.

Data from 21 societies reveal a consistent cultural-economic syndrome that was originally linked with the Protestant ethic: the wealthier nations and those

with highly developed tertiary sectors are most likely to be long-established democracies, *and* the publics of these societies tend to show a "civic" political culture, have less materialist value priorities, and tend to be Protestant in religion (cf. Huntington 1984). But the Protestant ethic seems to be unraveling: for the linkage between Protestantism and economic

Political Culture

Table 2. Economic Growth Rates in Protestant vs. Catholic Countries and Japan, 1870-1984

Rank	1870-1913	1913-38	1949-65	1965-84
1	U.S.A. (P)	Japan (B)	Japan (B)	Japan (B)
2	Canada (P)	Norway (P)	W. Germany (P)	Norway (P)
3	Denmark (P)	Netherlands (P)	Italy (C)	France (C)
4	Sweden (P)	U.S.A. (P)	France (C)	Belgium (C)
5	Germany (P)	Switzerland (P)	Switzerland (P)	Italy (C)
6	Belgium (C)	Denmark (P)	Netherlands (P)	W. Germany (P)
7	Switzerland (P)	Sweden (P)	Canada (P)	Canada (P)
8	Japan (B)	Italy (C)	Denmark (P)	Netherlands (P)
9	Norway (P)	Canada (P)	Norway (P)	Denmark (P)
10	Gt. Britain (P)	Germany (P)	Sweden (P)	Sweden (P)
11	Netherlands (P)	Gt. Britain (P)	U.S.A. (P)	U.S.A. (P)
12	France (C)	France (C)	Belgium (C)	Gt. Britain (P)
13	Italy (C)	Belgium (C)	Gt. Britain (P)	Switzerland (P)
Mean economic growth rate in Protestant countries ^a	152	120	98	72

Sources: 1870-1965 standings calculated from data in Angus Maddison, *Economic Growth in Japan and the U.S.S.R.* (London: Allen & Unwin, 1969), pp. 148-49; 1965-84 standings from data in *World Development Report, 1986* (Washington: World Bank, 1986).

Note: (P) = countries in which a majority of the population was Protestant in 1900; (C) = countries having a Roman Catholic majority in 1900; (B) = Buddhist majority in 1900.

^aPercentage of rates in Catholic countries.

achievement is a thing of the past. While the Protestant ethic syndrome was strongly correlated with high levels of economic growth in 1870-1913 (the earliest period for which we have data), this correlation weakens and then becomes strongly negative as we move into historically more recent periods.

Among those countries for which we have long-term historical data, those that had relatively high growth rates a century ago tend to have relatively low growth rates today. Table 2 illustrates this phenomenon. In 1870-1913, nearly all Protestant countries had growth rates that were higher than those of almost all Catholic countries. My table actually understates the extent to which this was true, because the few Catholic countries from which we have historical data are precisely the ones that were *most* developed in the nineteenth century. Protestant countries still had more dynamic

economies in the interwar years. But in the past few decades, this situation has reversed itself; during 1965-84, the Catholic countries in our sample had *higher* growth rates than most of the Protestant ones. Within the United States, as recently as 1958, Catholics and Protestants manifested different values concerning various aspects of economic and family life (Lenski 1963). But these differences have been dwindling (Alwin 1986).

In part, this reflects the fact that it is easier for a poor country to attain a high growth rate than for a rich one. By importing technology that has already been proven in more developed countries, one can catch up rapidly. But in global perspectives it is clear that this is only part of the story. For plenty of poor societies are *not* showing rapid economic growth, while others (like those of East Asia) have been growing at an extraordinary pace. Clearly, some societies are more receptive

to economic development than others. Conversely, some rich nations (like Japan) continue to develop relatively rapidly—even when they can no longer rely on imported technology but are increasingly developing their own—while others have become relatively stagnant.

High economic growth was once an almost uniquely Protestant phenomenon; today it has become global in scope and is *less* likely to be found in the Protestant societies than elsewhere. This does not mean that the civic culture that emerged in these societies will disappear. On the contrary, these countries are still becoming richer and on the whole, life satisfaction, political satisfaction, and trust have been gradually rising in recent years. The syndrome that linked Protestantism with wealth and democracy has become less distinctively Protestant because it is permeating other regions of the world.

It is doing so unevenly, however, with its spread being shaped by the cultural traditions of given societies. The Confucian system was virtually unique among traditional cultures in that it institutionalized a socially accepted and even honored channel for upward social mobility, based on nonviolent individual achievement rather than ascription. By passing a series of difficult academic examinations that were open to any promising young male, one could attain power, status, and wealth as a government official. Consequently, in the sixteenth century a social scientist from Mars might have ranked East Asia, with its Confucian tradition, as the region of the world that was culturally most conducive to economic development. Though narrowly circumscribed, social mobility through individual achievement was accepted to a degree virtually unknown elsewhere. Education, rather than armed force, was the principal means to rise in society. And a secular orientation was relatively conducive to technology and worldly achievement.

I suspect that the Confucian cultural

tradition, its traditional rigidity having been shattered by the impact of the West, is an important element underlying the current economic dynamism of certain portions of Asia. During the period from 1965 to 1984, 5 of the 10 fastest-growing nations in the world were countries shaped by the Confucian and Buddhist traditions: Singapore, South Korea, Hong Kong, Taiwan, and Japan. China ranked thirteenth. Moreover, three more of the top 20 countries had significant Chinese minorities that in each case played disproportionately important economic roles: Malaysia, Thailand, and Indonesia. Finally, immigrants of East Asian origin have shown disproportionately high rates of economic achievement throughout Southeast Asia and in the United States, Canada, and Western Europe. It is difficult to avoid the conclusion that the Confucian cultural tradition is conducive to economic achievement today. It would be unrealistic to view these traits as indelible, however. My broader thesis suggests that the intense emphasis on economic achievement now found among peoples shaped by the Confucian tradition could emerge only when the static orientation of traditional society was broken and is likely to gradually erode when future generations have been raised in high levels of economic security. For the present, however, it may be a key factor in the world economy.

Conclusion

Both social analysis and social policy would be much simpler if people from different societies were interchangeable robots. But a large body of evidence indicates that they are not. The peoples of given societies tend to be characterized by reasonably durable cultural attributes that sometimes have major political and economic consequences. If this is true, then effective social policy will be better

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served by learning about these differences and about how they vary cross-culturally and over time than by pretending that they don't exist.

Rational choice models constitute one of the most promising tools now available for political analysis. As currently applied, they are effective in analyzing short-term fluctuations within a given system, taking cultural and institutional factors as constant. But these factors are *not* constant, either cross-nationally or over time. And current models cannot deal with long-term changes in the basic goals and nature of a system. One of the central debates in the field of political economy seems to reflect this fact. When it was found that political support responded to fluctuations in the economy, it was taken for granted that this reflected the workings of economic self-interest among the electorate. Subsequent research has made this interpretation increasingly doubtful. The linkage between economics and politics seems largely shaped by sociotropic concerns. The classic model of economically determined behavior has a strong grip on the minds of social analysts, probably because, throughout most of the history of industrial society, it provided a fairly accurate description of human behavior. In recent decades, the rising role of postmaterialist concerns may have helped make sociotropic concerns increasingly important, particularly among the politically more aware segments of the electorate.

Political economy research has demonstrated convincingly that short-term economic changes have significant political consequences. But the long-term consequences of economic change have barely begun to be analyzed in comparable fashion, though they may be at least equally significant. Evidence presented here indicates that the emergence and viability of mass-based democracy is closely related to economic development *and* that the outcome is contingent on

specific cultural changes. Though mass democracy is almost impossible without a certain amount of economic development, economic development by itself does not produce democracy. Unless specific changes occur in culture and social structure, the result may not be democracy but a variety of alternatives ranging from the Libyan to the Soviet. A large body of cross-national survey evidence indicates that enduring cultural differences exist. Though these differences may be related to the economic level of a given nation, they are relatively independent of short-term economic changes. These cultural factors have an important bearing on the durability of democracy, which seems to result from a complex interplay of economic, cultural, and institutional factors. To neglect any of these components may compromise its survival.

Finally, it appears that economic development itself is influenced by cultural variables. In my brief analysis of this, I have utilized one indicator of materialist and postmaterialist values that is available from only the past two decades and another indicator—the dominant religious or philosophic tradition of a given society—that is a greatly oversimplified indicator of prevailing worldviews at a given time and place but goes back over centuries. Clearly, this analysis cannot be regarded as conclusive. But the available evidence tends to confirm Weber's insight that culture is not just a consequence of economics but can shape the basic nature of economic and political life.

Note

1. These findings, incidentally, bear on a question that was recently considered grounds for denying Samuel Huntington membership in the National Academy of Sciences. His opponents charged that it was absurd to argue that relatively high or low levels of satisfaction can be attributed to given nations; but in a very significant sense, it appears that they *can*.

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CONDORCET'S THEORY OF VOTING

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Condorcet's criterion states that an alternative that defeats every other by a simple majority is the socially optimal choice. Condorcet argued that if the object of voting is to determine the "best" decision for society but voters sometimes make mistakes in their judgments, then the majority alternative (if it exists) is statistically most likely to be the best choice. Strictly speaking, this claim is not true; in some situations Borda's rule gives a sharper estimate of the best alternative. Nevertheless, Condorcet did propose a novel and statistically correct rule for finding the most likely ranking of the alternatives. This procedure, which is sometimes known as "Kemeny's rule," is the unique social welfare function that satisfies a variant of independence of irrelevant alternatives together with several other standard properties.

[Our aim is] to inquire by mere reasoning, what degree of confidence the judgment of assemblies deserves, whether large or small, subject to a high or low plurality, split into several different bodies or gathered into one only, composed of men more or less wise.

—Condorcet

A central problem in democratic theory is to justify the principle of majority rule. Why should the opinion of a majority of citizens or of elected representatives bind the rest of society? One of the earliest and most celebrated answers to this question was attempted by Rousseau in *The Social Contract* (1762). The opinion of the majority is legitimate, said Rousseau, because it expresses the "general will." "When a law is proposed in the people's assembly, what is asked of them is not precisely whether they approve of the proposition or reject it, but whether it is in conformity with the general will . . . each by giving his vote gives his opinion on this question, and the counting of votes yields a declaration of the general will. When, therefore, the opinion contrary to my

own prevails, this proves only that I have made a mistake, and that what I believed to be the general will was not so" (1962, 153).

This ambiguous and slightly disquieting idea was given a more satisfactory expression some twenty years later by the mathematician and social philosopher Marie Jean Antoine Nicolas Caritat, Marquis de Condorcet.¹ In his remarkable work, *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (1785), Condorcet proposed the following argument for decision by majority: Enlightened voters honestly attempt to judge what decision will best serve society. They may occasionally judge wrongly. But assuming that they are more often right than wrong, the majority opinion will very likely be "correct." This is just a property of large numbers and is intuitively obvious when there are only two possible decisions. Condorcet rigorously demonstrated this proposition using the newly developed calculus of probabilities. He then proceeded to show how the idea could be elaborated into a whole series of results about the

reliability of decisions of a voting body, depending on its size, the competence of its members, and the number of alternatives under consideration. These "jury theorems" have recently attracted considerable attention (Grofman and Owen 1986; Grofman, Owen, and Feld 1983; Urken and Traflet 1984).²

It has not been widely recognized, however, that Condorcet showed how the same arguments could be extended to the case of more than two decisions. He was, of course, the first to realize the complications that arise in this case, namely, the possibility of cyclic majorities. But he went on to suggest—or at least came very close to suggesting—a definite rule for ranking any number of alternatives even when cyclic majorities are involved. Admittedly, Condorcet's argument is difficult to follow and full of inconsistencies. Moreover, the rule that he proposed is at variance with the algorithm that he proposed for computing it. Nevertheless, his stated goal is clear: to find the social decision that is most likely to be "correct."

First I summarize Condorcet's proposal and dispose of the claims made by some commentators that Condorcet gave no solution for the general case. I then show that Condorcet's method can be interpreted as a statistical procedure for estimating the ranking of the candidates that is most likely to be correct. If only one candidate is to be selected, however, then an earlier method proposed by Condorcet's rival Borda often gives better results. In the final section I show that Condorcet's decision rule can also be justified in modern social choice terms. It is the unique ranking procedure that satisfies a variant of independence of irrelevant alternatives together with several other standard conditions in social choice theory.

Condorcet's Model

For Condorcet, as for Rousseau, the

object of voting is not merely to balance subjective opinions; rather, it is a collective quest for truth. In any field of candidates (or agenda of alternatives) there is some truly best candidate, a truly second-best candidate, and so forth. What is meant here by truly best? Ultimately it is an undefined term, a notion that is postulated in order to make the theory work—like the ether in classical physics. To be sure, in some situations the notion of "best" decision is quite realistic. An example is trial by jury, where the group decision problem is to determine guilt or innocence. Another is the pooling of expert opinions about an observable event, like the state of the economy or tomorrow's weather.³

For the present let us grant Condorcet's hypothesis that the notion of a "correct" decision also applies to political decision making. Thus, voters evaluate candidates for political office in terms of their ability to lead. Legislators consider bills in terms of their potential contribution to the public welfare. Granted that voters are at least sometimes mindful of the public interest and vote accordingly, Condorcet's argument proceeds along the following lines. People differ in their opinions because they are imperfect judges of which decision really is best. If on balance each voter is more often right than wrong, however, then the majority view is *very likely* to identify the decision that is objectively best. In other words, the majority opinion gives the best estimate, in a statistical sense, of which candidate is most likely to be best. One of Condorcet's major contributions was to show how this idea can be generalized to the case of many candidates.

Consider a list of candidates (or decisions) that are voted upon pairwise. Assume that each voter, when faced with a comparison of the form, "Is *a* better than *b* or *b* better than *a*?" chooses correctly with some fixed probability greater than 1/2. Each voter judges every

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pair of candidates and the results are tallied in a voting matrix. Table 1 shows a voting matrix with 13 voters and three candidates. Each entry in the matrix gives the total number of votes obtained by the row candidate over the column candidate. For example, the comparison a versus b yields eight votes in favor of a , five in favor of b . The comparison b versus c yields eleven votes for b and two for c . The comparison a versus c yields six for a and seven for c . Notice that this vote entails the cyclical majority $a > b$, $b > c$, $c > a$.

Given a series of pairwise votes as in Table 1, the problem posed by Condorcet is to find the ranking of all the candidates that is *most likely* to be correct, that is, the ranking that would have produced the observed votes with highest probability. At first blush it might appear that this problem is insoluble unless the competency levels of the voters are known. This difficulty can be circumvented if one makes the simplifying assumption (which Condorcet did) that the voters all have the same competence. In this case the answer turns out to be independent of the level of competence, as I shall show in the next section. Here we shall content ourselves with Condorcet's statement of the solution.

First some terminology is required. In Condorcet's lexicon, an "opinion" is a series of pairwise comparisons on the alternatives. Each pairwise comparison is called a "proposition" and written $a > b$, etc. An opinion is said to be "impossible," "contradictory," or "absurd" if some of the propositions composing it form a cycle, such as $a > b$, $b > c$, $c > a$. Normally, each individual voter is able to rank all of the candidates in a consistent order. This is not necessarily true of the aggregate opinion, however, as Table 1 demonstrates.⁴ To break such cyclic majorities, Condorcet proposed the following method:⁵

Table 1. Voting Matrix with Three Alternatives and 11 Voters

	a	b	c
a	—	8	6
b	5	—	11
c	7	2	—

1. All possible opinions that do not imply a contradiction reduce to an indication of the order of merit that one judges to exist among the candidates . . . therefore for n candidates one would have $n(n-1) \dots 2$ possible opinions.

2. Each voter having thus given his or her opinion by indicating the candidates' order of worth, if one compares them two by two, one will have in each opinion $n(n-1)/2$ propositions to consider separately. Taking the number of times that each is contained in the opinion of one of the q voters, one will have the number of voices who are for each proposition.

3. One forms an opinion from those $n(n-1)/2$ propositions that agree with the most voices. If this opinion is among the $n(n-1) \dots 2$ possible opinions, one regards as elected the subject to whom this opinion accords the preference. If this opinion is among the $2^{n(n-1)/2} - n(n-1) \dots 2$ impossible opinions, then one successively deletes from that impossible opinion the propositions that have the least plurality, and one adopts the opinion from those that remain. (1785, 125-26)

Let us apply this rule to the situation in Table 1. First we are to choose the three propositions having a majority, namely $b > c$ with eleven votes, $a > b$ with eight votes, and $c > a$ with seven votes. Since these three propositions form a cycle, however, we delete the proposition with smallest plurality, namely $c > a$. This leaves the combination $b > c$ and $a > b$ (and implicitly $a > c$), which implies the ranking $a > b > c$.

The rule is clear enough in the case of three alternatives. However, in the general case some ambiguities arise. The difficulty lies in the phrase "successively deletes" in step 3. Taken literally, it means the following: If there are cycles in the propositions selected in step 2, delete first the proposition having the least number of votes in its favor. (This proposition

Table 2. A Voting Matrix with 25 Voters and Four Alternatives

	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>
<i>a</i>	—	12	15	17
<i>b</i>	13	—	16	11
<i>c</i>	10	9	—	18
<i>d</i>	8	14	7	—

might or might not be contained in a cycle.) If any cycles remain, delete next the proposition with the next lowest number of votes in its favor, and so forth. Continue until all cycles are eliminated.

There are several reasons why this is probably not what Condorcet meant to say. In the first place, the procedure may yield an ambiguous ordering of the candidates. Consider Table 2, with twenty-five voters and four candidates.

In step 2 of Condorcet's algorithm one would select the six propositions having greatest majorities. In descending size of majority, these are $c > d$, $a > d$, $b > c$, $a > c$, $d > b$, $b > a$. According to a literal reading of step 3, one would first delete the proposition $b > a$, as it has the smallest majority in its favor. But this does not result in an "opinion" because one cycle still remains: $b > c$, $c > d$, $d > b$. Therefore one would delete the proposition $d > b$, as it has the next-smallest majority in its favor. All cycles are now eliminated. But there is a difficulty: in the resulting partial order both a and b are undominated. Either one of them could be interpreted as the top-ranked candidate, so the outcome is indeterminate.

It seems more likely that Condorcet meant to *reverse*, rather than simply *delete*, the weakest propositions. According to this interpretation one would first reverse the proposition with the least majority (thus $b > a$ becomes $a > b$), then reverse the proposition with the next-least majority (thus $d > b$ becomes $b > d$) and so forth until no cycles remain.

In the above example the resulting opinion is $abcd$. Unfortunately, this answer does not square with the rest of Condorcet's argument. His stated intention is to choose "the most probable combination of opinions," that is, the set of propositions that contains no cycles and is supported by the largest number of pairwise votes. The total number of votes supporting the ranking $abcd$ is 89. (That is, the sum of the votes for a over b , a over c , a over d , b over c , b over d , and c over d equals 89.) But this is not a maximum. The ranking $bacd$ is supported by a total of 90 pairwise votes. This ranking is obtained by reversing exactly one majority proposition, $d > b$. Note that this is not the proposition with smallest majority.

At this point we might be tempted to throw up our hands and declare, as previous commentators have, that "the general rules for the case of any number of candidates as given by Condorcet are stated so briefly as to be hardly intelligible . . . and as no examples are given it is quite hopeless to find out what Condorcet meant" (E. J. Nanson as quoted in Black 1958, 175). Even the influential mathematician Isaac Todhunter found Condorcet's argument completely exasperating: "The obscurity and self-contradiction are without any parallel, so far as our experience of mathematical works extends . . . no amount of examples can convey an adequate impression of the evils" (Todhunter 1949, 352).

In fact, Condorcet's overall intention is quite clear. It is to find *the most probable combination of opinions*. In describing his method he seems to have used the term "successively deletes" rather loosely as a heuristic for computing the solution.⁶ (He may also have believed, mistakenly, that successive deletion always does give the most probable combination.) But there appears to be little doubt about his objective. Hence I propose to amend Condorcet's statement of the rule as follows:

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3'. One forms an opinion from those $n(n-1)/2$ propositions that agree with the most voices. If this opinion is among the $n(n-1) \dots 2$ possible opinions, one regards as elected the subject to whom this opinion accords the preference. If this opinion is among the $2^{n(n-1)/2} - n(n-1) \dots 2$ impossible opinions, then one *reverses* in that impossible opinion the set of propositions that have the least combined plurality and one adopts the opinion from those that remain.

It seems reasonably likely that this is what Condorcet meant to say. In any case, it is the only interpretation that is consistent with his goal of finding the ranking that is most likely to be correct.

Condorcet's Method As a Statistical Hypothesis Test

From a historical standpoint, Condorcet's theory is particularly interesting because it represents one of the earliest applications of what today would be called "statistical hypothesis testing." Based on a sample of observations, one wishes to infer which of several underlying but unobservable states of nature is most likely to be true. This approach is known as *maximum likelihood estimation*. All that is required is a model of how the observed quantities depend probabilistically on the unobservable state of nature. In Condorcet's scheme of things a state of nature corresponds to the true or correct ordering of the candidates. This datum is essentially unobservable. What can be observed are the votes of the representatives who are attempting to read the true state of nature.

Condorcet assumed, first, that in any pairwise comparison each voter will choose the better candidate with some fixed probability p , where $1/2 < p < 1$ and p is the same for all voters.⁷ Second, every voter's judgment on every pair of candidates is independent of his or her judgment on every other pair. Finally, each voter's judgment is independent of the other voters' judgments (a significant assumption).

Consider again the voting matrix in Table 1. Suppose that the true ranking is abc — a first, b second, c third. A vote for a over b will occur with probability $1 - p$, whereas a vote for b over a will occur with the lesser probability $1 - p$. The combined probability of observing the 39 pairwise votes in Table 1 is therefore

$$L(abc) = \frac{13!}{8!5!} [p^8(1-p)^8] \frac{13!}{6!7!} [p^6(1-p)^7] \frac{13!}{11!2!} [p^{11}(1-p)^2] \\ = \frac{(13!)^3}{8!5!6!7!11!2!} [p^{25}(1-p)^{14}].$$

Similarly, the probability of observing the results in Table 1 if the true ranking is acb would be

$$L(abc) = \frac{13!}{8!5!} [p^8(1-p)^8] \frac{13!}{6!7!} [p^6(1-p)^7] \frac{13!}{11!2!} [p^2(1-p)^{11}] \\ = \frac{(13!)^3}{8!5!6!7!11!2!} [p^{16}(1-p)^{23}].$$

The coefficient is the same for all six rankings, so their relative likelihoods are as in Table 3.

For each ranking the exponent of p is the sum of all pairwise votes that agree with the ranking, and the exponent of $(1-p)$ is the sum of all pairwise votes that disagree with it. Assuming that $p > 1/2$,

Table 3. Likelihoods of Six Rankings Based on Table 1 Data

Ranking	Likelihood
abc	$p^{25}(1-p)^{14}$
acb	$p^{16}(1-p)^{23}$
bac	$p^{22}(1-p)^{17}$
bca	$p^{23}(1-p)^{16}$
cab	$p^{17}(1-p)^{22}$
cba	$p^{14}(1-p)^{25}$

which seems to be a credibly optimistic view of human judgment, it follows that the most likely ranking is the one with the highest exponent of p . In the above example the answer is abc .

In general, suppose that there are n voters and m candidates or alternatives a_1, \dots, a_m . Let n_{ij} be the total number of votes that a_i receives over a_j . Assume that every voter registers an opinion on every pair of candidates, so that $n_{ij} + n_{ji} = n$ for all $i \neq j$. Let R be a complete ranking of the candidates, where $a_i R a_j$ means that a_i is preferred to a_j . The argument outlined above is easily generalized to show that a ranking R has maximum likelihood if and only if it maximizes $\sum n_{ij}$, the sum over all pairs ij such that $a_i R a_j$. That is, the maximum likelihood ranking is the one that agrees with the maximum number of pairwise votes, summed over all pairs of candidates. This is equivalent to the statement of Condorcet's rule given in 3'. Note that it does not depend on the value of the success rate p as long as all voters have the same success rate, which is larger than one half. This rule is very natural and, not surprisingly, has been rediscovered by other writers. In particular it was proposed in a different guise by John Kemeny (1959; see also Kemeny and Snell 1960) and is sometimes known in the literature as "Kemeny's rule" (Young and Levenglick 1978).

Condorcet versus Borda

Another pioneer in the theory of elections was the Chevalier Jean Charles de Borda. Like Condorcet, Borda was a member of the French Academy of Sciences and an important figure in official scientific circles. Unlike Condorcet, he was an applied scientist with a practical bent. He was noted for his work in hydraulics, mechanics, optics, and especially for the design of advanced navigational instruments. He also captained maritime vessels. Because of his ex-

pertise in measurement, he served on the committee for determining the standard weights and measures in the new metric system (Mascart 1919).

In 1770, some 15 years prior to the publication of Condorcet's treatise on elections, Borda read a paper to the academy on the design of voting procedures (see Borda 1784). Exactly what prompted Borda to sail in these uncharted waters is unclear. Possibly he was concerned with reforming the election procedures in the academy itself. He began by pointing out the defects with the customary "first-past-the-post" system in which the candidate with the largest plurality is elected. When there are many candidates, this procedure can easily elect someone who is endorsed by only a small minority of the electorate. Instead, Borda suggested, the election procedure should be based on each voter's entire rank ordering of the candidates. The correct candidate to choose is the one that on average stands highest in the voters' lists.

Operationally, the method works as follows: Each voter submits a list in which all of the candidates are rank-ordered. In each list a candidate who is ranked last receives a score of zero, a candidate who is ranked next-to-last receives a score of one, a candidate who is ranked next above that receives a score of two, and so forth. Thus the score of a candidate in any particular list is equal to the number of candidates ranked lower on that list. The scores of the candidates are summed over all voter lists and the candidates with highest total score is elected.

If Borda's objective was practical reform, he appears to have been successful, for something like his method was subsequently adopted for the election of new members to the academy.⁸

Condorcet was certainly aware of Borda's work. Indeed he introduced the method in the *Essai* with the following remark: "As the celebrated Geometer to whom one owes this method has pub-

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lished nothing on this subject, I have taken the liberty of citing it here" (1785, clxxix).⁹ Condorcet repeatedly refers to Borda, not by name, but as "the celebrated Geometer." The tone is ironical. From his private correspondence it is clear that Condorcet held Borda's work in low esteem and considered him a personal enemy within the academy. For example, in a letter to Turgot (1775), he gave this assessment: "[M. Malesherbes] makes a great case for Borda, not because of his memoirs, some of which suggest talent (although nothing will ever come of them, and no one has ever spoken of them or ever will) but because he is what one calls a good academician, that is to say because he speaks in meetings of the Academy and asks for nothing better than to waste his time doing prospectuses, examining machines, etc., and above all because, feeling eclipsed by other geometers, he, like d'Arcy, has abandoned geometry for petty physics" (quoted in Henry 1883). The contempt with which Condorcet viewed Borda's scientific contributions may help to clarify an abrupt shift in Condorcet's argument. As we have already seen, Condorcet's initial objective was to design a method for estimating the "true" ranking of the candidates. But he also recognized that the problem is often one of determining which single candidate is most likely to be best (1785, lxi-lxv, 122-23). At first glance the solution would appear to be obvious: first rank all of the candidates and then choose the one that is top-ranked. But this is not necessarily the most convincing answer.

Consider the following example of Condorcet (1785, lxiii) with 60 voters and three candidates. Here *c* has a simple majority over both *a* and *b* and *b* has a simple majority over *a*. So there are no cycles, and the ranking by Condorcet's rule is *cba*. That is, *cba* is the ranking most likely to be correct. But is *c* the candidate that is most likely to be the best (i.e., top-ranked in the true ranking)?

Table 4. A Voting Matrix with 60 Voters and Three Candidates

	<i>a</i>	<i>b</i>	<i>c</i>
<i>a</i>	—	23	29
<i>b</i>	37	—	29
<i>c</i>	31	31	—

Not necessarily. Candidate *c* will be best if two propositions hold: namely, if *c* is better than *a* ($c > a$) and *c* is better than *b* ($c > b$). We are therefore interested in the joint probability of these two statements being true. If $c > a$, then the probability of observing the voting pattern in Table 4 (31 votes for *c* versus 29 for *a*) is $(60!/31!29!)p^{31}(1-p)^{29}$. If the contrary holds ($a > c$), then the probability of observing 31 votes for *c* versus 29 for *a* is $(60!/31!29!)p^{29}(1-p)^{31}$. Assume that $a > c$ and $c > a$ are a priori equally likely. Then the probability of observing the vote 31 for *c*, 29 for *a* is

$$\Pr(\text{vote}) = (60!/31!29!) (p^{31}(1-p)^{29} + p^{29}(1-p)^{31})/2.$$

By Bayes' rule¹⁰ it follows that the posterior probability of $c > a$, given the observed vote, is

$$\Pr(c > a | \text{vote}) = \frac{\Pr(\text{vote} | c > a) \Pr(c > a)}{\Pr(\text{vote})},$$

that is,

$$\Pr(c > a | \text{vote}) = \frac{p^{31}(1-p)^{29}}{p^{31}(1-p)^{29} + p^{29}(1-p)^{31}}.$$

Similarly,

$$\Pr(c > b | \text{vote}) = \frac{p^{31}(1-p)^{29}}{p^{31}(1-p)^{29} + p^{29}(1-p)^{31}}.$$

Therefore the posterior probability that c is best is

$$\begin{aligned} \Pr(c|\text{vote}) = \\ \frac{\Pr(c > a|\text{vote})\Pr(c > b|\text{vote})}{[p^{31}(1-p)^{29}]^2} \\ \frac{[p^{31}(1-p)^{29} + p^{29}(1-p)^{31}]^2}{[p^{31}(1-p)^{29} + p^{29}(1-p)^{31}]^2} \end{aligned}$$

The posterior probability that b is best is

$$\begin{aligned} \Pr(b|\text{vote}) = \\ \frac{p^{37}(1-p)^{23}p^{29}(1-p)^{31}}{[p^{37}(1-p)^{23} + p^{23}(1-p)^{37}] \times [p^{29}(1-p)^{31} + p^{31}(1-p)^{29}]} \end{aligned}$$

And the posterior probability that a is best is

$$\begin{aligned} \Pr(a|\text{vote}) = \\ \frac{p^{23}(1-p)^{37}p^{29}(1-p)^{31}}{[p^{23}(1-p)^{37} + p^{37}(1-p)^{23}] \times [p^{29}(1-p)^{31} + p^{31}(1-p)^{29}]} \end{aligned}$$

Which of a , b , or c has the highest posterior probability? The answer depends on the assumed value of the competence level p . If p is close to one, then the controlling factor is the size of the exponent of $1-p$. The most probable candidate will be the one whose *weakest* comparison is as strong as possible.¹¹ In this case the answer would be c , because the lowest number of votes that c receives in any pairwise contest is 31, while a 's weakest showing is 23 and b 's is 29.

Suppose instead that p is close to $1/2$, that is, $p = 1/2 + \epsilon$ for some small $\epsilon > 0$. In this case the relative probabilities depend essentially only on the numerators of the above three expressions. Substituting in $p = 1/2 + \epsilon$ we find that the relative probabilities of a , b , and c can be approximated as follows:

$$\begin{aligned} a: (1/2 + \epsilon)^{23}(1/2 - \epsilon)^{37}(1/2 + \epsilon)^{29}(1/2 - \epsilon)^{31} \\ b: (1/2 + \epsilon)^{37}(1/2 - \epsilon)^{23}(1/2 + \epsilon)^{29}(1/2 - \epsilon)^{31} \\ c: (1/2 + \epsilon)^{31}(1/2 - \epsilon)^{29}(1/2 + \epsilon)^{31}(1/2 - \epsilon)^{29} \end{aligned}$$

If we expand these expressions using the binomial formula and drop all powers of ϵ higher than the first, then the relative probabilities of the three candidates are

approximately as follows:

$$\begin{aligned} a: (1/2)^{120} + (1/2)^{119}(23 - 37 + 29 - 31)\epsilon \\ b: (1/2)^{120} + (1/2)^{119}(37 - 23 + 29 - 31)\epsilon \\ c: (1/2)^{120} + (1/2)^{119}(31 - 29 + 31 - 29)\epsilon \end{aligned}$$

In each case the coefficient of ϵ is the sum of all pairwise votes for the candidate minus the sum of all pairwise votes against the candidate. Since the total number of votes (for and against) is the same for all candidates, the coefficient of ϵ will be maximized for that candidate that receives the most pairwise votes over all others. In this case the answer is b .

This argument is quite general. For any number of voters and any number of alternatives, if p is sufficiently close to $1/2$, then the candidate with the highest probability of being best is the one that receives the most pairwise votes. This is precisely the Borda candidate. The reason is that the Borda score of a candidate in each individual voter's list is equal to the number of candidates ranked below it, that is, the number that it beats. So a candidate's total Borda score is its row sum in the voting matrix.¹²

Borda's method is therefore one solution to Condorcet's problem. In particular, a candidate that beats every other by simple majority does *not* necessarily have the strongest claim to being the best candidate; the Borda winner may be stronger.¹³ Condorcet apparently realized this implication of his argument. Understandably, he would be loath to draw much attention to it, given his feelings toward Borda. In addition, Condorcet may have become disillusioned with his approach because of its failure to provide a simple and definitive solution independently of the competence level p .

For whatever reason, Condorcet abruptly changed course at this point in the *Essai* (1785, lxiii-lxv). He abandoned the statistical framework that he had so painstakingly built up and fell back on a more "straightforward" line of reasoning. In doing so he opened up a whole new

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approach that has had enormous influence on the modern theory of social choice. To illustrate Condorcet's new-found argument, consider the example in Table 4. If a candidate, such as c , obtains a simple majority over every other candidate then, Condorcet argued, that candidate is the only reasonable choice. Indeed, candidate a has no reasonable claim to being best, therefore a should be set aside. So the contest is between b and c . But c defeats b , so the choice must come down on the side of c .

More generally, Condorcet proposed that whenever a candidate obtains a simple majority over every other candidate, then that candidate is presumptively the "best." This decision rule is now known as "Condorcet's criterion," and such a candidate (if it exists) is a "Condorcet winner" or a "majority candidate" (Fishburn 1973).

In advancing this new approach, Condorcet was not abandoning his basic premise that the object of voting is to make the "correct" choice. He merely shifted his ground and argued for adopting a simple estimate of the best choice rather than a complex one. Furthermore, this "new" approach is entirely consistent with Condorcet's earlier solution of the ranking problem. Indeed, if a majority candidate exists, then it must be ranked first in a Condorcet ranking. The reason is simple. Suppose that x is a Condorcet candidate but that this candidate is not ranked on top in the Condorcet ranking. Then it must be ranked just below some other candidate y . By assumption, x defeats y by a simple majority. Therefore, if we switch only the order of x and y in the ranking, then we obtain a new ranking that is supported by more pairwise votes. But this contradicts the definition of the Condorcet ranking as the one supported by the *maximum* number of pairwise votes. Thus Condorcet's choice rule is consistent with his ranking rule whenever a majority candidate exists.

While Condorcet's solution is attractive, Borda's rule still has advantages that cannot be dismissed easily. First, it is considerably simpler to compute than Condorcet's ranking rule. Second, if one accepts Condorcet's basic premises, then the Borda winner is in fact a better estimate of the best candidate provided that p is close to $1/2$. Third, if p is *not* close to $1/2$, then it is still *very likely* that the Borda winner is the best candidate, even though strictly speaking it may not be the optimum estimate of the best candidate.¹⁴ If there are a large number of voters and p is not very close to $1/2$, then the probability is very high that the truly best candidate will be selected by *any* reasonable choice rule (i.e., with high probability it will be the majority winner and the Borda winner at the same time). The critical case is precisely when p is near $1/2$, and then Borda's rule is definitely optimal. Thus we must either jettison the whole idea of selecting the "best" candidate with high probability or admit that Borda's rule is a good way of estimating which candidate that is.

On the other hand, when the problem is to rank a set of alternatives, Condorcet's rule is undoubtedly better than Borda's. Furthermore, even if one does not accept the specific probability model on which this conclusion is based, Condorcet's rule has an important stability property that Borda's rule lacks, as I shall now show.

Local Stability and Nonmanipulability

One of the most important concepts in the theory of social choice is independence: a social decision procedure should depend only on the voters' preferences for the "relevant" alternatives, not on their preferences for alternatives that are infeasible or outside the domain of discourse. Independence has been given several distinct formulations (Arrow 1963;

Table 5. A Voting Matrix on Six Alternatives and 100 Voters

	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>	Borda Scores
<i>a</i>	—	51	54	58	60	62	285
<i>b</i>	49	—	68	56	51	58	283
<i>c</i>	46	32	—	70	66	75	289
<i>d</i>	42	44	30	—	41	64	221
<i>e</i>	40	48	34	59	—	34	215
<i>f</i>	38	42	25	36	66	—	207

Nash 1950). The one that I shall adopt here is closest in spirit to Nash's version: if the set of alternatives shrinks, then the reduced set of alternatives should be ranked in the same way that they were within the larger ranking. There are at least two reasons why independence is desirable. First, it says that decisions cannot be manipulated by introducing extraneous alternatives. Second, it allows sensible decisions to be made without requiring an evaluation of all possible decisions. In practice, society cannot consider all possible choices in a situation. It cannot, or at least usually does not, even consider all of the best choices. In choosing a candidate for public office, for example, it is impractical to consider all eligible citizens. In considering a piece of legislation, it is impossible to hold votes on all possible amendments. Independence assures that a decision made from a limited agenda of alternatives is valid *relative to* that agenda. Enlarging the agenda may introduce better possibilities, but it should not cause us to revise the relative ranking of the old possibilities.

Unfortunately, it is impossible to design any reasonable social decision rule with this property. I claim, however, that independence can be satisfied if we restrict ourselves to agendas that are sufficiently "connected." By a connected agenda I mean, roughly speaking, a subset of closely related alternatives rather than an arbitrary collection of unrelated or extreme

ones. Consider the following example: A legislature of 100 members is considering three alternative decisions *a*, *b*, and *c* (see Table 5). Other alternatives might in theory be considered, such as *d*, *e*, and *f*, but they are weaker than *a*, *b*, and *c* in the sense that any one of them would be defeated by any one of *a*, *b*, or *c*.

The question is whether the exclusion of the nominally "inferior" alternatives *d*, *e*, *f* affect the choice between *a*, *b*, and *c*. Put differently, can an inferior alternative be introduced strategically in order to manipulate the choice between the "superior" alternatives? This is surely a situation that we would like to avoid. Unfortunately Borda's rule suffers from this defect. To see this, observe that the total Borda score of each alternative is simply the sum of the row entries corresponding to that alternative (e.g., the right-hand column of Table 5). These scores imply the ranking *cabdef*. Now consider just the three alternatives *a*, *b*, and *c*. The corresponding voting matrix is the one enclosed by dashed lines. The Borda scores for this three-alternative situation are 117 for *b*, 105 for *a*, and 78 for *c*. Hence when just the top three alternatives are considered, they would be ordered *bac*, which is the *reverse* of how the electorate would have ordered them in the context of all alternatives. Introducing the inferior alternatives changes the outcome from *b* to *c*.

I claim that Condorcet's rule is immune

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to this type of instability. To compute the Condorcet solution observe first that *a* has a simple majority over every other alternative, hence (as was demonstrated in the previous section) Condorcet's rule must rank *a* first. Since each of the alternatives *a*, *b*, and *c* defeats each of the alternatives *d*, *e*, and *f* by a majority, it also follows that the former three alternatives must be ranked above the latter three. For if this were not the case, then some alternative would be ranked immediately below another alternative that it defeats, which is impossible in a Condorcet ranking. To maximize agreement with the pairwise voting data, *a* must be ranked first, *b* second, and *c* third. Positions four, five, and six in the ranking will be occupied by *d*, *e*, and *f* in some order. To determine which order, it suffices to apply Condorcet's criterion to these three alternatives alone; that is, they should be ordered so as to agree with the maximum number of pairwise votes that they receive *among themselves*. The reader may verify that the ordering that accomplishes this objective is *dfe*. Hence the total ordering implied by Condorcet's rule is *abcdfe*. This is quite different from the Borda ordering.

I have worked out the details of this example to show that Condorcet's rule is not necessarily difficult to compute even when a sizable number of alternatives is involved (though it is certainly more difficult than Borda's method). It also illustrates why Condorcet's method satisfies a certain form of independence. For example, if the bottom three alternatives are dropped from consideration, then Condorcet's rule (unlike Borda's) must preserve the ordering *abc*. Indeed, if it did not, then the interval *abc* could be rearranged into a new ordering that is supported by more pairwise votes. But then *abc* could also be rearranged *within the full ordering abcdfe* to obtain a new ordering that is supported by more pairwise votes. This contradicts the assump-

tion that *abcdfe* is supported by the *maximum* number of pairwise votes. A similar argument applies if the last two alternatives are dropped, the last four, or indeed any bottom segment of the ranking. Similarly, the ordering of the bottom part of the list will be preserved if we remove the top alternative, the top two alternatives, or any top segment of the ranking.

In general, a Condorcet ranking has the property that the ordering of the alternatives does not change whenever we restrict attention to an *interval* of the full ordering.¹⁵ This property will be called *local stability*. Intervals tend to consist of alternatives that are relatively closely related. For example, an interval might consist of a set of candidates that occupy a certain segment of the political spectrum (e.g., "centrist"). Or it might consist of a series of minor modifications or amendments to a proposed piece of legislation.

I shall show that Condorcet's method is the only plausible ranking procedure that is locally stable. To do so we must recall several properties that are standard in the social choice literature. Let *F* be a social *ranking rule*, that is, a rule for strictly ordering any set of alternatives given the preference data of the voters. We shall assume that the input data to *F* is given in the form of a voting matrix: the number of votes for and against each pair of alternatives. All voters are weighted equally because only the *number* of votes counts, not the identities of the individuals who cast the votes. This property is known in the literature as *anonymity*. *F* is *neutral* if it is not "biased" with respect to one or another alternative: if the names of the alternatives are permuted, then their ranking is similarly permuted. Taken together, anonymity and neutrality require that *F* sometimes yields ties.¹⁶ A third standard condition in the social choice literature is that *F* be *unanimous*: if all voters cast their votes the same way on every pair, then the ordering of the candidates agrees with all of the votes.

Finally we shall require a degree of consistency in the way that F aggregates individual opinions into group opinions. Consider a bicameral system such as the U.S. Congress. To pass, a decision must receive a majority of both chambers. This much is clear when a decision involves just two choices. Now consider a decision involving more than two alternatives. Suppose that both chambers use the same decision rule F , and that they consider the same agenda. Let each chamber separately rank all of the alternatives. If both chambers happen to rank them in exactly the same way, then we may say that this ranking is *approved* by both chambers. If a ranking is approved by both chambers, then it stands to reason that it would also be approved if the two chambers were to be considered as a single body (i.e., to cast their votes in plenary session). F *satisfies reinforcement* if whenever a ranking is approved by two separate groups of voters, then it would also be approved when the votes of the two groups are pooled.¹⁷

The condition of "local stability" can now be formally stated as follows: Let A be an agenda of alternatives, let V be a voting matrix on A , and let $F(V) = R$ be the (unique) consensus ranking. F is *locally stable* if for every interval B of the ranking R , $F(V_B) = R_B$ where V_B and R_B represent V and R restricted to the subset B . In case of ties, the condition says that for every $R \in F(V)$, every interval B of R , $R_B \in F(V_B)$, and if $R'_B \in F(V_B)$ for some $R'_B \neq R_B$, then R'_B can be substituted for R_B in R to obtain a new ranking $R' \in F(V)$.

It follows from a theorem of myself and Arthur Levenglick (1978) that Condorcet's rule is the unique ranking rule that satisfies anonymity, neutrality, unanimity, reinforcement, and local stability.¹⁸ Note that Borda's rule satisfies all of these properties *except* local stability.

Conclusion

The purpose of this article has been to draw attention to a major, but little-

known, contribution by Condorcet to the theory of group decision making. Condorcet proposed a decision rule that applies to any voting situation whether cyclic majorities are present or not. This rule can be interpreted as a best guess about the "true" ordering of the alternatives. The rule can also be justified on entirely different grounds; namely, it is unbiased with respect to both individuals and alternatives and it aggregates individual opinions in a "consistent" way. This axiomatic justification differs significantly from the probabilistic one in that it places emphasis on the operational properties of the method rather than on a model about how the world works. Of course one may still question whether these are the "right" properties, just as one may question whether Condorcet had the "right" probabilistic model. But at least the nature of the choice is clarified. Furthermore, one must admit that the specific probabilistic model by which Condorcet reached his conclusions is almost certainly *not* correct in its details. The plausibility of his solution must therefore be subjected to other tests, as I have attempted to do.

I conclude that there is a strong case for Condorcet's rule on two counts. First, in situations where voters are primarily concerned with the accuracy of a decision (as in jury trials, expert opinion pooling, and in some types of public referenda), Condorcet's method gives the ranking of the outcomes that is most likely to be correct. Second, where voters are motivated primarily by their subjective preferences about outcomes, Condorcet's rule is responsive to changes in individual preferences, is unbiased with respect to voters and alternatives, and (unlike Borda's rule) it is relatively stable against manipulation by the introduction of unrealistic or inferior alternatives. In short, we need not resolve the question whether voters attempt to judge which decision is in the public interest or whether they merely register their subjective opinions on an issue. Both of these elements are probably present in most decisions. In either case,

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Condorcet's method is a rational way of aggregating individual choices into a collective preference ordering.

Notes

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1. Condorcet did not explicitly say that he was addressing Rousseau's problem, but Rousseau's ideas would have been much discussed in Condorcet's circles. Baker (1975, 229) also suggests that Condorcet drew his inspiration from Rousseau. Grofman and Feld (1988) explore the connections between Condorcet's and Rousseau's ideas in greater depth.

2. Poisson (1837) studied a similar set of questions using probabilistic methods. See also Gelfand and Solomon (1973) for recent work in the Poisson tradition.

3. A related problem is to rank teams or players based on their win-loss records (as in baseball, tennis, or chess). Condorcet's model can be used to estimate the most likely true ordering of the players (see Batchelder and Bershad 1979; Good 1955; Jech 1983).

4. The following preference data would yield the pairwise votes in Table 1: six voters have preference *abc*, five voters have preference *bca*, and two voters have preference *cab*. It bears emphasizing, however, that Condorcet did not think of the voters' opinions as expressing their personal preferences. Rather, they represent the voters' best judgments as to which candidate in each pair is the better one.

5. In this translation I have modified the mathematical notation slightly to conform with modern conventions.

6. Michaud (1985) comes to a similar conclusion.

7. I exclude the case $p = 1$, because this would imply that all voters agree with probability one. The case where the voters have different a priori probabilities of being right is treated in Young 1986 (see also Nitzan and Paroush [1982] and Shapley and Grofman [1984]).

8. For reasons that remain obscure, Borda's method was jettisoned by Napoleon after his own election to membership (Mascart 1919).

9. In a footnote, Condorcet adds that his own treatise "had been printed in its entirety before I had any acquaintance with this method [of Borda], apart from the fact that I had heard several people speak of it" (1785, clxxx).

10. Condorcet arrived at this answer intuitively, not by an explicit application of Bayes' rule.

11. This "minimax" rule was proposed in modern times by Kramer (1977).

12. Borda himself gave this alternative statement of his method.

13. This idea was conjectured independently by Grofman (1981). The distinction between the majority alternative and the alternative most likely to be best has also been discussed by Baker (1975) and Urken and Traflet (1984).

14. The following argument was suggested by Bernard Grofman.

15. This property of Condorcet's method (which is known in the literature as the "median procedure") was noted by Jacquet-Lagrèze (1969). For other results on the median procedure see Michaud and Marcotorchino (1978), Barthelemy and Monjardet (1981), Michaud (1985), and Barthelemy and McMorris (1986).

16. For example, if there are an even number of voters and every pairwise vote is a tie, then anonymity and neutrality imply that all linear orderings of the alternatives are equally valid. Thus F must sometimes take on multiple values.

17. We require furthermore that if another ranking is approved by only one of the two groups, then it is *not* tied with a ranking that is approved by both of them.

18. Let F satisfy the five conditions of the theorem. When there are exactly two alternatives, it follows that F is simple majority rule (Young 1974). Now consider the case of more than two alternatives. Let R be the social order. Local stability implies that whenever two alternatives are ranked one immediately above another in R , then their ordering remains invariant when F is restricted to those two alternatives alone. Since F is simple majority rule on every two alternatives, it follows that in the social order R every alternative must defeat (or tie) the alternative ranked immediately below it. Furthermore, if the two alternatives are tied, then they may be switched to obtain a social ordering tied with R . It can be shown by induction on the number of alternatives that this property, together with neutrality, unanimity, and reinforcement implies that F is Condorcet's ranking rule (Young and Levenglick 1978).

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Once sparse and sporadic, histories of political science have proliferated in recent years. We contend that such histories are a necessary feature of the discourse of political science, because there are essential connections between the history, identity, and actual practices of any rationally progressive discipline. In light of the fact that the objects political scientists study are historically and contextually contingent, there has been—and should be—a plurality of histories to match the diversity of approaches in political science. Unfortunately, most histories of political science prove either “Whiggish” and condescending toward the past, or “skeptical” and negative. The consequence has been an inadequate understanding of the relationship between plurality, rationality, and progress in the discipline. Taking into account both the deficiencies and achievements of Whiggish and skeptical accounts, we argue that context-sensitive histories would better serve the rationality and progress of political science.

Who controls the past controls the future.
Who controls the present controls the past.

—George Orwell, 1984

The occasion of this essay is a recent plethora of works attending to the history of political science. Recent additions to what was once a sporadic and discontinuous genre include books by Blondel (1981), Collini, Winch, and Burrow (1983), Higgott (1983), Kavanagh (1983), Natchez (1985), Janos (1986), Ricci (1984), and Seidelman and Harpham (1985) and shorter pieces by Riker (1982), Keohane (1983), and Gunnell (1983).¹ On reading these works one is struck by the variety of conclusions drawn regarding the discipline's past and how it shapes the present state and future promise of the discipline. Our intent is to ask what, if anything, disciplinary histor-

ies can contribute to the identity, practice, and progress of political science.

Our account is designed to enable political scientists to parse disciplinary histories for positive and negative lessons. The foundations of this account, however, require that we say something about the relationship between disciplinary history and identity, for two related reasons. In the first place, it may not be obvious—especially in light of the variety of conclusions they have drawn—that disciplinary histories can provide practitioners with any guidance for their research. We shall argue that they can and indeed that such history is an ineliminable feature of any account of the discipline's identity. This connection arises not just in terms of an intimate relationship between the history and philosophy of science—two forms of commentary on the practice of science. We shall argue that in political

science more than elsewhere there is an essential link between disciplinary history and the actual practice of inquiry. And if this first thesis can be sustained, standards of judgment for adjudicating competing histories must also be articulated.

We follow the lead of "postempiricist" philosophy of science to argue for the centrality of disciplinary history. This claim is both prescriptive and descriptive, for the writing of disciplinary history is not limited to the texts we cited at the outset but rather pervades the discourse of political scientists. The purpose of this section is thus to establish the importance of disciplinary history and its place in the actual practices of political scientists.

The utility of such histories in understanding the identity of political science is, however, a different question. One of the difficulties in establishing standards of judgment for adjudicating disciplinary histories is that our histories have a character different from those in the natural sciences. Whereas the natural sciences generally enjoy hegemonic histories to which their practitioners subscribe, political science has multiple histories. This difference turns out to follow from the socially constituted and historically determined nature of the objects of political inquiry. Thus an adequate understanding of the relationship between history and identity in political science requires an appreciation of the historically and contextually contingent character of disciplinary practices.

In the final section we flesh out the implications of the preceding arguments by pinning down the contributions disciplinary history has made—and could make—to the progress of political science. This exploration is carried out by examining actual histories of the discipline. We shall argue that for all their virtues, these works have often distorted the history and identity of the discipline. Virtually all of them are either "Whiggish" and overly condescending toward the past, or "skeptical" and excessively dismissive. We

build on our criticisms to argue that political science has a past whose achievements can be vindicated and a promising future—but only if political scientists are willing and able to write disciplinary histories that do full justice to past contexts.

Disciplinary History and Disciplinary Identity

We began this essay by noting that there have been a number of self-consciously historical studies of the discipline published in the last few years. Several of the authors' aims are clear enough: they intend to provide not only a description of the past but also a set of prescriptions for the discipline's future (the one exception being Collini, Winch, and Burrow 1983). The past rarity of such sustained treatments should not obscure the fact that political scientists have frequently deployed historical reconstructions in arguments about the discipline. To test our contention the reader need only consult any collection of "scope and methods" texts. Most of them—and we found few exceptions—include some discussion of the history of political science, deployed to warrant a particular methodological stance. Or consider the review essays that periodically grace the professional journals, APSA presidential addresses on the state of the discipline, the texts introducing students to a subfield, and the numerous collections of essays on the state of subfields. All of these use historical reconstruction to warrant some particular perspective.

The same has always been true of political philosophy. So, for example, Aristotle felt the need to wrestle with Plato (e.g., *Nicomachean Ethics* 1095a32), and Machiavelli with those who "imagined republics and principalities which have never been seen or known in reality" (Machiavelli 1950, 56). Even

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Hobbes—that most antihistorical of thinkers—felt he had to engage “the philosophy schools, through all the universities of Christendom, grounded upon the texts of Aristotle, [which] teach another doctrine” (Hobbes 1962, 22).

Our point in noting these uses of historical reconstruction is that disciplinary history in political science, as in other fields, is generally used to legitimate a particular perspective while delegitimizing competing approaches (see Lepenies and Weingart 1983, xv–xx). Such a conclusion does not sit well with some historians. Most notably, Collini, Winch, and Burrow (1983) bemoan the insidious effects of such uses of disciplinary history. Their own work stands out from the other histories now available for a number of reasons. First, the authors are not political scientists, but rather self-styled intellectual historians. Second, their concern is with a somewhat distant episode, “the aspiration to develop a ‘science of politics’ in nineteenth-century Britain” (p. 3). But more significant for our purposes is that the authors proclaim an intellectual “aversion to discipline history” of the sort that would provide “nourishment or some other form of comfort” for present-day practitioners (pp. 4, 7) and so attempt to sever the connection between disciplinary history and identity. In fact, they deny that theirs is a *discipline* history at all (pp. 4–5).

As they note, legitimating histories are often informed by “the present theoretical consensus of the discipline, or possibly some polemical version of what that consensus should be” and as such “reconstitute” the past “as a teleology leading up to and fully manifested” in this consensus. Consequently, “the intellectual map of the present, or some version of it” is superimposed on the past, thereby obliterating the interests, concepts, categories, and self-understandings of past figures (pp. 4–5).

There is more than a kernel of truth to this criticism of what the authors call

Whiggish history (p. 5). Others have also noted the ubiquity of such histories in the social sciences (see Bernstein 1976, 97–98; Lepenies and Weingart 1983, xiii).

It is noteworthy that the attempt of Collini, Winch, and Burrow to sever disciplinary history from current disputes about the identity of political science is belied by their own work. While insisting that they “remain agnostic on . . . fundamental and ultimately epistemological problems” (p. 7), they do in fact pass judgment regarding such questions on their subjects (Dugald Stewart, Malthus, Bagehot, Sidgwick, etc.; see, e.g., p. 376).

Now it might be objected that in fact these judgments are merely contingent features of their work: remove them and one is left with a history having no partisan intent. The difference between the approach advocated by Collini, Winch, and Burrow and the approach of the political scientist interested in disciplinary history would then be that the former wish merely to explicate the self-understandings of past figures while the latter must care about the adequacy of those self-understandings.

This distinction, however, collapses on closer scrutiny. “Intellectual history,” Rorty, Schneewind, and Skinner (1984, 9) argue, “cannot be written by people who are illiterate in the culture of their prospective readers. . . . To put present day readers in touch with a past figure is precisely to be able to say such things as ‘This was later to be known as . . .’ and ‘Since the distinction between X and Y was yet to be drawn, A’s use of “Z” cannot be interpreted as . . .’ But knowing when to say such things—knowing what to bracket when—requires knowing what has been going on in all sorts of areas.”

In other words, the very possibility of writing about something that would count as an episode in a discipline presupposes some understanding of its identity. Indeed, one need only read the introductory chapter of their book to recognize the

extent to which Collini, Winch, and Burrow actively "bracket" their subject for, or make it intelligible to, contemporary practitioners. This task requires some understanding of what these practitioners actually do (and should) mean by political science.

Our criticism of Collini, Winch, and Burrow is not meant to disparage the substantive content of their historical reconstruction but simply to stress that there can be no nonlegitimizing or neutral stance from which a disciplinary history can be written. All such histories will be selective, and guided by some commitment (or opposition) to a particular identity.

But if disciplinary history must always be written from some such perspective, might it not follow that we really have no need to tarry with the problem of which histories to accept as adequate? Here, it might be argued that to write the history of the discipline is one thing, to actually do political science quite another. On this objection, at least, intellectual historians such as Collini, Winch, and Burrow and the political *scientist* might well agree—at least to the extent that both would like to sever the connection between history and identity.

And indeed, for their part, political scientists have often treated the discipline's past as if it were a history of "prescience" or "ideologies" or "philosophy," with the present (or imminent future) bearing witness to the emergence of a real science. It is not only twentieth-century practitioners who have tendered this line. Since Hobbes (at least) it has been widely argued that social and political science is a possibility if not a reality. At the root of these arguments one finds a common belief that science proceeds by following a particular method. Hobbes' method was one of thinking of "men as if but even now sprung out of the earth, and suddenly, like mushrooms, come to full maturity

without all kinds of engagement to each other" (quoted in Lukes 1968, 119). Most modern practitioners place their faith in the methodology of the natural sciences.

If these methodological aspirations could be realized, they would clearly vindicate those political scientists who read the history of the discipline in terms of a modern break from a prescientific past. On this account, the standards against which the materials generated by the historian are to be judged would be provided by the scientist. Moreover, the identity of the discipline would be independent of its past, a methodological not a historical matter. The history (or better, prehistory) of political science would have interest only as a source of examples of attempts to articulate a scientifically grounded knowledge of politics.

Unfortunately for its advocates, this position is now discredited among philosophers of science. The undermining of this "method fetishism" (Putnam 1981, 188) had been in the offing for some time (see Manicas 1987, 241–44) before the death blow dealt to it by Kuhn (1970). Kuhn's work served as an important catalyst in the development of post-empiricist philosophy of science (Hesse 1980, 167–86; Bernstein 1983, 20–25). The virtually unanimous conclusion of the postempiricists is that the rationality and progress of science do not depend on conformity to the "logic of scientific inquiry," whether positivist, logical empiricist, or Popperian. As Kuhn (1970, 200) put it, "There is no neutral algorithm for theory choice, no systematic decision procedure" for determining the "scientific" status of a knowledge claim. Or as MacIntyre (1977, 468) argues, "There is no set of rules as to how science must proceed and all attempts to discover such a set founder in their encounter with the actual history of science."

MacIntyre's comment returns us to the history-identity connection. Against the apparent relativism implicit in a Kuhnian

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perspective, more recent postempiricist philosophy of science has tended to emphasize that the rational status of scientific theories is secured in their historical development (Burian 1977, 30). To evaluate a theory, or set of theories, or even a methodological and metatheoretical stance is to write a particular sort of history in which the stance in question is seen as rationally superior to its competitors, past and present. Following Lakatos (1970), stances "are to be assessed by the extent to which they satisfy historiographical criteria; the best scientific methodology is that which can supply the best rational reconstruction of the history of science and for different episodes different methodologies may well be successful" (MacIntyre 1977, 469).

Lakatos' claim that "philosophy of science without history of science is empty; history of science without philosophy of science is blind" drives home the point (1978, 102). Not only is disciplinary history going to be articulated from a particular account of identity, but any identity must be grounded in a disciplinary history. Disciplinary history and prescriptions for identity are properly understood as but two moments in the same reflective process.

It follows from these claims that "how we judge the status of a science depends on how we judge the quality of the history it assists in providing" (MacIntyre 1984, 44). This turns us toward the question of the kind of history that can underwrite the rationality and progress of political science. An answer can be approached by examining the differences between the history of political science and that of other rational disciplines—especially the natural sciences.

Hegemony and Plurality in Disciplinary History

All disciplines go through occasional upheavals during which competing orien-

tations (metatheories, paradigms, research programs, or research traditions) vie for supremacy. The proponents of any new view must, of course, argue that their program is superior. But if the postempiricists are right, this superiority cannot be one of *method*.² Rather, rational superiority must be established through the writing of a disciplinary history in which it can be shown that one program provides an account that is able to explain both the successes and failures of its extant competitors (MacIntyre 1984, 43).

This superiority, however, must be established not only over the current received wisdom but also with respect to all orientations that have previously held prominence in the discipline (MacIntyre 1984, 44–45). In other words, the Young Turks, or at least some of their number, must do disciplinary history of two related sorts. In a narrow and fore-shortened sense, they need to demonstrate (following Lakatos) that their extant competitor has been running out of steam with time in terms of its ability to solve problems or predict novel facts. Such arguments, however, also presuppose a longer narrative in which all past approaches (i.e., prior to the current competitor) are safely dead and buried.

On most accounts, the contemporary natural sciences exemplify this kind of progressive history. Their practitioners may safely assume that their history consists of the effective supercession of less- by more-fruitful research traditions. Thus they need not consider resurrecting anachronistic research traditions to further their science. For example, in evolutionary biology, today's punctuated equilibrium theorists quite reasonably feel little need to trouble themselves with pre-Darwinian understandings. As Kuhn (1969, 407) argues, "Science destroys its past."

It can, then, be argued that the contemporary natural sciences enjoy a hegemonic (albeit multifaceted and complex) history and identity. And disciplinary

history can figure in natural scientific progress to the extent of its ability to identify the rational warrants of paradigm shifts. This kind of contribution can also apply in political science, but there is an important difference to be taken into account. For in political science we lack general agreement on who counts as an important figure and what counts as a progressive development. Instead, we allow numerous competing accounts of the intellectual movements, research programs, traditions, and central figures that have shaped (or on some accounts misshaped) the discipline. We have a variety of histories to match our plurality of identities.

This contrast with natural science might be taken as grounds for arguing that political science is "immature." But such a claim ignores the particular features that give rise to our disciplinary plurality. Unlike natural science, political science *cannot* "destroy its past." "Global" progress, at least in the commonly understood terms of linear succession of increasingly successful research traditions, cannot occur in political science. For the empirical problems and conceptual disputes that are the grist for comparative judgments across research traditions are themselves historically specific and socially constituted. Thus any computation of a standings table of different traditions is contingent upon time, place, and a particular set of empirical and conceptual problems. To take a crude example: behaviorist approaches seem to work well in the apparently placid fifties but are severely strained by the turbulent sixties. Resurrection of apparently obsolete research traditions is also possible if socially constituted empirical problems shift in a direction that gives them renewed problem-solving power. So Marxism returns along with protest and conflict in the industrial world, and an obvious lack

of development in much of the third world.

In this light, the progress of political science consists of an expanding capacity to cope with contingency in the content of empirical problems (Dryzek 1986, 315-17). Thus political science is always *rationally* going to be home to a variety of research orientations. Any approach trying to demonstrate its utility or superiority must do so not just through reference to the extant range of traditions (and the history of each) but also in relation to past traditions. Every approach needs its historians, to write (or rewrite) at least some of the discipline's past.

However, such histories will be limited in their scope, for, as we have already intimated, the "objects" of political science—unlike those of the natural sciences—are constituted by the beliefs and self-understandings of social agents, among whom may of course be numbered political scientists themselves.³ The very existence of these objects—say, a bureaucracy, army, monetary system, political party, monarchy, capitalist economy, socialist state, or democracy—is contingent on the subscription by social agents to some particular beliefs or theories. There is no analog to this in the natural sciences, where the objects—however they may be conceptualized—are not constituted by theories. They exist quite independently of whatever the scientist may *say* about them (Hacking 1984, 115, 116). Moreover, in political science, as agents' "theories" change, the objects will be altered, come into, or go out of existence. Add to this the post-empiricist claim that theories are "underdetermined" by evidence (Hesse 1980, 32, 144, 187), and you have all that is needed for a radical plurality of research traditions, hence of disciplinary histories and identities.

What you also have, however, is the groundwork for the special centrality of

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disciplinary history in political science. In this regard, political science resembles philosophy more than it does the natural sciences. For to develop any creative alternative to dominant current understandings—arguably the essence of philosophy—the philosopher has to engage in “genetic” spadework of the sort that would explain why some orthodoxy came to be victorious and how and why it is vulnerable (Taylor 1984, 19). In so doing, the philosopher can warrant the superiority of his or her favored alternative not just through reference to the extant range of traditions (and the history of each) but also in relation to past traditions.

This need to do an in-depth history of a discipline like philosophy or political science is most obvious in those cases where some orthodoxy is being challenged (Taylor 1984, 21–22).⁴ Young Turks, in other words, are less privileged than their orthodox opponents (whose successful battles with other traditions are often safely behind them) and must work harder at their history.

Sometimes the Young Turks may even have to pin down the identity of the orthodoxy they are challenging. The hard work of reconstruction of a rival is exemplified by Seidelman and Harpham (1985), whose history confirms the demise of what they call “third tradition” U.S. political science. This tradition begins with Ward, ends with Lowi, and along the way is host to Woodrow Wilson, Bentley, Beard, Merriam, Lasswell, Key, Truman, and Burnham. According to Seidelman and Harpham, the third tradition is defined by its use of the tools of science in democratic reform of a sick but not incurable U.S. polity. The leading figures in this third tradition were doubtless unaware of their common membership, and lacked even a name until Seidelman and Harpham came along. In what sense, then, do we have a real tradition here? The answer is surely that a coherent tradi-

tion of such reform science makes most sense in terms of a competitor to the more radically anticapitalist agenda for political science to which Seidelman and Harpham seem to subscribe. Here one can readily see the legitimating function of disciplinary history, for the very idea that there is an extended and coherent third tradition of reformism is of interest primarily to those who seek an alternative to it. On Seidelman and Harpham’s own account, those who practiced and extended the tradition did so without benefit of the kind of history these two authors have now written. So the most logical approval for Seidelman and Harpham would come from leftists like themselves, or possibly antireformist conservatives.⁵

An equally dramatic and ambitious retelling of a hundred or more years of U.S. political science is offered by Ricci (1984). To Ricci this history is a tragedy in which a desire for scientific respectability has muffled the contributions the discipline could have made to the great conversation of democratic development. Thus Ricci reconstructs the last century of U.S. political science in terms of an enterprise that is avowedly and self-consciously scientific from the outset. Again, the legitimating function of disciplinary history is apparent. Ricci’s narrative of tragedy makes sense only in contrast to his own agenda for the discipline, which involves more in the way of a focus on the key concerns of democratic politics, speculative thinking, and grand theorizing. However, Ricci’s reconstruction would probably make no sense to behaviorist disciplinary historians. For example, to Easton (1953, 4–7) the main problem with U.S. political science in the first half of the twentieth century was its *repudiation* of scientific reason. Where Ricci sees only a lamentable obsession with science, behaviorists see only an equally lamentable absence of science.

The need to proclaim research agendas through reference to intellectual history

applies not just at the grand disciplinary level of Seidelman and Harpham and Ricci but also in the subfields of political science. Thus Natchez (1985) writes a history of opinion research to demonstrate that nobody (partially excepting V. O. Key) has studied voting behavior from the angle of the liberal constitutionalist political theory that Natchez believes should be the wellspring of this research. And Keohane (1983) justifies his self-consciously Lakatosian "modified structural realist program" for international relations through references to the achievements and failures of realists from Thucydides to Morgenthau and beyond.

However important disciplinary history is for Young Turks, the defenders of orthodoxes have to write it as well. One reason for this is that given the plurality and disunity of the discipline, "orthodoxy" in political science is a rather tenuous and fleeting condition. So even a widely accepted understanding of disciplinary identity needs it historians in order to demonstrate its own historical coherence—that it really is a paradigm, research program, or research tradition worthy of the name. Equally important, every approach must fend off challengers by demonstrating the expansion of its explanatory or problem-solving capabilities over time. Thus Riker's (1982) interpretation of history of Duverger's Law is designed to bolster the rational choice research tradition, not to mention a positivist understanding of our history. To this end, Riker relates not just the refinement of theory through its confrontation with (what he takes to be) the eternal verities of political life and party systems but also the impact upon theory of events like the advent of a single party in India's plural voting system. Riker's interpretation proceeds with the avowed intent of demonstrating that what he calls political science but what we may call the rational choice research tradition does in fact have a history that is both coherent and progressive

in terms of explanatory power.

While we could multiply examples here, our general point should by now be clear enough. As long as there is variety in orientations toward political science—and this condition is to be fully expected as long as there is history and politics—then there will be variety in disciplinary histories. However, it does not follow that one can write a disciplinary history in any way one chooses, nor does it mean that all disciplinary identities are created equal. We now turn to the question of how one might separate the wheat from the chaff in both histories and identities.

The Limits and Lessons of Disciplinary History

Having established the importance of disciplinary history for our identity, we can appreciate the efforts of those who have engaged the difficult task of addressing this history and applaud their contributions to the rational pluralism of the discipline; for if our arguments are right, then it is only by judging the quality of these histories that practitioners can determine the merits of the research programs they presuppose. But this also means that any appreciation and applause must be tempered by a critical interrogation of these histories. We shall now attempt to advance some critical standards for disciplinary historians and practitioners alike.

Our critique begins by noting that disciplinary histories are necessarily linked to agendas for disciplinary identity, as our earlier discussion of Collini, Winch, and Burrow sought to demonstrate. This link is at once a strength and a weakness. It is a strength because it provides both a reason and a vantage point for historical reconstruction. Without this connection, a history could hardly be a history at all; devoid of an intelligible point of reference, it would itself be rendered unintelli-

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gible. Readers of such an imaginary history would lack the conceptual resources needed to make sense of the story at hand.

But the link is also a weakness, for any history so informed is going to be partial and limited in some potentially debilitating ways, especially when one considers the extreme variety in political science approaches—and hence in disciplinary histories. No Archimedean point is possible here. Disciplinary historians, no less than other social agents, are limited by the contexts from which their own understandings arise. They cannot possibly encompass the whole range of contemporary—let alone past and future—perspectives.

Variety in political science is a mixed blessing. While we have argued that it is both rational and necessary, unsympathetic observers of the discipline might charge relativism and a lack of global progress. To make matters worse, disciplinary history as currently practiced might well lead one to conclude that political science is without past achievement. Most of the extant histories are at best condescending toward past practitioners. Often they are outright condemnations. This is particularly true in those cases where it is modern political science that is in question (although still true in the longer history of political thought). If these histories are taken to heart, our past would hardly seem to constitute proud, happy, and progressive disciplinary development.

Our own more sympathetic view is that properly understood, such histories can indeed provide reasons for the adoption of one orientation or the rejection of others and so the stuff of a progressive discipline. But as yet that promise remains unfulfilled, for reasons we shall now explain.

Virtually all of these histories fall into two mutually exclusive categories. On the one hand we have those historians who are just as Collini, Winch, and Burrow

described them: *Whigs*.⁶ On the other there are those best described as *skeptics*.⁷

Whigs write the history of the discipline as though it were teleological, culminating in some correct current understanding or, more usually, some bright imminent future in which past difficulties are resolved. For them the past has little intrinsic value. Indeed, their inspiration stems from the ideal of liberating the discipline from the dead weight of past errors and illusions.

Our Whigs are legion. To begin over 50 years ago, Merriam (1925), with a little foreshortening, classified the history of political science into four increasingly powerful stages. The first was a priori and deductive, ending in 1850; the second was historical and comparative, dominating the second half of the nineteenth century; the third involved observation and measurement, entering with the twentieth century; and the fourth, just beginning in 1925, was to stress psychological analysis. Only in this fourth, highest, stage would true science arrive. Merriam repudiated his immediate predecessors such as Beard and Bentley for their want of science.

Later, the behavioral revolutionaries would see only intimations of real science amid the supposedly dominant hyperfactualism and institutionalism of their forbears. But few among the revolutionaries believed they were starting with a blank slate. Selected precursors like Bentley and Merriam could be congratulated. Easton (1953) pays homage to a long line of thinkers from Aristotle to Bentham and beyond, even as he believed the momentum toward a science of politics was interrupted in the twentieth century.

Later still, the Kuhnian moment in APSA presidential addresses announced the arrival of the first paradigm (e.g., Almond 1966). More recently, the contemporary "science of political science" has been celebrated by Weisberg, who (even while admitting he has no idea what *science* means) asserts that "political

science has become more scientific over the years" (1986, 3, 10). Blondel (1981) and Kavanagh (1983) are equally pleased with the momentum of post-1945 political science in comparison to its primitive past. Within public administration, Miller and Moe (1986, 167) announce the arrival of the "positive theory of hierarchies" to rescue the subfield from its "lack of theoretical progress."

Whigs generally restrict their approval to a few precursors. For example, Riker (1982) takes pains to identify the individuals who, over a span of more than a century, have anticipated, stated, or refined Duverger's Law. Yet Riker's approval of these people is for the most part lukewarm, for it is only in the last three decades that the law has been taken under the wing of rational choice theory, and thereby "examined with increasing scientific sophistication" (Riker 1982, 765). For him, the past merits congratulation only for its intimations of a glorious present. Riker does, of course, recognize that alternative disciplinary identities have been advanced by those outside the rational choice tradition. But beyond cursory denigration of "belles lettres," Riker is aware of no serious rivals. That is the privilege of those who subscribe to orthodoxies.

Some Whigs are more charitable still in ascribing to their predecessors the possession of paradigms—albeit partial and flawed ones. If the Kuhnian terminology is taken at face value, these precursors thereby have scientific rather than pre-scientific standing. Thus Janos (1986) portrays a switch of paradigms in the study of political development from the "classical" approach of Smith, Comte, Marx, and Weber to contemporary cultural theorists of postindustrial society. Whether charitable or not, however, Whiggish histories treat the disciplinary present and future in terms of triumph over the limited perspectives of past practitioners.

In contrast to our Whigs, skeptical historians find little to commend in the present and still less to approve of in the modern history of the discipline. Skeptics write the history of political science in terms of unremitting error. So Ricci (1984) laments the results of the persistent efforts of the discipline to be a science just like all the others. Gunnell (1983) condemns contemporary political theorists for retreating from real political concerns and intelligible language into abstruse metatheory and incomprehensible idioms. Natchez (1985) laments the all-pervasive atheoretical bent of voting studies, from beginning to end. From an earlier era, Crick (1959) is none too pleased with the Americanized scientism of politics (see also Storing 1962). Remember, too, Bentley's denunciation of the "soul stuff" of his predecessors and contemporaries; though, unlike Crick, Bentley wanted to turn toward, rather than away from, science. Seidelman and Harpham (1985) are somewhat more sympathetic than these other skeptics. Though in their eyes the third tradition ultimately fails, the very reason they need to take it on lies in its victory within the discipline over the first (institutionalist) and second (radically democratic) traditions.

The skeptics are the Whigs' obverse; they conclude that the redemption of political science requires that it abandon its recent past entirely. Those we have noted hold to a common view that what should succeed the jettison of our past is a turn to practical political concerns with a view to becoming relevant to society and its problems. To Seidelman and Harpham this turn would involve a radical agenda. To Natchez this same move would be conservative: he believes that liberal constitutionalist political theory should guide voting studies precisely because it is the dominant political tradition in the United States. For Ricci this turn is to be accomplished by moving beyond the apolitical nature of contemporary political science

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and theory. Bentley attacked "soul stuff" for the sake of a science committed to democratic reform. One can imagine skeptical histories not committed to political relevance, but we can locate none.

Between these two stances there would appear to be no others. Nor should we be surprised by this: Whiggism and skepticism are simply historiographical expressions of the contemporary rifts in the discipline of which most of us are—sometimes painfully—aware. But must we consort with either the Whigs or the skeptics? We think we need not consort with either but to see why we need to be clear about the errors—and strengths—of both stances.

As we have suggested, Whiggishness is defined by its attempted justification of a particular disciplinary present (or near future). This present (or imminent future) may in its turn be defined by present approaches, which are presumably proving themselves by their ability to cope with empirical and conceptual problems as defined and weighted by both present practitioners and the larger society in which they move. However, once we recognize that these present problems are themselves historically contingent and socially constructed, the Whigs' accounts of the past become suspect. For Whigs fail to allow that past practitioners could not have even conceptualized our problems, for they had their own problems to address. As we noted above, research orientations wax and wane under the influence of changes in socially determined and historically specific problems. So the "superior" understanding of the present is itself historically contingent—and likely to meet condemnation from future Whigs for its failure to contribute to the resolution of *their* problems.

Consider, in this light, Dahl's (1961, 279, 280–81) claim (as criticized by Ball 1983, 36–37) that modern opinion research has falsified Aristotle's assertion

that humans are political animals. Aristotle, of course, could not have even conceptualized the idea of most people failing to take advantage of minimal and constrained opportunities to participate in a governmental system that would not even merit the name of *politics* in his terms. Quite simply, humans are indeed *zōon politikon* in the terms of Aristotle's own milieu, however much they have become "apolitical" in most modern societies.

All this is not to say that the Whigs are without achievement. For they do engage in what we have argued is the essential task of reconstructing research traditions, thus providing historical warrants for the triumph of their favored approaches. Yet the Whig's historical ledger is distorted. The positive side of that ledger represents similarities between past problem contexts and present ones, making particular past practitioners look like precursors of the present and present insights useful for understanding past circumstances. But as the present changes, so will its precursors, and Whigs will have to continue rewriting history. Thus Bentley comes to prominence as a precursor of pluralist political science in the 1950s—and fades along with pluralist interpretations in the 1960s and 1970s.

In all of this, then, the self-confidence of Whiggish political science turns out to be unjustified. Today's Whigs do not necessarily possess a better understanding of politics than past practitioners. Instead, they may just have a better understanding of a particular kind of politics. Political change may render older conceptions anachronistic, but the claim that we are getting closer to some timeless truth as a result is unwarranted. The Whigs are bad historians because they miss the contextual dimension of inquiry. Their sweeping claims about political science will no doubt look as silly to future Whigs as past Whigs look to them.

The skeptics' errors are in a sense the obverse of the Whigs. Where Whigs see

nothing but brightening skies the skeptics see nothing but ever darkening ones. Their conclusion that the history of modern political science provides little worth salvaging would be justified if two arguments could be carried. The first is the argument that over the period of interest some exclusive approach has dominated the discipline as a whole, or at least some well-defined subfield of it. The second, contingent upon the first, is the argument that this orthodoxy has encountered a crisis of sufficient magnitude finally to extinguish its claims upon the discipline. So, for example, Seidelman and Harpham (1985) argue that the third tradition has dominated U.S. political science. This tradition expires in the hands of Burnham and Lowi, for—besides advocating particular reforms rooted in their science—these authors use their science to explain why such reforms can never make any headway in the U.S. polity (Seidelman and Harpham 1985, 221).

The skeptics' first argument is overstated: there is no hegemony, and there never has been. However dominant institutionalism or behavioralism or the Michigan model or realism may have been in their time, they have never been exclusive. Disciplinary pluralism is the norm, and the existence of skepticism itself accentuates that pluralism.

The second argument typically carries with it the charge that political science has always been—or has become—sundered from the hard and interesting questions of real politics. But the skeptics themselves and the occasional figures for whom they express some sympathy (as, for example, Natchez [1985, 183–210] pays homage to V. O. Key) have clearly revealed much about real political issues. More important, our skeptical historians show that even those they attack have made important contributions.

In this light, consider Seidelman and Harpham's (1985) portrayal of certain

"third traditionists." According to them, Lowi has explained why the reality of interest group liberalism is so resistant to reform (p. 201). Lasswell did indeed uncover many of the irrationalities of mass and elite politics (pp. 134–37). Bentley's "muckraking" science did indeed expose many of the illusions of U.S. politics held by the public and reinforced by political scientists (pp. 69–81).

Consider, too, some of Ricci's (1984) villains. Even as he disparages the "Popperist" foundations of midcentury political science, Ricci admits that Popper's grounding of liberal democracy in a model of scientific inquiry does indeed provide a rationalistic defense of democracy against its totalitarian rivals exactly when such a defense is needed—in the 1940s (pp. 119–25). And Ricci recognizes that the behavioralist voting studies he disparages did tell us some interesting things about politics—if only that Western electoral democracy cannot flourish unless both elites and masses are ignorant of the voting studies findings (p. 175). Natchez (1985) inadvertently demonstrates that the voting studies have all along been corroborating liberal constitutionalist claims about the limited capabilities of the ordinary person, even as the opinion researchers themselves, in their theoretical ignorance, were unaware of this fact. And Gunnell (1983) admits that political theorists have not been completely isolated from the political realities of their time, even if they have not become engaged in the ways Gunnell would prefer.

Like the Whigs, our skeptics fail to do justice to success in context. So Lasswell's accounts of deranged political man are successful in a deranged decade. Popper and Dewey are successful in the context of the midcentury global political struggle. The muckraking science of Beard and Bentley makes sense in the context of a political agenda largely defined by the Progressive political movement. The am-

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bitions for behavioralism of Key and Truman are understandable in a context dominated by a seemingly well-functioning democracy. The Almond and Verba (1963) account of the determinants of stable democracy are successful in a problem context defined by a smug and self-satisfied U.S. political system. And skepticism about the history of political science makes sense in the context of a polity and a discipline that have lost their bearings.

In sum, professionalization has not meant irrelevance to hard political questions. It is professional political scientists who have identified iron triangles, exposed the deficiencies of liberalism, and provided rationalistic comfort for (imperfect) Western democracy in the face of totalitarian challenges.

We can commend the skeptics' recognition of the limits and deficiencies of Whiggish political science. As Gunnell (1983, 38) argues, digging into the discipline's past "demonstrates the inevitability of mortality and the demise of the present." But this does not justify skepticism about the discipline, for the skeptics themselves have shown (just by their very existence) that the discipline is not without redeeming character.

None of this should lead political scientists to scoff at the histories Whigs and skeptics have produced. For it is only by critically examining these—and future—disciplinary histories that we will be able to do better history. Such histories should, above all else, attend to episodes of political science in context. Practitioners, approaches, research traditions, theories, and methods should be apprehended and adjudged for their success or failure according to how well they understood and resolved the problems *they* confronted. In this, disciplinary history can serve—and to some extent already has served—the progress and promise of political science, in two related ways.

At the methodological level, the history

of political science demonstrates that the discipline has been—and will continue to be—a historical one. Some methodologies are indeed going to be more or less useful than others. But this determination will be contingent on the ability of a methodology to substantiate its claims historically; that is, by its ability to provide good, hard-headed analyses of political life in particular contexts. Recognizing this would have the meritorious effect of tempering the claims of those who claim to have discovered *the* method of truth, as if this alone were sufficient to establish the progress of the discipline.

The second way in which disciplinary history serves disciplinary progress is at the theoretical level. Political science is about theories of how we have lived, how we do live, and how we can and should live together. And theories are, as the postempiricists have shown, historical entities: they develop over time. But in political science theories are doubly historical in that they are also time-bound (i.e., context-dependent). In this respect, good disciplinary history can serve the double function of enabling "paradigm workers" (or "normal scientists") to be sensitive to the history of their own research programs and to the historical limits of those programs. Given the historicity of political life, theoretical progress in political science will not be of the "vertical"—or transhistorically successive and successful—kind found in the natural sciences. Instead, our progress can only be a "lateral" accumulation of potentially useful research traditions, each of which is contextually constrained in its problem-solving power (Dryzek 1986). It may be the case that some long forgotten or apparently outdated research program will shed light on newly encountered problems or even old ones that remain unresolved. It may even be the case that in order to understand the deficiencies of a theory we need to recover its history to find out why, how, and whether it can still be

useful or should be abandoned. Thus good disciplinary history can improve our abilities to make good, contextual choices by making available a varied menu of alternative approaches to our subject matter, along with evidence about when each tradition is likely to be useful, and when it is likely to fail or be irrelevant.

We conclude that there is no neutral stance for evaluating, accepting, or rejecting disciplinary identities. Rather, standards can only emerge in the conflicts and debates within and between traditions of inquiry.⁸ It is in this conflict and debate that the relationship between disciplinary history and identity crystallizes, as we hope to have shown in our discussion of Whigs and skeptics. In this respect, plurality is going to be the essence of, rather than an obstacle to, the progress of political science.⁹ But complacency does not follow from this approval of pluralism. We should be vigilant in our criticisms without being dogmatic, hard-headed in our inquiries without being intolerant of differences, and vigorous in the development of our own positions without being parochial. Such attitudes result from recognizing the historical and contextual situation of perspectives held by those with whom we disagree—and by ourselves. Good disciplinary history both reflects these attitudes and cultivates them. What remains, therefore, is to write histories that would sort out the lessons of the past in a way that future practitioners—and publics—might find useful.

Notes

We thank Lee Cheek, David Jacobs, and Dean Minnix for their comments on an early draft of this paper. Terence Ball and James Farr provided much useful critical commentary on a later version.

1. Besides the numerous texts on the history of political thought, earlier efforts include Haddow 1939, Crick 1959, and Somit and Tanenhaus 1967.

2. In the natural sciences, for example, postempiricists have shown that if scientists had actually conformed to one or another particular method, a

number of clearly progressive theories and programs would have been strangled (Burian 1977, 39). The same is no less true in philosophy, where "analytical philosophy" has been under attack for its failure to offer a coherent account of its own foundations (Taylor 1984; MacIntyre 1984).

3. For a useful overview of the philosophical issues here, see Bernstein 1976. Disputes about the philosophy of social science have for the better part of a century involved the question of whether the natural sciences can provide us with methodological guidance. Postempiricist philosophy of science has now shown that our methods are or can be the same but that the nature of our objects will determine the knowledge claims we can advance (see Bhaskar 1979, 1986). Isaac (1987) provides an interesting account of the applicability of "scientific realism" to political science. Ultimately, these ontological theses must ride on their ability to adequately reconstruct scientific history. And in this, arguments about the belief- or theory-constituted character of social phenomena can underwrite a better explanation of plurality in the history of political science.

4. Even postempiricist philosophy of science itself needed to go back over the history of science to establish its claims (Bernstein 1983, 73–74).

5. Seidelman and Harpham nowhere admit that they are anticapitalists or leftists, but Theodore Lowi, their mentor, so describes them in his foreword to their book (1985, xvii).

6. The classical critique of Whiggish historiography is, of course, Butterfield 1931. While we do not agree with all of Butterfield's conclusions (our objections can be read through our critique of Collini, Winch, and Burrow), those inclined to Whiggish histories of political science might benefit from Butterfield's account of the mistakes of past Whigs.

7. These categories might be applied to premodern political theory as well, but such an application would take us far afield. In any case, the contemporary examples should be sufficient to illustrate our points.

8. For a fuller development of the implications of this kind of argument as it bears on more general questions of rationality, see MacIntyre 1988, esp. chaps. 18, 19.

9. Thomas (1979) argues for plurality in social sciences in a way that differs from but complements our own. For those who think that plurality is a problem, we recommend Thomas's analysis of Soviet sociology (pp. 180–95), which indicates the sorts of difficulties an exclusive paradigm might entail for political science.

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THE TYRANNY OF REASON IN THE WORLD OF THE POLIS

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The modern language of tyranny has distorted the significance of the Greek term tyrannos. In ancient Greek the term was accorded to the new ruler in the city, one whose legitimacy did not reside in his bonds to the ancient rulers and ancient families. Tyranny thus suggested a freedom from the past. Reason, as the Greeks understood it, also entailed a breaking away from the physical world. Reason and tyranny thus work together as expressions of freedom, but it is a freedom that in its transcendence of boundaries leads to tragedy. An examination of Sophocles' Oedipus draws out both the glory and the failure of the individual attempt of the political actor to rise above the historical particular and the mere body to build a world where reason alone is power.

The tyrant no longer figures prominently in our political vocabulary. We may talk of the petty tyrant, the child who at age three throws a temper tantrum in the middle of the supermarket and gets the candy bar, but our political terms of castigation refer to regimes totalitarian, dictatorial, or authoritarian. Except for an eccentric few like Idi Amin or Papa Doc, we reserve the term *tyrant* for the private world of power conflicts in the work place or the family.¹ The word *tyranny*, however, deriving from the ancient Greek *tyrannos*, is far richer than the popular image of an individual who abuses power might suggest. Indeed, it uncovers for us the meaning of rule without limits, whether moral, physical, or historical. The *tyrannos* is the new ruler, the one who has come to power in the city by means other than birth or established precedent. Therewith his illegitimacy—but also his freedom.

The ruler as tyrant becomes the paradigm of the free individual, the one self-perceived as unrestrained, able to carry forth thought into deed.² Among the an-

cient Greeks, tyranny incorporated a freedom to break away from what was old and limiting. Released from traditions, the tyrant could transform the world in which people lived. The tyrannical impulse was not necessarily evil but could indicate a creativity and a freedom to transcend the limits inherited from the past. As an escape from the past, tyranny could also become a model for human rationality and theorizing, the capacity to move beyond accepted boundaries and opinions in order to imagine what was previously unimaginable, to transform the world through the power of one's mind and one's speech, severed from the bonds of birth and history. The tyrant and the rational individual are alike in that they both are "unborn." The tyrant, the new ruler, arises irrespective of parentage, free of the limits of biological succession. The goddess of reason (Athene) in the myth springs forth fully armed from the head of Zeus, likewise free of biological generation.

The tyrannical freedom of reason to break away from the past, as the Greeks

present it, however, elicited tragic consequences for the individual and for the city as a whole. The political task of escaping the tyrant arose not because the tyrant's rule was harsh (often it was not) but because the tyrant's feeling of potency involved a failure to recognize boundaries set by the gods, by nature, and by history. The pre-Socratic philosopher Heraclitus, writing at the end of the sixth century B.C. articulated the general Greek concern with limits: "The Sun will not overstep his measures [*metra*]; if he does, the Furies, the guardians of justice, will find him out."³ Even the gods must acknowledge limits. But the tyrant and the rational individual feel free to transcend limits. On the one hand the tyrant and the rational individual express our freedom to do and be anything; on the other, they reveal the dangers of such freedom. Many of the Greek tragedies of the fifth century B.C. explore the relationships among freedom, reason, and tyranny as they become manifest in the life of the city.

I will turn to Sophocles' *Oedipus* as the man who most vividly illustrates the unity between abstract reason and tyranny—and then reveals for himself the tragic limits that history and biology place on the freedom of both. *Oedipus'* intellect is abstracted from the body (and thus he is able to understand numbers as others could not) and his status as tyrant comes from his apparent distance from the lineage of political authority in Thebes. He rules because he was able to answer the riddle of the Sphinx. Thus his ascent to power on the back of reason is an assertion of freedom that is tragically undermined by the biological basis of his birth. The revelation that his birth, not his reason, is the basis of his claim to rule underlies the tragic uncovering of the play. A political optimism that envisions a world of infinite possibilities, subject only to imagination and reason, meets its match in the last crushing moments of

Sophocles' *Oedipus*.

The *Oedipus* myth itself weighs heavily on us in the modern world. It is a story of passions, sexual and political, of guilt and innocence, of power and disability, of self-flagellation and resurrection, of intentionality and chance. All these surface in the story of the man driven by fate or his own character to kill his father where the three roads meet, to give seed to children in his own mother's womb, and to blind himself when the truth of his own origins is revealed. It is Freud's use of the myth that has colored and, I believe, distorted the tale of the man with the swollen feet. Freud's analysis focuses on the sexual desires inherent in the myth, the private passions that characterize the child's transformation into an adult and thus the family dynamics that take place within the crucible of our most intimate relations. For the Athenian Sophocles the myth of *Oedipus* is not one of family dynamics. Rather, it sits in the world of the polis, and his play addresses the spectators' lives as public actors.⁴ The intra-familial actions in the tragedy have relevance only insofar as they merge into and give force to the political world of action and order.⁵

Embedded in *Oedipus'* very name is the knowledge he has and the knowledge he lacks, the sight he at first had and the sight he will lose.⁶ The ironic tension implicit in the name is never long absent from the playwright's words, and it sears into the mind of the audience the ambiguous status of human knowledge. Sophocles' *Oedipus* raises the question of the nature of knowledge. What can we know and how can we know it? To what degree is our knowledge based on human calculation and to what degree is it based on our experience of ourselves as physical bodies?⁷ Sophocles binds the questions of epistemology to the meaning of rule within the city. To what degree is political action based on the freedom of the intellect, to what degree is it limited by the

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physical world? Oedipus, as ruler, tragically portrays the limits of human knowledge as against our arrogant assumptions of a boundless capacity for insight. The play in this analysis becomes an exploration of the necessary grounding of power and authority in the direct experience of the world of birth and change. As Heraclitus' Sun is bound by certain measures, so too is human intellect, human power, and human freedom. The tragedy of Oedipus is not the fall of a helpless and faultless ruler or the weakness of man subjected to divine laws but the dashed hopes of the power of the mind to rise above the limits imposed by nature, by our biology, and by our past. It is a tale of boundaries overstepped not because of divine prophecies and a divine world hostile to mortal man but because of the freedom that characterizes the tyrant as the tragic hero.

When Oedipus acquires power in Thebes, he is indeed a tyrant, not the legitimate heir. That place belongs to Laius's son—presumed devoured by the beasts on Citheron—or to Creon, Laius's brother-in-law. Oedipus, however, believes he rules because of his intellect. We know that it is his birth that brings him from Corinth to Thebes and makes him ruler there and thus the perpetrator of the most abhorrent of deeds. But we also know that his intellect—his ability to abstract—saves Thebes first from the Sphinx and then from the plague that his own presence evokes. We learn from Sophocles the political value of Oedipus' abstract knowledge, which saves the city. But Sophocles also raises fears about the potency of human reason and of any rule based on it. Reason and tyranny both entail freedom from the *metra* that even Heraclitus' Sun must follow, but both also meet in tragic confrontation the limits that the past imposes on our actions. As such, Sophocles' play becomes a commentary on the modern pursuit of political and intellectual freedom.

The Tyrant

Who is this *tyrannos* about whom the Greeks speak?⁸ We learn from Herodotus that the tyrant is the one who "moves ancestral laws [*patria nomia*] and forces himself on women and kills men who have not been tried" (3.80.5 [1927]). From Thucydides we learn that Alcibiades was considered the potential tyrant: "Most men, fearing the greatness of his lawlessness [*paranomia*] with regard to his body, his daily habits, and the intelligence he showed in whatever he did, set him down as desiring a tyranny" (6.15 [1900]). He manifested such "lawlessness" by breaking the conventions of the society; dressing outrageously in long, purple robes; and having on his shield no ancestral insignia but Eros carrying a thunderbolt (Plutarch, *Alcibiades* 16 [1916]). Or look at Socrates' words in the *Republic*: "Has it not been said of old that Love is a tyrant? . . . Does not the drunken man have something of the tyrannical mind about him?" (573b [1900]). The tyrant is mad, supposing that he can rule not only over human beings but over gods as well (573c). Freed from all shame, making no distinctions between men and women, beasts and men, women and gods, he does not hesitate to murder any man or sleep with his mother or any god or beast (571cd; cf. *Oedipus* 791 [1928]). In Socrates' stylized history tyranny follows a democracy where citizens see no boundaries between citizen and noncitizen or between human and animal. In the almost surreal democracy of the *Republic*, this lack of distinction takes the form of a gentle acceptance of all people. But when democracy becomes tyranny, the lack of distinction takes the form of indiscriminate violence. Plato's picture of a man who does not distinguish between his mother and other women, between the citizen and the noncitizen, between human and beast is meant to frighten us.

Oedipus' tyranny is not violent. He is

not the Caligula or Nero of the Theban world; he eats no babies, cuts off no heads. Indeed, he is a caring ruler, gentle to his subjects, who turn to him as the savior (48) who will set their polis upright again (46, 51) by ending the plague that is killing the young and as yet unborn of the city. Oedipus shows compassion for them in their suffering and devotes himself to healing the city's sickness. "O pitiful children," he calls them. ". . . I know that you all are sick. . . . My soul groans for the polis and me and you" (58-64).⁹ The citizens with their pleas and supplications do not wake him as from sleep, but "weeping" he has "traveled many roads of wandering thoughts" (66-67), calculating how to cure the city. Already he has sent Creon to Delphi to inquire about the source of the pestilence.

The tyrant among the Greeks often sees himself as equal to a god (*isotheos*), freed from the laws that limit other humans (Knox 1957; Vernant 1983, 207). In the first scene of the *Oedipus* the Thebans do appear as supplicants before Oedipus' altars (16), almost as if he were a god. The priest speaks of others in the city who likewise sit in supplication before the altars of Pallas Athene. Yet, despite the supplication and the incense burning, the priest makes clear that Oedipus is not considered a god. "Neither I nor these children here have come as supplicants making you equal to the gods" (31-32). They refer to him as the "first among men" (33) and the "best of mortals" (46), but he remains human. Nor does Oedipus claim divinity. He does not test the gods but respects their speech and their actions.

Oedipus displays none of the negative traits that may be associated with the fifth-century tyrant. He is not like the Alcibiades of Thucydides, whose lawlessness makes the Athenians suspect pretensions to tyranny. And yet the language of tyranny is on almost every page of the play. Knox (1957) in his seminal work, *Oedipus at Thebes*, explains the use of

this language with reference to Athens' growing empire during the fifth century—its position as *tyrannos* vis-à-vis the other cities within its orbit (Thucydides 2.63.2, 3.37.2)—and thus reads the play as a warning about the excesses in politics that led to the defeat of Athens in the Peloponnesian War. Such an interpretation captures well the political force of Greek tragedy as a medium for speaking to the Athenians as citizens of a polis, but it flattens the significance of Oedipus' role. Rather, we must see in Oedipus' "tyranny" not the desire to rule over others or the mad rejection of human and divine laws but rather the ambiguous grounding of his political rule and the freedom of his intellect.

Oedipus acquires authority in Thebes by answering the riddle posed by the Sphinx. As he says in anger to Teiresias, "I, coming, know-nothing Oedipus, stopped [the Sphinx] when I hit the mark, knowing with my mind [*gnome*] and not from the birds" (396-98). In the late moments of the play, the chorus sings about the former glory of Oedipus and says, "He, shooting his arrow high, ruled in all respects in happy prosperity, O Zeus, having destroyed the maiden riddler with the crooked nails; within my land he arose, a fortress against death—from which time you were called my king [*basileus*], . . . ruling in Thebes" (1197-1203). The "arrow" that he shot had only the force of his intellect. His strength lies in his mind and his own authority rests on that intellect. As he taunts Teiresias, "Come now, speak, where were you with the prophet's wisdom? Why, when the singing dog [the Sphinx] was here, did you not bring release for the townspeople?" (390-91). And then in a perverse attack on Creon, whom Oedipus suspects of trying to steal away his authority, Oedipus claims that Creon is a fool for not understanding that *tyrannida* is taken with numbers, friends, and a great deal of money (540-42). This,

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however, is not how Oedipus came to power in the city. He believes that he relied on no one and on nothing except his own mind. Oedipus—who indeed knows nothing about what is most important, the limits set by his own past—sees all as free and open. Thus his ascension to power in Thebes comes as a gift in thanks for his human intellect. His tyranny, as he sees it, is freedom from the past.

As the clues to the mystery of who killed Laius bring Oedipus closer to seeing himself as the object of his own search, it is Jocasta, his mother-wife, who offers an escape from the encroaching evidence against him. At first she appears the female analogue of Oedipus, using argument and calculation, insisting that many cannot be one. The lone witness to the murder of Laius had reported that it was a band of robbers—and the plural is emphasized—who were responsible for the attack.¹⁰ Lone Oedipus could not have been the author of the deed. "Know that this is how he spoke," she says. "It is not possible for him to go back on this. The polis heard these things, not only me" (846–49). But this is only a straw at which Jocasta grasps. In fact, Jocasta views the whole world as chaotic, without divine boundaries or set limits to human behavior; the prophecies of the gods, those orderers of the universe, are worthy of derision: "Why should the human being fear, for whom the things of chance rule. Foresight takes no part in wisdom. It is best to live at random, however one is able" (977–79). Earlier she had told Oedipus that she would look to prophecies "neither now nor henceforth" (857–58). They were wrong about Laius's death; he was not killed by their son—the baby who died on the mountain. The supposed divine order is merely a source of fear for those who do not see clearly.

The chorus is frightened by the impious implications of Jocasta's language and ask that "fate find me bearing holiness and piety in both word and deed—which the

high-footed laws prescribe, born in the airy heavens whose only father is Olympus, nor did mortal nature given them birth" (863–70). The chorus retreats to an order that is decreed from above and is unchanging, subject neither to human manipulation nor to control by speech. Jocasta's willingness to cast aside any divine order as she begins to recognize her son in her husband appeals to Oedipus. His arrogant feeling of the power of the human intellect, which had given him political power and had led to his harsh dismissal of Teiresias, is now prepared to challenge the gods as well, but he never goes as far as Jocasta.

After Jocasta has grasped that Oedipus is her son and the murderer of Laius, she is still willing to live with the apparent impiety, to scorn the gods and their divine rules. She tries to keep Oedipus from discovering the truth about his origins, a truth she recognizes before he does: "Do not, by the gods, if you care for your life, seek out these things. . . . I beg you not to do this" (1060–64). Though Jocasta comes to recognize the accuracy of the prophecies of the gods, she continues to reject the divine order. She is the tyrant who will knowingly overstep any restraints, any ancient laws, just as the tyrant of book 9 of the *Republic* and the Alcibiades of Thucydides' *Athenians*. Oedipus, who had unwittingly overstepped the boundaries that the gods had established, is never so "tyrannical." He tries to be a pious tyrant and fails.

The interchange between Jocasta and Oedipus is one of the highlights of Sophocles' play as the wife-mother begs her husband-son to view the world as random and to ignore the question of his own origins. Oedipus, who understands so much on the abstract level, misconstrues Jocasta's pleas, wrongly concluding that she, with a woman's arrogance, is ashamed that a lowly birth may lie in her husband's past. While he will not accept her urging that the divine world is

chaotic, he does revel in the idea of chaos on the human level—a chaos that liberates him from any restraints the past may have placed on him. He ascribes to the human world the disorder Jocasta argues for the divine and calls himself the child of Chance, “she who gives well.” He is not ashamed of this: “I was born of this mother, and the months—my brothers—defined me as both small and great” (1080–84).

The aristocratic concern with paternity—and thus legitimacy—whether political or familial, fades before the individual freedom to become great on one’s own. A drunken man’s taunt that Oedipus did not know who his parents were (779–93) precipitated the trip from Corinth to Delphi and then on to Thebes. While he was still in Corinth the question of origins determined his place in the world. Once away from Corinth, he secures his place in the world on the basis of his mind. Birth and origins no longer matter to him. Thus, suddenly buoyed by this hope that he is the child of Chance, Oedipus sees an opening, a vista of freedom for himself—and indeed for all men.¹¹ The possibility that he is such a child, born on the mountainside, opens the door to the greatest freedom, the freedom to be anything, to see no limits coming from his birth or physical form. A wild child of the mountains, he could become the king of Thebes. Released from all concern with origins and legitimacy, he is the tyrant who can be anything.

From such mad optimism Oedipus and the chorus will crash into the realization that he is not Chance’s child but is bound ever so tightly by the nature of his birth. He is a man of history and of place. When he discovers that he is not a tyrant because he is the legitimate heir to the kingship, he also loses the freedom he would have had as Chance’s child. As he discovers his legitimacy, he finds that he has been a tyrant in the more vicious sense, for he has transgressed the most

profound of human and divine laws in the murder of his father and the sexual relations with his mother. As Oedipus has his final moments of hope and the chorus sings of the nymphs who may have borne Oedipus, the audience knows that Oedipus is not Chance’s child, that the belief in such freedom in this world is ill founded, that all is circumscribed. Oedipus’ open vistas close, and he learns that human actions are not free but are limited by the past. When the truth of Oedipus’ birth is revealed, the tyrant becomes the king. As the biological legitimacy of his rule surfaces, the tyrant as “unborn,” freed from the limits of the physical world, becomes in the tragedy of this play a “born” king. As tyrant Oedipus was a good ruler. Now that he has been revealed as king, he is revealed as the transgressor of ancestral laws, the madman who—in the language of the *Republic*—does not distinguish between his mother and other women, his father and other men.

Reason

The talon-nailed Sphinx sits high on the road outside the city of Thebes shrieking forth her unmusical songs. Oracular in her speech, the winged virgin demands her tribute—the young men of the city—until someone, by solving the enigma she poses for them, can destroy her. None succeed until “know-nothing Oedipus” (397) wanders by in his resolve to escape the fate prophesied by the god Apollo—that he is to kill his father and have sexual intercourse with his mother. Thus, on the road to Thebes, the Sphinx asks who the creature is who walks on four feet in the morning, two in the afternoon, and three in the evening. “Man,” answers Oedipus. And from her high cliff the Sphinx throws herself to her death, and Oedipus becomes ruler in Thebes.

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Others who had stumbled in trying to resolve the enigma met cruel deaths. What peculiar mental ability enabled Oedipus to succeed where others have failed? As one crippled by the stake that pierced his feet as a baby—and perhaps leaning on a staff—(Benardete 1964, 1), he understands the issues of walking as others do not. He sees the differences between himself and others, they who walk comfortably on two feet in the afternoons of their lives. Specifically, Oedipus can abstract from himself, who walks on three feet in the afternoon. He does not rely on direct experience for any answer, but—freed from the limits of the body—he calculates and discovers the unity of the three metamorphoses to which the Sphinx' enigma referred. Thus he can save Thebes from its suffering even though he has never been taught by the city. His mind has the ability to abstract, he recognizes unity in diversity, and he sees that the several can indeed be one, that one can encompass many. Four-, two-, three-footed animals are one animal: morning, noon, and evening are one life. He sees fluidity in form where others can only see the unique and the particular. Thus he can add and he can count, for measurement entails making the diverse commensurate, abstracting from the particular, seeing the "chair" in the settee and the porch swing or the "man" in the baby and the tottering old fool or the Theban in the old priest and the soft-cheeked boy.¹²

Oedipus is able to move from the particular across boundaries to the general. The relationship between the one and the many does not confound him, he who in his own person expresses an unholy unity. Unlike the others, who fell victim to the stranglehold of the Sphinx, he transcends divisions. Oedipus, however, as one who is marked by the incest in which he unwittingly engages, carries the unity of apparent opposites to such impious extremes that he becomes the pollutor of Thebes rather than its savior. In the words of

Teiresias, Oedipus is to his children at once father and brother and to the woman from whom he was born both son and husband and to his father killer and "fellow sower" (*homosporos*) (457–60). Once he has discovered the unity in his own person, he cries out to the chorus, "O marriage, . . . you showed forth fathers, brothers, kindred blood, brides, wives, and mothers" (1403–7). Oedipus' personal life, destroying the boundaries between things that must be kept separate, parallels the transcendence of boundaries that characterizes his intellect.

The tyrant, likewise unifying what is multiple and transcending the boundaries of the traditional laws, threatens the vitality of the city. By freeing himself from the past in terms of origins of political power and ancient laws, the tyrant destroys the community over which he rules. The chorus, in a famous phrase claims, "Arrogance breeds tyranny (*Hybris phuetai tyrannon*)" (873). This passage has perturbed many (Scodel 1982). Lord Acton's adage about power's corrupting has clouded our consciousness and many an editor has worried over how to get this meaning back into the Greek. But Sophocles' chorus means what the words say: *hybris*, arrogance, a sense of freedom from the measures or boundaries that control other human beings, gives birth to the tyrant, he who madly does all, regardless of the laws of men or of gods.

The tyrant is often faulted in ancient Greek authors for treating the city as if it were his family and for failing to recognize boundaries between the private world and the public world of the city. The tyrant uses the wealth of the city as if it were his and the people as if they were his slaves. Oedipus' intellectual style of dissolving boundaries defines as well how he rules in the city. He is not sinister in his confusion of these boundaries between self and city and between city and family, as the Greeks suggested many tyrants

were. The mind that broke down the divisions that the Sphinx had posed also breaks down the divisions in the city. He himself becomes the city and the city becomes his own person. While each one of the citizens is sick and each weeps for himself alone, he, Oedipus, as the bodily unity of the many citizens, weeps for all, "for the city, for myself, for you altogether" (64). Creon, who will act as tyrant in the *Antigone* here complains, "There is also a part for me in the city, not for you alone" (630). But it is also Oedipus' identification with the city that pushes him to pursue a cure for the illness of his subjects.

Even more powerful than this identification with the city is the transformation of the city into his family.¹³ In the very first words of the play, Oedipus calls to his subjects, "O *tekna*" ("O offspring"). Disregarding the processes of birth and reproduction, Oedipus calls them his offspring not once but twice (1, 6). It is the priest who shifts from *tekna* to the more neutral *paides* (children), a term that Oedipus picks up in the first words of his next speech: "O *paides oikitroi*" ("O pitiful children") (58). Though Oedipus poses as a father to his city, he is father to a city that is sterile. The women are unable to bear children, suffering long in fruitless labor pangs (26-27). The seeds of the crops wither, as do the cattle in the fields. The natural world of Thebes, polluted by the presence of the man who unites in his body what must be separated, rebels against the processes of generation. Thus while the physical world denies women and men their offspring, denies the seeds their sprouts, Oedipus generates through speech and abstraction the *tekna* in the world of the polis. The man who conquers the Sphinx by overcoming the apparent differences between two, three, and four feet fails to acknowledge the difference between the theoretical world of the city created by the human mind and the intimate physical

realm of the reproductive family.¹⁴

Oedipus does not transform his city into his family only through language images. When Creon reports the oracle's demand that the murderer of Laius be expelled from the city, he does not say who is to find and punish the guilty man. Oedipus immediately commits himself to this endeavor,¹⁵ though he later admits the impropriety of this pursuit, since he is a stranger (*xenos*) in Thebes (218, 220, 813, 817). According to Attic law, at least, homicide cases were the responsibility of the kin of the murdered man (Gagarin 1981 and MacDowell 1963). The public prosecutor was unknown. Thus Oedipus acts as a relative of Laius, little understanding the dramatic irony entailed in such a task.

Oedipus expresses surprise that the murder of the king was not investigated at the time of the murder. "With your sovereignty [*tyrannidos*] thus fallen, what evil impeded the search?" (128-29). Obviously, it was the Sphinx. But instead of personalizing the issue—seeing the familial ties of Creon to Laius through Jocasta—Oedipus focuses on the impersonal, on the political possibility of sedition and the abstract notion of "sovereignty,"¹⁶ the abstract unity of the city incorporated in the body of the king. In part because the plague affects the city as a whole, Oedipus takes Laius out of the context of the family and sets him into the abstract world of the polis.

Since the murderer of Laius had not been pursued before, Oedipus himself will pursue him "as if" (*hosperei*) Laius were his own father (264). Despite all the dramatic irony in this image of himself as related to Laius, Oedipus cannot, at least early in the play, call himself kin to the murdered man. He must still introduce the *as if*, the simile that underlines the illegitimacy of the search he undertakes for the city and indicates his denial of the traditional laws concerning the prosecution of evildoers. The simile again shows

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Oedipus' capacity within the context of the city to move beyond the particular. The *as if*, while speaking one language to the audience who know the secrets of Oedipus' life, also alerts us to what is not: the city is not a family, and it is only the ability of Oedipus' mind to abstract from the particular that allows him to work on this level of abstraction where the former king of Thebes can be treated "as if" he were Oedipus' father. The images that Oedipus introduces to establish the city as family underscore the inadequacy of this analogy. The citizens of Thebes are not his *tekna*, his offspring. This is not to suggest that kinship is irrelevant for the city. Traditional legitimacy depends on the lines of blood relations. The curse that manifests itself in the lives of the citizens of Thebes reveals the relationship between the purity of the family and the purity of the city. But to treat the city as a family, as Oedipus does, is the mark of tyranny.¹⁷

Oedipus' search entails all the language of a legal investigation, but through the course of the play that search is transformed from a formal investigation to the most personal exploration, "Who am I?" Oedipus must reorient himself from the abstract city in which he has unified the many and with which he identifies to the most particularized and unique "I." As he says toward the end of the play, "My evils no one else except me among mortals is able to bear" (1414-15). From his identity with the city over which he ruled, he learns of his own uniqueness and the boundaries that separate him out from others.

When Creon returns with the message from Delphi, Creon suggests that Oedipus may wish to hear the oracle within (*eso*) in privacy (92). Oedipus refuses: he will hear the message *es pantes*, before all (93). Creon—dull-witted in this play—sets up boundaries between the many—the city—and its leaders. Oedipus smashes those boundaries. His openness reflects his at-

tempts to obscure the lines that separate the private and the public world, the traditional contrast between the family and the city (Saxonhouse 1983). Indeed, the search will go on within his household as well as without: "If [the pollutor] is at my hearth in my household with my knowledge may I suffer the curse I pronounced on others" (249-51). Oedipus makes no distinction between his own needs and those of the city. Creon recognizes the need to separate the public and the private. At the end of the play, when Oedipus appears on stage, his self-mutilation vividly apparent to all, the private tragedy reveals itself to the city. Creon, maintaining the distinctions between public and private, between leaders and subjects, sends Oedipus away from public view: "As quickly as possible, go into the house [*es oikon*]" (1429, 1515). Creon has a sense of shame, of limits on what we can do in public and what we can reveal of ourselves to others. The openness of Oedipus is shameless; he holds back nothing. It is precisely that openness that enables him to save the city at the same time that he becomes its pollutor.

The clarity and openness of Oedipus' reason contrasts starkly with the hidden knowledge of Teiresias, he who may see things "most clearly" (286). In his early confrontation with the prophet, Oedipus taunts him: Why was the private wisdom of prophecy not effective against the singing dog? (390-92). Teiresias, the medium through whom the gods transmit their laws to men, cannot on his own save the city; and in his frequent exhortations for Oedipus to cease his search, he appears ready to condemn the city to unending suffering. From Oedipus' vantage all knowledge must be open to all. There should be no boundaries between those who know and those who do not know. The message from Delphi is for all Thebans to hear. Words for bringing to light (*phano*) are repeated throughout the play. The plea is to open the unknown, to

reveal what is hidden in shadows. With all uncovered, the distinction between public and private knowledge vanishes. Thus Oedipus cannot understand Teiresias' willingness to hold back knowledge and to prevent some from knowing the whole truth. Oedipus sets no limits to what we must know. Teiresias does, for he understands, as Oedipus does not, that unfettered knowledge has its own dangers and that the answer Oedipus seeks is far more complex than Oedipus, whose intellectual skills lie in simplification, realizes.

The tragedy of the *Oedipus*, of course, lies in the fact that Teiresias' aim to hide the knowledge he has received from the gods leaves the sickness in the land, while Oedipus' attempt at discovery reveals that the pursuit of knowledge can uncover pollution at the same time that it saves. Sophocles does not leave us siding with Teiresias, that some things are best shrouded in mysteries. Oedipus is the hero in his testing of limits, in his quest to go beyond. The plague forces him to search. The uncovering must take place, but that uncovering is not without its own tragedy precisely because what is revealed is not freedom, but the *metra*. The revelations of the play uncover the unintended consequences of his intellectual and biological freedom. He must incorporate limits not only on the powers of reason, but also between individual and city, between public and private.

The chorus knows well that there are limits—that there are divine laws that do not grow old and that are not generated by mortal nature but are born with Olympus alone as their father (862–71). They are the laws that Antigone in her tragedy will describe as “unwritten” (454) and that today we might refer to as “natural laws.” Within this realm Teiresias works, revealing the laws of the gods to the city. He knows structure and understands limits. For Oedipus all is fluid, as man, for example, moves from one form to another,

from one period in life to another or as the city becomes the family and the family becomes the city.

In contrast to Oedipus' equation early in the play between the city and family are the powerful final scenes in which Oedipus weeps for his daughters, his own half-sisters. His intellect had enabled him to succeed in the world of abstractions and in the political world where the many must, in some fashion, become the one in order for the city to exist. At the end of the play, his attention turns to the particular, to his own offspring rather than the citizens whom he called *tekna* in the first line of the play. From the concern with the sterility of the city he turns to the sterility of the family. No one will marry his daughters, he laments, polluted as they are by their double relationship to him. Unlike the city and unlike the earlier understanding of his relationship to Laius, they are bound to him not by the *as if*, not by simile, but through the biological processes of reproduction. The intellect of Oedipus' earlier life created abstractions from the particular—the children of the city from the particular child. Now he returns from abstraction to particularity.¹⁸

As Oedipus pursues his quarry himself, he discovers that reason is not enough to rule. Teiresias fears Oedipus' reliance on reason, knowing that the very reason that saved the city the first time and put Oedipus in power may save the city again but destroy Oedipus. While reason can destroy the Sphinx, it also enables Oedipus to learn of the impieties that lie at his birth and at the foundation of his rule. Despite Oedipus' early focus on the world of abstraction, a concern with the particular facts about his own birth motivate him. Thus Teiresias, insulted by Oedipus, upsets Oedipus in turn by asking, “Do you know from whom you are?” (415). Coursing powerfully through Oedipus' psyche was his curiosity and his fear about his birth. A drunken man's taunting him with the suggestion that he

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was not his father's son upset him deeply (781). Even after assurances from his presumed parents, Polybus and Merope, he let the worry get under his skin (786), headed towards Delphi and the oracle's response that set him on the path to where the three roads meet, and then went on to where the Sphinx reigned.

Oedipus in his search for his own origins has raised the problem of the importance of history. As the "know-nothing Oedipus," as "untaught" Oedipus, he can measure (795, 1110)—see the unity between the one and the many—but he does not understand the past as a limit on what one can achieve or know. He tries to cast off his history by coming to Thebes. Sophocles dismisses such optimism, for such actions entail, as well, the dismissal of the gods and their prophecies. Had Oedipus accepted the god's words, he would have returned to Corinth, killed Polybus, married Merope, and thereby avoided the fate predicted for him. Although along with the chorus he was shocked by Jocasta's dismissal of divine order, he himself, by trying to escape his origins and the future foretold for him, demonstrated the same impiety.

Oedipus searches for a freedom from the past, but it is a freedom that the poet and the gods do not allow him. It is this struggle between freedom and openness of the mind and the limits of biology and history that lies at the heart of the tragedy of Oedipus. He who is free to answer the riddle of the Sphinx by going beyond the particular must learn the importance of the particular. In the poignant final scenes Oedipus has, with his blindness, turned to his particular children rather than the abstract children of the city. He calls to them, laments for them. Still he emphasizes a unity with his daughters, but the focus has shifted from the general to the particular, from the abstract to the precise. He longs for the unity afforded in the private world: "Touching them with my hands, I might seem to have them"

(1469–70).¹⁹ Such unity and identification with the city, with the public world, has been denied him.

The movement of the play is in part the discovery of the importance of boundaries on what we, as human beings, can accomplish. This education takes us from the unity of city—one in which youth and age are jumbled together and the ruler is identified corporeally with the city—to one in which Oedipus weeps for himself and his daughters.²⁰ He who could cut through categories learns that differences do exist between the many and the one, the mother and the wife, the daughter and the sister, family and polis, ruler and his city. Reason and tyranny play the same role in the world of the polis. In one sense they can liberate and save the city from its suffering, can bind the city together into a unified whole; but in another sense, by obliterating differences they bring on the wrath of the gods. Oedipus, as the tyrant and the paradigm (*paradeigma*, 1193) of the rational individual, learns vividly that the reason that can unify cannot alone be the basis of his rule: his birth—his bonds with the physical world—are the origins of his power. Before, he relied on nothing but the brilliance of his mind; "know-nothing" and "untaught" were the phrases he used in self-descriptive scorn of those who he thought threatened him. At the end, in his blindness, he has learned that knowledge is more than calculation and that political power is not only the gift of reason.

Limits (*Metra*)

Greek tragedy and Greek thought in general explore the tension between limits and freedom of action. Heraclitus in his cosmology saw limits that constrained even the movements of the divine Sun. The orderly world will not brook independent actions even on the part of the gods. Herodotus gave limits a physical

form in the geographical boundaries over which kings marched to their own destruction. Herodotus called the enforcement of these limits not the justice of the Furies, as Heraclitus had, but the jealousy (*phthonos*) of the gods; this jealousy preserved an order among men, allowing no man in his pride to become too great. In Thucydides the limits do not have the visual impact of rivers and mountains but appear rather in the consequences of the dynamic of the growth of cities. The Athenian's limitless desire for more (*pleonexia*) sends Athens on the ill-fated expedition to Sicily. Oedipus is like all these characters from the writings of the Greeks. His ability to reason and to ignore traditional boundaries in his mind and in his body makes him like the errant Sun; like Cyrus who, in Herodotus' tales, crosses the Araxes; like the Athenians who send troops to Sicily. Sophocles tells the audience about the tragic limits to human knowledge and to human power. Boundaries hold back the Sun; the same boundaries restrain the tyrant, the measurer, the calculator.

The *Oedipus* in its cathartic final moments, as the self-blinded tyrant-king gropes for his children, demands that the audience fear the awesome power of abstraction and the freedom of the ruler's intellect. The tyrant as the "unborn" and the tyranny of reason bring tragedy to the family and the city. In his capacity to abstract, he has lost a sense of the limits that his own origins set on himself as well as on the city. The tragedy of the *Oedipus* draws for us both the glory and the failure of the individual attempt of the political actor to rise above the mere body and build a world where reason, released from the defective body, alone is power.

Sophocles offers his play as a warning. Humans—tyrants or not—attempting the transformation of the world on the basis of abstract, calculating reason alone, with no regard to the limits of history or of piety, will call forth the Furies enforcing

the *metra*. Sophocles urges the audience of Athenian citizens to reflect on the consequences of viewing the world as a sea of undifferentiated unity, where the child is the youth is the grandfather in the comprehensive concept, "man," where "the Theban" is at the same time the "young not yet ready to fly" (16–17) and the old priest. Such abstraction belongs to the divine world of "unborn laws," towards which the city in its own desire for universality strives, while the individual, no matter how wise, must acknowledge differentiation and the boundaries between self and the ruler in the city, between city and family.²¹

Oedipus stands forth as a "paradigm" (1193), a shocking example to the chorus and to us of the excessive unity and overstepping of boundaries that brings sterility to the city. The city itself flourishes and gains life from differentiation. The son cannot also be the father. The city cannot also be the family. Yet, to exist, the city must rise above the particular and unify the many, see "the Theban" in the young boy and in the old priest. The balance is difficult and Oedipus in his person and his intellect captures the excesses of such unity. The tragic vision of Oedipus shattered by the knowledge of what he has done does not resolve the tension or delimit where the boundaries are or how we can recognize them. On the one hand we must abstract and generalize, move from the particular to the universal and not be hedged in by the particular. On the other hand this drive can be dangerous if we lose sight of the particular and unify what cannot be unified, the mother and the wife, the sister and the daughter, the family and the city—as Socrates, for example, argues in the fifth book of the *Republic*.

In some sense we are all like Oedipus, not in Freud's terms of our psychosexual development, but rather in our desire to theorize, to impose an order on the world in which we live, while rejecting the ran-

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domness of Jocasta's world or the privacy of Teiresias. As theorists, we see the world encompassed by human intellect and we value the coherence that Oedipus wishes to impose on the world. We can also see in this theorizing impulse of Oedipus that tyranny does not necessarily come from what is base or ignoble but from what is perhaps most noble and what is most daring, what Oedipus and Socrates in a sense share—in their transcendence of tradition, in the denial of limits and of origins, and in their care for the whole. At the same time however, Oedipus the tyrant, when he appears before us on the stage, bloodied, groping for his daughters, incorporates the tension between the limits we must observe and the freedom of our intellect. He becomes the tragic symbol of the tyranny of reason in the polis.

Notes

Earlier versions of this paper were presented at the 1987 annual meeting of the American Political Science Association, Chicago and as the Arthur M. Wilson Memorial Lecture at Dartmouth College, 1988. I have benefited greatly from comments on both those occasions.

1. Strauss, in response to events of the twentieth century and to a political science that he saw as "haunted by the belief that 'value judgments' are inadmissible in scientific considerations" tried to revive *tyranny* as a term to describe "a danger coeval with political life" (1963, 21–22) but his attempt met with little success at least in the "mainstream" professional discourse. While there is some overlap between our understandings of the meaning of tyranny, I want to draw out the positive aspects of tyranny as well as the negative ones so as to portray the ambiguous status of the term.

2. The language here is meant to recall the language used by the Corinthians to describe the Athenians in Thucydides (1.70.2). See also Callicles' description of the "real man" in Plato's *Gorgias* (491b [1900]).

3. Fragment 44 in Kahn 1979 (p. 48).

4. Benardete (1964, 2) sees a movement "from a political to a family crime." He argues that the latter is the "more comprehensive."

5. The literature on the *Oedipus* is so thick, the insights so varied that one cannot take account of all the interpretations and analyses that have emerged

in the last decades. Much of the literature on the *Oedipus*, however, is concerned with the relationship between Oedipus and the fates, whether Oedipus because of his anger or his suspicions of others in some way "deserved" to suffer as he did. After all, his parents were the ones who tested the oracle and the shepherd who kept him alive disobeyed orders from his ruler. This tack would take us into the question of the justice of the Greek gods. I wish here rather to plumb the play for what it tells us about reason, power, and politics. Little of what has been written has dealt with the political thought implicit in the *Oedipus*. Those works that do treat the play with a view to its political thought that I have found most useful include Benardete 1964, Schwartz 1986, Segal 1981, and Zeitlin 1986.

6. The Greek word *oida* means both "know" and "see"; the word *oideo* means "swell." Sophocles takes full advantage of the verbal games such language allows him. See, e.g., lines 16 and 292.

7. In many ways this play can serve as a prelude to Plato's epistemology. In particular, the image of divided line in book 6 and the cave in book 7 of the *Republic* tie these two works together. Cf. Nussbaum (1986, 13) who suggests that Plato must be read in the context of the poetic tradition: "Plato's writing so continually alludes to his poetic context in its choice of image, story, and turn of phrase that the meaning of many salient details is lost on us if we do not try to approach him in awareness of this context." See nn. 12 and 17. Interpretations of this tragedy have often focused on Oedipus' "love of learning" as his "tragic flaw." But within the whole play "love of learning" cannot be dissociated from the political context. Oedipus does not want to know who killed Laius for the sake of knowing; he needs that knowledge because he is the ruler of Thebes. Similarly, the knowledge he *can* gain depends on who he is.

8. Some unknown Hellenistic editor is responsible for the title *Oedipus Tyrannos*. So far as we know, Sophocles titled his tragedy simply *Oedipus*. Yet, the epithet *tyrannos* (unlike the Latin *rex*) is apt. Andrewes notes that a tyrant "in these Greek terms, is not necessarily a wicked ruler, but he is an autocrat (and generally a usurper) who provides a strong executive" (1956, 7) and that "tyrant, which was never any man's title, is the appropriate word for a man who seized power in an irregular way" (p. 28).

9. All translations are my own. The elegance of the poetry is sacrificed for the sake of precision.

10. The curiosity is that Oedipus responds in the singular, changing the number of robbers from many to one (122–24, 292). Vellacott (1971) uses this change as the basis for his novel interpretation that Oedipus knew all along that he was the murderer of his father and husband of his mother.

11. Segal reads this passage somewhat differently: "He lacks the basic information about his origins that gives man his human identity and sets

him apart from the undifferentiated realm of nature and the anonymous realm of beasts" (1981, 207). I read a more optimistic tone into the passage than Segal, sensing on my part the antiaristocratic implications of the words. I find myself more in sympathy with Bowra's remark, "He is a self-made man who will be remembered not as the son of someone, but as the man who answered the Sphinx's riddle and became king of Thebes" (1944, 198).

12. Socrates' discussion of the divided line in the *Republic* (509d-11e) is apposite here. Benardete comments, "Oedipus saw the heterogeneity that underlies an artificial homogeneity" (1964, 6). I am not sure why Benardete calls this "artificial," unless it is because the homogeneity is imposed by the human intellect. Knox draws out well the use of numbers throughout the play and Oedipus' role as the measurer, the one who proceeds through calculation (1979, 101).

13. In contrast to Benardete's emphasis on the focus away from the political to the family (1964, 3, 9), I see the development in terms of the separation of the two that Oedipus must acknowledge to understand his tyranny. Benardete is not entirely consistent here, for while he sees a "shift," he also argues forcefully that Oedipus violates "equally the public and private with a single crime."

14. This dichotomy between family and city obviously ignores many of the overlapping points between the two and their interdependence. In Greek literature, however, this dichotomy plays a frequent and profound role. For a fuller discussion of this opposition see Saxonhouse 1980 and 1983.

15. Here again we must note the similarity between Oedipus and the Platonic Socrates. The oracle as reported by Chaerephon gives no command to Socrates, though Socrates interprets it as one (*Apology* 23ab [1900]).

16. I realize that "sovereignty" is not a very good translation for *tyrannis*, *-idos* but I am hard pressed to find the English equivalent that can capture the abstraction of the word and not also transfer to it the negative connotation of "tyranny."

17. The connection with Plato's *Republic* is important. Consider in this context the city in book 5; see further Nichols 1937, chap. 3.

18. See n. 4.

19. The impious sexual overtones of this sentiment remind us that the boundaries must exist within the family as well as within the city.

20. The failure of Oedipus may perhaps be matched by the success of Sophocles, who as tragic poet can combine the knowledge of Teiresias with the reason of Oedipus in one play. On the peculiarly privileged position of the playwright in Athens see further Euben 1986, chap. 1.

21. See further Saxonhouse 1988.

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LIBERAL VIRTUES

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I explore possible links between liberalism and conceptions of individual virtue. I first distinguish between virtue seen as instrumental to the preservation of societies and institutions and virtue seen as intrinsically valuable, that is, as an end in itself. I argue that certain distinctive instrumental virtues are required for well-functioning liberal societies, economies, and political institutions. I then sketch different versions of liberal intrinsic virtue and explore the tensions among them and between them and liberal instrumental virtue. I conclude with some competing conjectures as to what extent necessary liberal virtues are being fostered through current institutions.

For two generations, scholarly inquiry has been dominated by the belief that the liberal polity does not rest on individual virtue. On the theoretical plane, liberalism has been understood as the articles of a peace treaty among individuals with diverse conceptions of the good but common interests in preservation and prosperity. On the level of basic institutions, the liberal constitution has been understood as an artful contrivance of countervailing powers and counterbalancing passions. In the arena of liberal society, individual behavior has been analyzed through the prism, and public policy guided by the precepts, of neoclassical economics.

Virtues versus Interests in Contemporary Liberalism

The conclusion that liberalism could be severed in theory and in practice from the concern for virtue was shared by scholars of widely divergent orientations. While Leo Strauss was on the whole sympathetic, and C. B. Macpherson hostile, to the liberal polity, they converged on an interpretation of Locke that stressed his

effort to liberate individual acquisitiveness from traditional moral constraints (Macpherson 1962; Strauss 1953). Martin Diamond and Gordon Wood could agree on the essentials of the interest-based "new science of politics" that displaced civic republicanism and undergirded the Constitution (Diamond 1959; Wood 1969). The understanding of modern liberal society as an agglomeration of self-interested individuals and groups formed a common point of departure for both defenders of pluralism (such as Robert Dahl in his 1950s incarnation) and critics, led by Theodore Lowi (Dahl 1956; Lowi 1969).

Although the various analysts of liberalism were not in agreement on a specific conception of virtue, they united in the accusation that liberal theory and practice were bereft of—if not hostile to—any conception of virtue. For Strauss and many of his followers, liberalism placed in jeopardy both the restraints on passion that should govern the daily life of the many and the striving for excellence that should guide the activities of the few. For J. G. A. Pocock and his allies liberalism represented the evisceration of republican

virtue understood as the disposition to subordinate personal interests to the common good (Pocock 1975; Skinner 1984). For Charles Taylor and his fellow communitarians, liberalism undercut the very possibility of community and thus the significance of the virtues understood as the habits needed to sustain a common life (MacIntyre 1981, 1984; Sandel 1982, 1984; Taylor 1979). To the extent that virtue could nonetheless be found in the actual practices of liberal society, it was to be understood, argued Irving Kristol and many others, as the residue of an older moral and religious tradition at odds with—and under relentless assault by—liberalism's most fundamental tendencies (Kristol 1980).

The proposition that liberalism does not rest on virtue is not the arbitrary invention of contemporary scholarship. Albert Hirschman (1977) has traced the emergence in seventeenth- and eighteenth-century social thought of the thesis that republican government could best be secured not through civic virtue but rather through the liberation of the commercial-acquisitive "interests" of the middle class in opposition to the politically destructive "passions" of the aristocracy. The most famous of the *Federalist Papers* (10 and 51) contain memorable formulations of the need to counteract interest with interest and passion with passion. Immanuel Kant, who was at once the most profound moral philosopher and the most devoted liberal theorist of his age, argued vigorously for the disjunction between individual virtue and republican government:

The republican constitution is the only one entirely fitting to the rights of man. But it is the most difficult to establish and even harder to preserve, so that many say a republic would have to be a nation of angels, because men with their selfish inclinations are not capable of a constitution of such sublime form. But [this is an error: republican government] is only a question of a good organization of the state, whereby the powers of each selfish inclination are so arranged

in opposition that one moderates or destroys the ruinous effect of the other. The consequence . . . is the same as if none of them existed, and man is forced to be a good citizen even if not a morally good person. The problem of organizing a state, however hard it may seem, can be solved even for a race of devils, if only they are intelligent. (1963, 111–12)

In spite of the considerable evidence for the proposition that the liberal-republican polity requires no more than the proper configuration of rational self-interest, this orthodoxy has in recent years come under attack from scholars who argue that liberal theory, institutions, and society embody—and depend upon—individual virtue. Judith Shklar has traced the emergence of liberalism to a revulsion against the cruelty of religious wars, that is, to a decision to replace military and moral repression with a "self-restraining tolerance" that is "morally more demanding than repression" (1984, 5). Rogers Smith has found in Locke a core conception of "rational liberty" on which are based a distinction between liberty and license and an account of individual excellence (Smith 1985). Summarizing a painstaking reexamination of the neglected *Thoughts Concerning Education*, Nathan Tarcov concludes that "instead of a narrowly calculating selfishness, Locke teaches a set of moral virtues that make men able to respect themselves and be useful to one another both in private and in public life" (1983, 137; see also Tarcov 1984). Ronald Terchek extends this thesis to Adam Smith and John Stuart Mill, whom he interprets as recommending "the cultivation of those habits which turned us toward the practice of virtue" (1986, 18). J. Budziszewski offers a general argument for the proposition that liberalism and the cultivation of the virtues can be logically compatible and even mutually supportive (1986). In Harvey Mansfield, Jr.'s striking rereading of the *Federalist*, the "automatic or mechanical" view of our constitutional arrangements is replaced by a focus on well-ordered souls as the foundation of

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sustainable republican government: "Not only are the people expected to be virtuous but also those who run for office" (1987, 59). James Q. Wilson's survey of contemporary public policy dilemmas concludes that economic diagnoses and prescriptions, which treat individual dispositions as fixed and exogenously determined "tastes," are at best one-sided. The challenge of social policy is not just the manipulation of incentives, but also the formation of character: "In almost every area of important public concern, we are seeking to induce persons to act virtuously. . . . In the long run, the public interest depends on private virtue" (1985, 15-16).

If this line of argument is correct, there is a tension at the heart of liberalism. The liberal state must by definition be broadly tolerant, yet it cannot be wholly indifferent to the character of its citizens. As Thomas Spragens has noted, "a citizenry without public spirit, without self-restraint, and without intelligence accords ill with the demands of effective self-governance" (1986, 43). To quote Judith Shklar once more, the alternative before us "is not one between classical virtue and liberal self-indulgence. . . . Far from being an amoral free-for-all, liberalism is, in fact, extremely difficult and constraining, far too much so for those who cannot endure . . . the risks of freedom. The habits of freedom are developed, moreover, both in private and in public, and a liberal character can readily be imagined" (1984, 5). The challenge, then, is to give an account of individual virtue that supports rather than undermines liberal institutions and the capacious tolerance that gives liberal society its special attraction.

The thesis that liberalism rests in some measure on virtue is not the palpable absurdity that the liberal polity requires an impeccably virtuous citizenry, a "nation of angels." Nor is it incompatible with the mechanical-institutional interpretation of liberalism, for clearly the artful arrangement of "auxiliary precau-

tions" can go some distance toward compensating for the "defect of better motives." Nor, finally, does this thesis maintain that the liberal polity should be understood as a tutelary community dedicated to the inculcation of individual virtue or excellence. The claim is more modest: that the operation of liberal institutions is affected in important ways by the character of citizens (and leaders) and that at some point the attenuation of individual virtue will create pathologies with which liberal political contrivances, however technically perfect their design, simply cannot cope. To an extent difficult to measure but impossible to ignore, the viability of liberal society depends on its ability to engender a virtuous citizenry.

While this requirement is not unique to liberal societies, it poses special difficulties for them. The liberal way of life frees individuals from traditional restraints and allows them to pursue their own conceptions of happiness. To the extent that the liberal virtues are not simply consistent with individual self-interest, processes of forming and maintaining them will come into conflict with other powerful tendencies in liberal life. The liberal virtues are the traits of character liberalism needs, not necessarily the ones it has. Yet it is unnecessary to import these virtues from the outside, for they are immanent in liberal practice and theory. The tension between virtue and self-interest is a tension within liberalism, not between liberalism and other traditions.

The Virtues As Ends and As Means

The classical conception of the relation between virtue and politics was spelled out by Aristotle. Individual virtue (or excellence—the Greek *arete* will bear both meanings) is knowable through everyday experience, definable through philosophic inquiry, and is always and everywhere

the same. For Aristotle, the virtues are not just Greek, but rather human, virtues. Political life must be seen as in large measure a means to the attainment of virtue, understood as an end in itself. Once the threshold conditions of physical and material security are met, the political community should structure its institutions and policies to promote virtue in its citizens; and its worth as a community is contingent on the extent to which it achieves that goal.

Aristotle was under no illusion that the communities of his day were actually organized in pursuit of virtue. Some, like Sparta, were devoted to military victory, others to commercial prosperity; most had no single discernible goal. Each nonetheless had a largely tacit, operative conception of the virtuous individual as the good citizen whose character and conduct were most conducive to the preservation of the community and of its way of life. In this understanding, the relation of politics and virtue are reversed: virtue becomes the means, and the political community provides the end.

This reversal gave rise to the question explored by Aristotle in book 3 of the *Politics* (1984): Are the virtues of the good human being and of the good citizen identical or different? It turns out that they are nearly always different, a conclusion that generates a double dilemma. If a community is notably imperfect, the citizen who shapes himself in its image and devotes himself to its service will undergo a kind of moral deformation. Conversely, the virtues of the good human being simpliciter may not only not promote but may actually impede the activities of that person's particular community.

Liberal theorists were not unaware of this dilemma, and they responded to it in two very different ways. Some, such as John Stuart Mill, retained a place for the Aristotelian conception of virtue as an intrinsic good but argued that the practice of virtue, so understood, would also be

supportive of the liberal polity. In a liberal order, the same virtues are both ends and means: the good citizen and the good human being are identical.

The other liberal strategy was to cut the knot by denying the very existence of intrinsic virtue, that is, by reinterpreting virtue as purely instrumental to the non-moral goods that constitute the true ends of liberal politics. Thus Hobbes: "All men agree on this, that peace is good, and therefore also [that] the way, or means, of peace, which . . . are justice, gratitude, modesty, equity, mercy, and the rest of the laws of nature, are good. . . . But the writers of moral philosophy, though they acknowledge the same virtues and vices, [do not see] wherein consisted their goodness; nor that they come to be praised as the means of peaceable, sociable, and comfortable living" (1962, 124).

During the past two decades, John Rawls has moved between the Hobbesian and Millian strategies. For Rawls, the ultimate justification—and overriding objective—of the liberal polity is the attainment of justice viewed not as an individual virtue but as a social state of affairs. Rawls rejects "perfectionism"—the thesis that society should be so arranged as to maximize the achievement of individual virtue or excellence. Yet justice as a virtue predicated of individuals does occupy a place within his overall theory. Individuals are presumed to have a capacity for a sense of justice, that is, the ability to accept and to act upon the agreed-on principles of social justice, which in turn supply the substantive content of individual justice. As this conception is developed in *A Theory of Justice*, the engendering of just individuals is not the goal of liberal society but rather the means to the preservation of that society. That is why each member of a well-ordered liberal society wants the others to have a developed sense of justice. And more broadly, Rawls declares in a Hobbesian spirit, "a good person has the features of moral

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character that it is rational [i.e., instrumentally rational] for members of a well-ordered society to want in their associates" (1971, 436–37). In his more recent work, however, he has placed increased emphasis on the development and exercise of "moral personality" as an intrinsic good or end in itself. The very practices that help sustain a just society also express our nature as free and equal rational beings who have realized their innate capacity for justice (Rawls 1980; for an analysis, see Galston 1982b).

In the liberal tradition, then, we find traces of both sides of the Aristotelian conception: virtue as end, and virtue as means. It would be surprising, however, if on closer inspection the liberal canon of the virtues turned out to mirror the classical enumeration. Indeed I shall argue (pace Hobbes) that it does not. The liberal virtues are not simply the classical virtues justified on a different basis. They are in important respects different virtues.

Instrumental Liberal Virtues

I begin by examining the liberal virtues understood instrumentally as means to the preservation of liberal societies and institutions. To fix terms, let me characterize the liberal polity as a community possessing to a high degree the following features: popular-constitutional government; a diverse society with a wide range of individual opportunities and choices; a predominantly market economy; and a substantial, strongly protected sphere of privacy and individual rights. And to avoid misunderstanding, let me briefly characterize the status of the propositions I am about to advance.

The discussion of the instrumental virtues is a catalog not of logical entailments within liberal theory but rather of empirical hypotheses concerning the relation between social institutions and individual character. I offer at most fragmentary evi-

dence in support of these hypotheses, an adequate test of which would require a far more systematic historical and comparative inquiry.

When I speak of certain virtues as instrumental to the preservation of liberal communities, I do not mean that every citizen must possess these virtues, but rather that most citizens must. The broad hypothesis is that as the proportion of nonvirtuous citizens increases significantly, the ability of liberal societies to function successfully will progressively diminish.

The fact (if it is a fact) that the instrumental virtues are socially functional does not mean that they are individually advantageous. To be sure there is some overlap between these two objectives. The liberal virtues demand less self-discipline and sacrifice than do the virtues of classical antiquity, of civic republicanism, or of Christianity, and the practice of many of these social virtues will simultaneously make it easier for individuals to succeed within liberal communities. Still, these virtues are not reducible to self-interest, even self-interest "rightly understood." Thus while the liberal virtues do not presuppose a specific moral psychology, they do at least imply the rejection of any comprehensive egoism.

General Virtues

Some of the virtues needed to sustain the liberal state are requisites of every political community. From time to time, each community must call upon its members to risk their lives in its defense. Courage—the willingness to fight and even die on behalf of one's country—is thus very widely honored even though there may be occasions on which the refusal to fight is fully justified.

In addition, every community creates a complex structure of laws and regulations in the expectation that they will be accepted as legitimate—hence binding—

without recourse to direct threats or sanctions. The net social value of a law is equal to the social benefits it engenders minus the social costs of enforcing it. As the individual propensity to obey the law diminishes, so does a society's ability to pursue collective goals through the law. Law-abidingness is therefore a core social virtue, in liberal communities and elsewhere. (This does not mean that disobedience is never justified, but only that a heavy burden of proof must be discharged by those who propose to violate the law.)

Finally, every society is constituted by certain core principles and sustained by its members' active belief in them. Conversely, every society is weakened by the diminution of its members' belief in its legitimacy. Loyalty—the developed capacity to understand, to accept, and to act on the core principles of one's society—is thus a fundamental virtue. And it is particularly important in liberal communities, which tend to be organized around abstract principles rather than shared ethnicity, nationality, or history.

Beyond the virtues needed to sustain all political communities are virtues specific to liberal communities—those required by the liberal spheres of society, economy, and polity.

Virtues of Liberal Society

A liberal society is characterized by two key features: individualism and diversity. To individualism corresponds the liberal virtue of independence—the disposition to care for and take responsibility for oneself and to avoid becoming needlessly dependent on others. Human beings are not born independent, nor do they attain independence through biological maturation alone. A growing body of evidence suggests that in a liberal society the family is the critical arena in which independence and a host of other virtues must be engendered. The weakening of families is thus

fraught with danger for liberal societies. In turn, strong families rest on specific virtues. Without fidelity, stable families cannot be maintained. Without a concern for children that extends well beyond the narrow boundaries of adult self-regard, parents will not effectively discharge their responsibility to help form secure, self-reliant young people. In short, the personal independence required for liberal social life rests on self-restraint and self-transcendence, the virtues of family solidarity.

I turn now from individualism to diversity, the second defining feature of liberal society. The maintenance of social diversity requires the virtue of tolerance. This virtue is widely thought to rest on the relativistic belief that every personal choice, every "life-plan," is equally good, hence beyond rational scrutiny and criticism. Nothing could be further from the truth. Tolerance is fully compatible with the proposition that some ways of life can be known to be superior to others. It rests, rather, on the conviction that the pursuit of the better course should be (and in many cases can only be) the consequence of education or persuasion rather than coercion. Indeed, tolerance may be defined as the ability to make this conviction effective as a maxim of personal conduct.

Virtues of the Liberal Economy

The liberal, market economy relies on two kinds of virtues—those required by different economic roles, and those required by liberal economic life taken as a whole. In a modern market economy, the basic roles are the entrepreneur and the organization employee. The entrepreneurial virtues form a familiar litany: imagination, initiative, drive, determination. The organizational virtues are very different from (and in some respects the reverse of) the entrepreneurial. They include such traits as punctuality, reliabil-

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ity, civility toward coworkers, and a willingness to work within established frameworks and tasks. As economic units evolve, one of the great management challenges is to adjust the mix of entrepreneurial and organizational practices. Sometimes this takes the form of an organizational displacement (or routinization) of entrepreneurial charisma, as in the ouster of Steven Jobs as head of Apple Computer. Sometimes it requires just the opposite, as when a large, stodgy organization replaces a centralized structure with semiautonomous units and loosens individual task and role definitions in an effort to encourage more entrepreneurial practices on the part of its employees.

There are three generic (as distinct from role-specific) virtues required by modern market economies. The first is the work ethic, which combines the sense of obligation to support personal independence through gainful effort with the determination to do one's job thoroughly and well. The second is the achievement of a mean between ascetic self-denial and untrammelled self-indulgence—call it a capacity for moderate delay of gratification; for while market economies rely on the liberation and multiplication of consumer desires, they cannot prosper in the long run without a certain level of saving, which rests on the ability to subordinate immediate gratification to longer-run self-interest.

The third generic economic virtue is adaptability. Modern market economies are characterized by rapid, sweeping changes that reconfigure organizations and occupations. Patterns of lifelong employment within a single task or organization—common for much of this century—are being displaced. Most individuals will change jobs several times during their working lives, moving into new occupations, new organizations, and even new sectors of the economy. To be sure, collective political action can help regulate the pace of change, ameliorate its

consequences, and share its costs. Still, domestic and international pressures combine to make the fact and basic direction of economic change irresistible. Thus the disposition to accept new tasks as challenges rather than threats and the ability to avoid defining personal identity and worth in reference to specific, fixed occupations are essential attributes of individuals and economies able to cope successfully with the demands of change.

Virtues of Liberal Politics

I come, finally, to the sphere of politics, which calls for virtues of both citizens and leaders.

Virtues of citizenship. Some generic citizen virtues have already been identified: courage, law-abidingness, loyalty. In addition to these are the citizen virtues specific to the liberal polity. Because a liberal order rests on individual rights, the liberal citizen must have the capacity to discern, and the restraint to respect, the rights of others. (Invasion of the rights of others is the form of *pleonexia* specific to liberal political life.) Because liberalism incorporates representative government, the liberal citizen must have the capacity to discern the talent and character of candidates vying for office, and to evaluate the performance of individuals who have attained office. Liberalism also envisions popular government responsive to the demands of its citizens. The greatest vices of popular governments are the propensity to gratify short-term desires at the expense of long-term interests and the inability to act on unpleasant truths about what must be done. To check these vices, liberal citizens must be moderate in their demands and self-disciplined enough to accept painful measures when they are necessary. From this standpoint, the willingness of liberal citizens to demand no more public services than their country can afford and to pay for all the benefits

they demand is not just a technical economic issue but a moral issue as well. Consistently unbalanced budgets—the systematic displacement of social costs to future generations—are signs of a citizenry unwilling to moderate its desires or to discharge its duties.

The liberal citizen is not the same as the civic-republican citizen. In a liberal polity there is no duty to participate actively in politics, no requirement to place the public above the private and to subordinate personal interest to the common good systematically, no commitment to accept collective determination of personal choices. But neither is liberal citizenship simply the pursuit of self-interest, individually or in factional collusion with others of like mind. Liberal citizenship has its own distinctive restraints—virtues that circumscribe and check, without wholly nullifying, the promptings of self-aggrandizement.

Virtues of leadership. The need for virtue and excellence in political leaders is perhaps more immediately evident than is the corresponding requirement in the case of citizens. The founders saw popular elections as the vehicle for discerning and selecting good leaders. Thomas Jefferson spoke for them when he wrote to John Adams that

there is a natural aristocracy among men. The grounds of this are virtue and talents. . . . The natural aristocracy I consider as the most precious gift of nature, for the instruction, trusts, and government of society. . . . May we not even say, that that form of government is the best, which provides the most effectively for a pure selection of these natural *aristoi* into the offices of government? . . . I think the best remedy is exactly that provided by all our constitutions, to leave to the citizens the free election and separation of the *aristoi* from the *pseudo-aristoi*. (Mason 1965, 385)

The leadership virtues specific to liberal polities include patience—the ability to accept, and work within, the constraints on action imposed by social diversity and

constitutional institutions. Second, the liberal leader must have the capacity to forge a sense of common purpose against the centrifugal tendencies of an individualistic and fragmented society. Third, the liberal leader must be able to resist the temptation to earn popularity by pandering to immoderate public demands. Against desire the liberal leader must counterpose restraints; against the fantasy of the free lunch he or she must insist on the reality of the hard choice; against the lure of the immediate he or she must insist on the requirements of the long term. Finally, while the liberal leader derives authority from popular consent, he or she cannot derive policy from public opinion. Rather, the leader must have the capacity to narrow—so far as public opinion permits—the gap between popular preference and wise action. The liberal leader who disregards public sentiment will quickly come to grief, but so will the leader who simply takes that sentiment as the polestar of public policy. Through persuasion, the liberal leader tries to move the citizenry toward sound views. But the limits of persuasion must constitute the boundaries of public action, or else leadership becomes usurpation.

As the authors of the *Federalist* insisted, and as experience confirms, there are also specific virtues required for the successful conduct of the different offices in a liberal-constitutional order: optimism and energy in the executive, deliberative excellence and civility in the legislator, impartiality and interpretive skill in the judge (for a parallel account, see Mansfield 1987, 60). And, as Jefferson suggested, the ultimate test of systems of election or appointment is their tendency to select officeholders with the appropriate virtues. For that reason, it is appropriate and necessary to inquire whether particular systems of selection (e.g., presidential nominating primaries) tend on balance to reward the kinds of personal traits that their corresponding offices require.

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General political virtues. There are two other political virtues required of liberal citizens and leaders alike. While not all public policies need be made in the full light of day, liberal politics rests on a presumption of publicity, that is, on a commitment to resolve disputes through open discussion unless compelling reasons can be adduced for restricting or concealing the policy process. Thus a general liberal political virtue is the disposition—and the developed capacity—to engage in public discourse. This includes the willingness to listen seriously to a range of views that, given the diversity of liberal societies, will include ideas the listener is bound to find strange and even obnoxious. The virtue of political discourse also includes the willingness to set forth one's own views intelligibly and candidly as the basis of a politics of persuasion rather than manipulation or coercion.

A second general political virtue is the disposition to narrow the gap (so far as is one's power) between principles and practices in liberal society. For leaders, this means admitting and confronting social imperfections through a public appeal to collective convictions. For citizens it can mean either such a public appeal or quiet acts that reduce the reach of hypocrisy in one's immediate community. For both it can lead to a tension between social transformation and law-abidingness, which can only be resolved prudentially with reference to the facts of specific cases. (This is a tension rather than contradiction between these two liberal virtues because the virtue of law-abidingness embodies not the absolute priority of the law but rather a presumption in favor of the law that can be rebutted in a narrow range of instances.)

Liberal Virtues and the Liberal Good

In the previous section I have tried to sketch a catalog of instrumental liberal

virtues understood as empirical hypotheses about connections between individual character and social institutions. There is another approach that yields complementary results. The analysis of liberal theory reveals certain broad structures—in particular, a liberal theory of the good—that can enlarge our understanding of the liberal virtues.

John Rawls has tried to rest liberal theory on a "thin" account of the good as means to the attainment of whatever objectives individuals may pursue. Many critics have wondered whether *any* means can really be comprehensively neutral in the Rawlsian sense, equally serviceable for each and every goal. But the deeper difficulty lies elsewhere. Rawls's theory presupposes individuals who value their earthly existence, who give positive weight to the achievement of their purposes, and who are prepared to accept rationality as a constraint on social action and principle. More generally, every contemporary liberal theory ends up relying, covertly or overtly, on this same triadic theory of the human good: existence, fulfillment, and social rationality (Galston 1982a).¹

This liberal theory of the noninstrumental good can serve as an independent basis for a range of instrumental liberal virtues. The worth of existence implies, first, severe strictures against cruelty and brutality as dispositions that tend to the needless destruction of existence; and second, an endorsement of the humanitarian disposition to act affirmatively to preserve the existence of others, at least when this can be done without excessive cost to ourselves. The value liberalism attaches to the fulfillment of individual purposes rests on a critique both of the otherworldliness² that denigrates earthly striving in light of the hereafter and of the nihilism that withdraws all moral significance from such striving in the name of the absurdity of existence. When combined with the empirical diversity of human purposes, the liberal account of

purposiveness leads directly to the vindication of tolerance, interpreted as the settled disposition of each individual to minimize interference with the strivings of others. Finally, the liberal endorsement of social rationality provides a basis for a distinctively liberal account of moderation, that is, a critique of passions and desires insofar as they weaken the disposition to confine the pursuit of individual purposes to the limits set by rational social principle.

Intrinsic Liberal Virtues

The thrust of the argument thus far has been to specify the virtues instrumental to the preservation and operation of the liberal polity. I turn now from considering liberal virtues as means to examining them as ends. The question is whether there is a conception of the virtuous or excellent individual linked intrinsically to liberal theory and seen as valuable, not instrumentally but for its own sake.

It might be thought that the answer must be negative. After all, it is characteristic of both liberal societies and liberal theories to be open to a wide variety of life plans and to their corresponding excellences. Yet the liberal tradition is by no means silent on this question. Indeed, it suggests three conceptions of intrinsic individual excellence, overlapping yet distinct.

The first is the Lockean conception of excellence as rational liberty or self-direction. As persuasively reconstructed by Rogers Smith, rational self-direction includes the capacity to form, pursue, and revise life plans in light of our personal commitments and circumstances. But it is a substantive, not merely instrumental standard: "If we value rational self-direction, we must always strive to maintain in ourselves, and to respect in others, these very capacities for deliberative self-guidance and self-control. Correspondingly, we must see the habitual exercise of

these capacities as constituting morally worthy character and their enhancement as constituting morally praiseworthy action" (1985, 200).

The second noninstrumental liberal conception of individual excellence is the Kantian account of the capacity to act in accordance with the precepts of duty, that is, to make duty the effective principle of personal conduct and to resist the promptings of passion and interest insofar as they are incompatible with this principle. Judith Shklar has offered a fine sketch of Kant's morally excellent individual in action:

At all times, he must respect humanity, the rational moral element in himself and in all other men. For his own sake, he must choose to avoid all self-destructive and gross behavior, and above all else, he must not lie. . . . To other men he owes no liberality or pity or *noblesse oblige* of any kind, because this might humiliate the recipients. What he does have to show them is a respect for their rights, decent manners, and an avoidance of calumny, pride, and malice. . . . This is a thoroughly democratic liberal character, built to preserve his own self-respect and that of others, neither demanding nor enduring servility. (1984, 233)

The third liberal conception of individual excellence was adapted in different ways from Romanticism by John Stuart Mill, Ralph Waldo Emerson, Henry David Thoreau, and Walt Whitman. It is the understanding of excellence as the full flowering of individuality. As Mill expounded this thesis, the excellence of individuality combines the Greek emphasis on the development, through activity, of human powers with the modern realization that the blend and balance of these powers will differ from individual to individual. Because liberal societies allow maximum scope for diversity, they are most conducive (though not perfectly so) to the development of individuality. And because liberal societies rest on individual freedom, they tend to foster the self-determination that is at the heart of true individuality. As Mill put it, "He who lets

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the world, or his own portion of it, choose his plan of life for him, has no need of any other faculty than the ape-like one of imitation. He who chooses his plan for himself, employs all his faculties" (1975, 56). Or as George Kateb sums up the parallel argument within the Emersonian tradition, "One must take responsibility for oneself—one's self must become a project, one must become the architect of one's soul. One's *dignity* resides in being, to some important degree, a person of one's own creating, making, choosing rather than in being merely a creature or a socially manufactured, conditioned, created thing" (Kateb 1984, 343).

Can these three distinct but recognizably liberal conceptions of human excellence be made to cohere in a single unified view? Yes, to a point. They have a common core—a vision of individuals who in some manner take responsibility for their own lives. Each links excellence to a kind of activity. And all lead to a vindication of the dignity of every individual and to the practice of mutual respect.

Beyond this common core, however, certain tensions become manifest. The exercise of Lockean rational liberty can lead to a wide range of deliberative outcomes, while Kantian duty usually prescribes a single course of conduct as generally binding. The pursuit of Emersonian individuality can tend toward a kind of poetic, even mystical self-transcendence at war with both the rationalism of Kantian morality and the prosaic, orderly self-discipline of Lockean liberty (see Rosenblum 1987, chap. 2). It is possible, of course, to resolve these tensions by giving one conception pride of place and requiring the others to maintain consistency with the preferred standard. (This is the course that Rawls—like Kant—follows when he subordinates the rational pursuit of individual life plans to the social requirements of moral right.) But it may be more advisable to accept a range

of tension and indeterminacy, that is, to see the liberal polity at its best as a community that encourages all of these overlapping but distinct conceptions of individual excellence and that provides an arena within which each may be realized, in part through struggle against the others.

Intrinsic versus Instrumental Liberal Virtues

The fact that liberalism has accommodated conceptions of intrinsic as well as instrumental virtue raises the question of whether these broad categories are mutually consistent—whether (in Aristotle's terms) the liberal "good man" is the same as the liberal "good citizen." In important respects, it turns out, they are not. As Kateb observes, the Emersonian individual sees laws and institutions—including liberal-democratic institutions—as constraints on true individuality even though these very institutions are the practical presuppositions for the emergence of the ideal self. On the Emersonian account, moreover, the virtues required to maintain the liberal polity fall far short of the highest human qualities (Kateb 1984, 356–57). Similarly, even though Kant himself prescribed law-abidingness as a moral duty, the possibility of laws that command immoral action and challenge self-respect is ever present. In cases of conflict the preservation of individual integrity and dignity would seem to require that positive law give way to moral law. Finally, the Lockean doctrine of rational self-direction stands in dual tension with the requirements of liberal citizenship. Self-direction, so understood, does not readily lead to, or easily cohere with, the willingness to die for one's country. And self-direction as a maxim of individual perfection is not obviously or always consistent with a maxim of social action, binding on each individual, that

commands the maximization of self-direction for all individuals throughout the community. While optimistic empirical assumptions may narrow the disjunction between the imperatives of personal perfection and social solidarity, this gap cannot be fully bridged. Indeed, it would not be farfetched to interpret some of the deepest tensions within liberal polities as a clash between means and ends, that is, between requirements of liberal citizenship and aspirations toward liberal excellence.

Engendering Liberal Virtues

It has been the burden of this article to argue that the health of liberal polities is intertwined in complex ways with the practice of what I have called liberal virtues. Now assuming that these virtues are not innate, by what means are they to be engendered? And are they adequately developed in our own liberal community? A full examination of these issues lies well beyond the scope of the present discussion. But let me conclude with three alternative hypotheses.

The optimistic hypothesis claims that daily life in the liberal polity is a powerful if tacit force for habituation to at least the minimal requirements of liberal virtue. The sorts of things regularly expected of us at home, in school, and on the job shape us in the manner required for the operation of liberal institutions. And while hardly models of moral perfection, citizens of modern liberal communities are at least adequately virtuous and not demonstrably less so than were citizens in times past.

The neutral hypothesis maintains that tacit socialization is not enough; that authoritative institutions such as families, schools, churches, the legal system, political leaders, and the media must deliberately and cooperatively foster liberal virtues; that these institutions are not now

performing this task adequately; but that there is no reason in principle why they cannot do so once they come more fully to understand their responsibility.

Finally, the pessimistic hypothesis—associated with such thinkers as Daniel Bell—suggests that powerful strands of contemporary liberal culture tend to undermine liberal virtues, that in particular the various forms of liberal self-restraint have fallen victim to the imperatives of self-indulgence and self-expression (Bell 1976). From this perspective, the task of strengthening and renewing liberal virtues requires more than improving the formal institutions of moral and civic education. It requires as well a sustained effort to reverse corrosive tendencies fundamental to modern culture.

I do not know which of these hypotheses is most nearly correct. Optimists can point to the demonstrable fact that our polity has generated—or at least has not thwarted the generation of—the minimal conditions for its survival over the past two centuries. Neutralists can argue that such key social indicators as crime, drugs, family stability, and others are headed in alarming directions and that the impact of major forces of socialization—in particular, television and popular culture—is on balance negative. And pessimists can observe that with the important exception of organized religion, most sources of social authority have a diminished confidence in their ability to establish, inculcate, and enforce social norms of conduct and character.

In sorting through these competing perspectives, informal observation is no substitute for systematic, theory-guided empirical research. I cannot imagine a line of inquiry of greater significance for the conduct—and the fate—of liberal societies.

Note

1. "Any *workable* political conception of justice that is to serve as a public basis of justification that

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citizens may reasonably be expected to acknowledge must count human life and the fulfillment of basic human needs and purposes as in general good, and endorse rationality as a basic principle of political and social organization. A political doctrine for a democratic society may safely assume, then, that all participants in political discussion of questions of right and justice accept these values, when understood in a suitably general way. Indeed, if the members of society did not do so, the problems of political justice, in the form with which we are familiar with them, would seem not to arise" (Rawls 1987, 4). I explore this acknowledgement's important implications for the structure of Rawls's theory and for liberal theory as a whole in Galston n.d.

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RESEARCH NOTES

UPSETTING NATIONAL PRIORITIES? THE REAGAN ADMINISTRATION'S BUDGETARY STRATEGY

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We use simulations based on a multiequation model of federal budgetary outcomes to assess the Reagan administration's impact on the federal budget during fiscal years 1982–86. Reagan's aggregate budget priorities represent a significant departure from the priorities of prior postwar administrations. The bulk of this shift in priorities had occurred by fiscal 1984. Defense spending and uncontrollable domestic spending were higher, and spending on domestic controllable programs lower under Reagan than they would otherwise have been. The distinctiveness of Reagan's budgetary priorities can be attributed to his tax cuts and—far from a strategy of “starving the budget by reducing revenues”—to a failure to allow fiscal pressures to restrain spending. The model projects that without tax cuts Reagan's predecessors would have spent no less on defense than Reagan. Cutting taxes increased the deficit by about \$400 billion cumulatively during fiscal years 1982–86 and reduced expenditures by roughly \$30 billion.

One of the most important goals of the Reagan administration upon its accession to power was a reorientation of U.S. budgetary, fiscal, and tax policies. The Reagan administration yielded to no one in 1981 in describing its approach to budgetary and economic policy as a break with the economic and political priorities of previous administrations.¹ The key components of this new approach were increases in defense spending, cuts in domestic spending, and cuts in taxes. In 1981, Congress approved a budget that incorporated these three components, and the Reagan “revolution” appeared to be off to a fast start.

Even as the administration won congressional approval of its budgetary and tax policies, however, critics argued that the Reagan budgetary, tax, and fiscal policies were inconsistent. How could defense spending be increased, the social safety net for the poor kept intact, major “middle-class entitlements” left untouched, and taxes cut, without creating huge deficits for the foreseeable future? The administration had two responses to this question. The first was based on the timely economic theory of “supply side economics,” which seemed to show that tax cuts and investment incentives would produce rapid economic growth in the short run, increasing revenues and balan-

cing the budget. Without debating the long-run benefits to economic prosperity of incentives for investment, it can be safely stated that the projected short-run growth in federal revenues did not materialize.²

A second basis for the Reagan administration strategy was the view that revenue cuts would lead to budgetary reductions. According to this "tax-spend" theory of the state, the level of available revenues drives spending; if available revenues are reduced, government spending must follow. As Meyer (1986) writes: "The government has brought pressure to bear on total outlays through the unconventional approach of scaling back available revenue. . . . Although President Reagan's strategy of pulling the revenue plug first is risky, it is better than the hopeless program-by-program fights with each constituency group against a back-drop of revenue flows that make government concessions too easy" (p. 87).³

We examine two questions that are central to an evaluation of the Reagan administration's budgetary strategy. First, to what degree did the Reagan administration's budgetary, tax, and fiscal policy priorities diverge from those of past administrations? Second, what evidence supports the theory behind Reagan's attempt to "starve the budget" by cutting revenues, and how successful was Reagan's application of this strategy?

Our analysis employs a model of executive and congressional budgetary decision making during fiscal years 1955-81 taken from Kamlet and Mowery 1987 to project spending levels for the fiscal 1982-86 period. Spending projections were developed under different scenarios concerning revenue policy. For each scenario, we projected spending outcomes for different budgetary categories (defense, "uncontrollable" domestic spending, and "controllable" domestic spending) for each year of the Reagan administration. We also analyzed the tem-

poral stability of the Kamlet-Mowery model by reestimating the model leaving out, one at a time, each postwar administration since Eisenhower. The reestimated model was used to simulate spending levels for the omitted administration, allowing us to assess the robustness of the consensus concerning aggregate budgetary and fiscal priorities in the pre-Reagan period and the extent to which the Reagan administration altered these priorities.

Prior Work on the Impact of Reagan on the Budget

The budgetary priorities and programs of the Reagan administration have been scrutinized in a flood of scholarly and journalistic commentary and analysis. Many accounts describe the budgetary priorities of this administration as a significant departure from those of previous administrations. Nathan (1983) concluded that "The Reagan presidency . . . has already marked a watershed in domestic affairs. The long cycle of growth in the role and activism of the national government in domestic affairs that began with FDR's New Deal ended with Reagan's New Federalism" (p. 49). Weicher (1986) analyzed Reagan's impact on entitlement spending and concluded that "the rapid real growth of means-tested benefit programs over the previous two decades has almost been brought to a halt" (p. 7). Russett's (1982) analysis of the relationship between defense and non-defense (health and education) expenditures found that in contrast to his Republican predecessors, who promoted health and education spending as much as Democrats, Reagan cut health and education spending at the same time he expanded defense. Russett concluded that "the Reagan presidency is different" (p. 776). Butler (1985) and Mills (1984) independently attribute an increase in defense spending of approximately 1% of GNP during fiscal years 1981-85 to Reagan's in-

fluence. Mills (1984) further contended during 1981-85, nondefense spending was reduced by 1.5% of GNP below pre-Reagan policy base lines.

Other evidence, however, suggests that Reagan has not transformed national spending priorities. A study by the Congressional Budget Office (1985) concluded that the budgetary policies in effect when Reagan was elected would by fiscal 1985 have held total outlays below their actual levels by \$18 billion.⁴ Several scholars have argued that the buildup in defense predates Reagan (Henry and Oliver 1987; Huntington 1983) or represents a shift that would have occurred, at least to some degree, under other administrations (Glazer 1984; Mills 1984; Ostrom and Marra 1986).⁵ Other analyses of domestic spending claim that cutbacks in spending on programs for the low-income population predate Reagan. Nathan (1983) argues that "retrenchment had been gaining strength in domestic policy since the mid-seventies" (p. 48), as does Sawhill (1986).

Still other evidence supports skepticism about the magnitude of the shift in spending priorities under Reagan, especially in domestic programs. Total nondefense spending, for instance, has not declined under the Reagan administration, measured either in real terms or as a percentage of the GNP. Indeed, Butler (1985) found that transfer payments to individuals rose as a percentage of GNP during the first part of the Reagan administration. By 1985, the architect of the Reagan budgetary policy, David Stockman, characterized this policy as a failure (Stockman 1986), due to the inability of the administration to cut domestic spending.

Other analysts have questioned the novelty of Reagan's tax policy. Galbraith (1986) argues that the 1981 tax cut represents a change in the timing, rather than the substance, of the revenue strategies of prior administrations: "Up until the third

quarter of 1983 the deficit was very near what would have been expected from the behavior of past administrations confronted with the same economic conditions that Reagan faced" (p. 21). According to Galbraith, previous administrations would have responded to the 1981-82 recession with a tax cut comparable in size to that in 1981, although the tax reductions that took effect in mid-1983 were greater than predicted from data on previous administrations. In this view, Reagan's only significant departure from the postwar pattern of revenue policy was in the second- and third-year tax reductions contained in the 1981 bill.

Clearly, there remains substantial controversy about the actual impact of the Reagan administration on budgetary and fiscal priorities. In part, this reflects the fact that most empirical analyses have examined the various components of federal spending and revenue policy in isolation from one another and have not compared the Reagan budgetary and tax policies against a "counterfactual" model of budgetary and fiscal outcomes. A thorough assessment of Reagan's impact on the budget must analyze the joint effects of Reagan spending and tax policies and must specify a clear counterfactual scenario in doing so.

The Basic Model

The basic model in Kamlet and Mowery 1987 can be used to assess Reagan's effect on total spending and taxes and impact on spending priorities by comparing his policies with those that would have been observed had the budgetary priorities of past administrations been in effect during the past six years. The Kamlet-Mowery model is a multiple-equation, reduced form model of budgetary outcomes that examines three categories of expenditures: defense outlays, "controllable" domestic outlays, and "uncon-

trollable" domestic outlays (excluding interest payments).⁶ A fourth category of expenditures—interest payments—is embedded in the model but is treated as exogenous. Each equation in the Kamlet-Mowery model allows for three types of influences on spending: influences that are specific to the spending category being examined (in the case of defense spending, for instance, these influences included the Soviet threat and the level of armed conflict); total revenues; and spending pressures in other components of the budget. The model treats revenues as unresponsive to spending pressures, at least in the short run, a feature that merits additional discussion.

The Kamlet and Mowery model allows revenue and fiscal pressures to influence spending outcomes but assumes that spending pressures do not affect revenues. The parameter estimates of the model are not strongly affected by this assumption, however.⁷ Similar assumptions have been employed in other recent papers (Auten, Bozeman, and Cline 1984; Fischer and Crecine 1981; Fischer and Kamlet 1984).⁸

Empirical attempts to test the causal relationship between spending and taxation have yielded mixed results (see, e.g., Anderson, Wallace, and Warner 1986; Blackley 1986; Furstenberg, Green, and Jeong 1985, 1986; and Congressional Budget Office 1987, Manag and Marlow 1986, Vedder, Gallaway, and Frenze 1987). This is not surprising, given the small number of data points in studies of year-to-year spending and revenue trends and the methodological difficulties in determining the existence and direction of a causal link between these two variables. Kamlet, Mowery, and Su (1988) assess the robustness of these empirical studies, applying univariate and multivariate Granger tests to alternative specifications for the 1953–80 period. They conclude that with a few exceptions (e.g., the 1969 Vietnam surtax), U.S. tax policies were independent of spending pressures during

the postwar period prior to Reagan but that spending was heavily affected by revenue availability. If the assumption that revenues affect spending and not the reverse is incorrect, our model will overstate the influence of revenues on spending. Since the Reagan administration's budgetary strategy assumes the existence of a close link between revenue and spending decisions, the model will err in the direction of supporting the validity of the Reagan budgetary strategy.

Kamlet and Mowery (1987) provide a detailed discussion of the model and estimates of its parameters for fiscal years 1955–81.⁹ Several of the estimation results are of particular interest for this analysis. First, the relatively high goodness of fit of the estimating equations for the entire period, as well as the lack of statistical significance of administration-specific dummy variables, suggests that aggregate budgetary priorities were relatively stable from Eisenhower through Carter, although both entitlement spending and the size of the deficit grew steadily during this period. Traditional political party variables had little impact on these budgetary priorities in either the White House or Congress. These results support the view that there was a general consensus on aggregate budgetary and fiscal policy priorities during 1955–81 that spanned Republican and Democratic presidencies. A final conclusion of relevance for this work is that the revenue environment and spending pressures in other spending programs constrain growth in the defense budget but exert much less influence on domestic controllable and uncontrollable spending.

The Simulations

A Simulation Assuming the Reagan Tax Changes

The parameter estimates from the model for fiscal years 1955–81 were used

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to simulate the budget for fiscal years 1982-86. The results of the simulation represent budgetary priorities that would have been obtained for fiscal years 1982-86 if these priorities and interdependencies had remained unchanged from those of the 1955-81 period; that is, the simulation assumes a "composite" president and Congress based on those of the 1955-81 period and projects budgetary outcomes under such a president and Congress during 1982-86. In each case, the dependent variable is outlays after congressional action on the budget.

The first simulation is reported in the second column of Table 1. It is a "successive" simulation. Budgetary outcomes first are projected for fiscal 1982. These simulated values for fiscal 1982 are used to simulate fiscal 1983, and the procedure is extended through fiscal 1986. Because the basic model takes interest payments as exogenous, simulated and actual interest

payments are identical. This first simulation also employs actual 1982-86 federal revenues. It therefore reflects the tax changes that took place during the Reagan administration. The projected values are expressed both in dollar terms and as a percentage of GNP. The first simulation uses as macroeconomic variables (inflation and unemployment) the actual economic conditions during 1982-86.¹⁰

The simulation is subject to three sources of uncertainty. First, each equation in the Kamlet-Mowery model has an additive error term. Second, the parameters from the 1955-81 period are themselves only estimates and contain error. Third, in a successive simulation of an equation system like that employed here, some of the predetermined variables are simulated (e.g., prior year outlay levels), adding an additional stochastic component. It is generally impossible to derive analytically tractable expressions for

Table 1. Simulation Results

Fiscal Year	Simulation 1	Simulation 2	Actual Outlays ^a
Defense Outlays			
81	—	—	158 (.052)
82	159 (.050) [.042, .058]*	174 (.055) [.047, .063]	183 (.058)
83	171 (.050) [.038, .061]*	204 (.060) [.047, .073]	209 (.061)
84	161 (.043) [.031, .053]*	209 (.056) [.042, .068]	231 (.061)
85	174 (.044) [.033, .053]*	233 (.058) [.042, .069]	246 (.062)
86	203 (.048) [.035, .060]*	273 (.065) [.051, .078]	258 (.061)
Controllable Domestic Outlays			
81	—	—	146 (.048)
82	156 (.049) [.040, .059]	157 (.050) [.040, .060]	133 (.048)
83	184 (.054) [.042, .067]*	186 (.055) [.041, .069]*	132 (.039)
84	197 (.052) [.040, .064]*	199 (.053) [.040, .065]*	146 (.039)
85	209 (.052) [.042, .063]*	210 (.053) [.040, .066]	168 (.042)
86	229 (.054) [.042, .068]*	228 (.054) [.040, .070]*	160 (.038)
Uncontrollable Outlays			
81	—	—	307 (.101)
82	329 (.104) [.096, .112]	330 (.104) [.095, .112]	342 (.108)
83	366 (.107) [.096, .119]	367 (.108) [.094, .119]	395 (.116)
84	389 (.103) [.093, .113]	390 (.103) [.089, .116]	386 (.102)
85	406 (.102) [.090, .111]*	405 (.101) [.088, .111]*	447 (.112)
86	425 (.101) [.089, .112]	422 (.100) [.085, .112]	456 (.108)

Table 1. (continued)

Fiscal Year	Simulation 1	Simulation 2	Actual Outlays ^a
Interest			
81	—	—	80 (.026)
82	99 (.031)	99 (.031)	99 (.031)
83	105 (.031)	105 (.031)	105 (.031)
84	128 (.034)	128 (.034)	128 (.034)
85	156 (.039)	156 (.039)	156 (.039)
86	172 (.041)	172 (.041)	172 (.041)
Total Outlays ^b			
81	—	—	691 (.226)
82	742 (.235)	760 (.240)	757 (.239)
83	826 (.243)	862 (.253)	841 (.247)
84	875 (.232)	926 (.245)	891 (.236)
85	945 (.237)	1,003 (.251)	1,017 (.255)
86	1,029 (.244)	1,095 (.260)	1,046 (.248)
Total Revenues ^c			
81	—	—	636 (.208)
82	659 (.208)	700 (.221)	659 (.208)
83	633 (.186)	708 (.208)	633 (.186)
84	707 (.187)	806 (.214)	707 (.187)
85	795 (.199)	906 (.227)	795 (.199)
86	837 (.198)	967 (.229)	837 (.198)
Deficit			
81	—	—	55 (.018)
82	83 (.026)	60 (.019)	98 (.031)
83	193 (.057)	154 (.045)	208 (.061)
84	168 (.045)	120 (.032)	184 (.049)
85	150 (.038)	97 (.024)	222 (.065)
86	192 (.046)	128 (.030)	209 (.050)

Note: The first figure in each row represents outlays in billions of dollars. Figures in parentheses are outlays as a share of GNP. Figures in brackets reflect 95% confidence interval for share of GNP.

^a Outlays after congressional action on the budget.

^b Total outlays as reported here equal "total outlays" as listed in the *Budget of the United States Government* apart from undistributed offsetting receipts.

^c Total revenues as reported here equal "total receipts" as listed in the *Budget of the United States Government* plus undistributed offsetting receipts.

simulation confidence intervals for equation systems estimated sequentially and containing simulated predetermined variables.

These difficulties were overcome through the use of Monte Carlo methods to generate confidence intervals for the simulation results. We specified a joint

distribution for the parameters of each equation in the basic model using the parameters' variance-covariance matrix from the 1955-81 estimation. Similarly, we specified a distribution for the additive error term of each equation using the estimated variance of each error term from the 1955-81 estimation. Using these

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distributions, a set of parameter values and additive error terms were drawn at random for each equation and used in a deterministic successive simulation. For each year of the simulation, a new additive error term was drawn for each equation. A total of one thousand such simulations were run for each budget category, with a 95% confidence interval constructed for each year and each budgetary category.¹¹

The results in Table 1 show that defense spending under Reagan substantially exceeds the levels projected by the simulation. In each year this gap is statistically significant; that is, given prevailing revenues, defense outlays in fiscal 1986 substantially exceeded the level that would have been achieved by this fiscal year had the pre-Reagan budgetary and fiscal priorities applied. By fiscal 1986, actual defense spending was \$55-billion greater than projected by the simulation, an increase in defense spending of nearly 25%.¹² Interestingly, this excess of actual over predicted defense spending was attained in its entirety by fiscal 1984.

Controllable domestic spending under Reagan is lower than would otherwise have been the case. The difference is statistically significant for four of the five years.¹³ The difference for fiscal 1986 between actual and simulated outlays is \$69 billion, or nearly 33% of predicted controllable domestic outlays. As with defense spending, most of the gap between predicted and actual domestic controllable spending emerged by fiscal 1984.

Surprisingly, uncontrollable spending (excluding interest) under Reagan exceeds the level projected by the simulation for four of the five years analyzed. The difference is statistically significant for only one year. The sum of uncontrollable spending for fiscal years 1982-86 as a proportion of GNP, .5464, however, exceeds the 95% confidence level for this sum (.4918, .5380). In 1986, actual uncontrollable outlays exceed predicted levels by \$31

billion, or over 5% of actual uncontrollable spending. The results of this simulation suggest that the Reagan administration's spending policies served to reinforce one of the central federal budgetary trends of the past three decades: growth in uncontrollable spending. More disaggregated analyses of the changing structure of domestic uncontrollable spending during the Reagan years (see Boskin 1987; Meyer 1986; Mills 1984) suggest that the administration's unwillingness or inability to impose greater cuts in so-called "middle-class" entitlements (Social Security, Medicare, veterans' pensions), coupled with congressional resistance to even more stringent reductions in entitlements for the poor and working poor (AFDC, food stamps), supported continued growth in domestic uncontrollable spending above the levels permitted by the spending and fiscal priorities of Reagan's predecessors.¹⁴

Total spending under Reagan is \$17-billion higher than the level projected by the simulation for fiscal 1986, consistent with the findings of the Congressional Budget Office study (1985). A similar conclusion applies to the deficit. Under the first simulation the fiscal 1986 deficit is \$192 billion, compared to the actual fiscal 1986 deficit of \$209 billion. The simulation suggests that tax cuts of the size passed during the Reagan administration would under other administrations have produced *greater* reductions in total spending than was true under Reagan. Had the Reagan administration followed the spending priorities of its predecessors, military and domestic uncontrollable spending would have been cut more severely in response to the reductions in taxes, while domestic controllable spending would have been reduced by a smaller amount.

Moreover, since lower deficits would reduce interest payments, the simulated total overstates total outlays. The model used for the simulations takes interest

payments as exogenous and equal to their actual levels during 1982-86. Budget deficits in the simulations, however, are lower than the actual deficits. Assuming an average interest rate on government debt of 6% during 1981-86 and compounding, interest payments in fiscal 1986 would be lower than their actual level by about \$8 billion. The simulated level of total spending in fiscal 1986 thus would be smaller than actual spending by more than \$25 billion.

A Counterfactual Simulation Assuming No Reagan Tax Changes

The results of an alternative simulation are also reported in the third column of Table 1. This second simulation employs Congressional Budget Office projections of the level of revenues that would have prevailed during 1982-86, given actual macroeconomic experience, in the absence of the fiscal 1981 tax cuts. This simulation depicts spending outcomes if the spending priorities of prior presidents had been applied without the Reagan tax cuts.

The second simulation yields conclusions concerning the impact of Reagan on uncontrollable and controllable domestic spending that are similar to those of the first simulation. Uncontrollable spending is higher, and domestic controllable spending lower under Reagan than would have been true under his predecessors, given similar revenue environment. The conclusions of the two simulations differ, however, for defense spending. The second simulation predicts that defense spending would have *exceeded* its actual 1986 level had Reagan not cut taxes. The simulated levels of defense spending for fiscal 1986 under the second simulation is \$273 billion, \$15-billion greater than the actual level of \$258 billion.¹⁵

The explanation for the contrasting results of these simulations concerning defense spending is straightforward. Prior

to Reagan, defense was more responsive to the revenue environment and to spending pressures from other parts of the budget than were other budgetary components. Defense spending was relatively sensitive to "top-down" spending pressures and to fiscal policy concerns. The less stringent revenue environment in this second simulation could support faster growth in defense spending during fiscal years 1981-86 than was observed under Reagan.¹⁶

Total spending under the second simulation exceeds that of fiscal 1986 by \$49 billion (about 5% of total budgetary outlays, over 1% of GNP). Thus, by cutting taxes and "starving the budget," Reagan shrank total spending by about \$50 billion by fiscal 1986—or by \$94 billion for the entire 1982-86 period—below the levels that would have prevailed under his predecessors. These spending reductions, however, were far too small to offset the impact on the deficit of the 1981 tax cut. The fiscal 1986 deficit predicted by the second simulation is \$128 billion, well below the actual fiscal 1986 deficit of \$209 billion. The cumulative amount by which the actual deficit exceeds that predicted by the simulation for fiscal years 1982-86 is \$362 billion. Moreover, when the comparison is adjusted to incorporate higher interest outlays (more than \$22 billion by fiscal 1986), Reagan's actual spending cuts shrink to \$28 billion for fiscal 1986 and the gap between the simulated and actual deficit expands to \$103 billion for fiscal 1986 and to a cumulative total of \$398 billion for fiscal years 1982-86.

The results from the second simulation are largely consistent with the conclusions from a comparison of the Reagan budget with the long-run projections (fiscal years 1983-86) in President Carter's fiscal 1982 budget, which of course did not assume tax cuts of the size of those of 1981 (Office of Management and Budget 1981a). Defense spending was projected by Carter to

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Table 2. Cumulative Residuals

Administration	Defense	Controllable	Uncontrollable
With Actual Tax Changes			
Eisenhower	.0035	-.0140	-.0075
Kennedy	.0041	-.0027	.0018
Johnson	-.0085	.0038	.0102
Nixon	-.0025	-.0081	.0261
Ford	-.0058	.0050	-.0054
Carter	-.0022	.0032	-.0182
Reagan	.0130	-.0163	.0072
With No Tax Changes			
Eisenhower	.0041	-.0098	-.0089
Kennedy	.0060	-.0035	.0016
Johnson	-.0113	.0053	.0130
Nixon	-.0085	-.0050	.0216
Ford	.0002	.0003	-.0039
Carter	.0031	.0030	-.0168
Reagan	-.0035	-.0162	.0080

Note: Residual = actual - projected (measured as a share of GNP).

be 5.9% of GNP in fiscal 1986, nearly as high as the actual level of 6.1% in the fiscal 1986 budget after congressional action. Carter projected uncontrollable spending to equal 9.1% of GNP by 1986, compared to 10.8% in the fiscal 1986 budget after congressional action. Controllable spending was projected by Carter to be 4.6% of GNP by 1986, compared to 3.8% in the fiscal 1986 budget after congressional action. As in the second simulation analysis, Carter's projections for fiscal 1986 produce a budget with defense spending that is comparable to the Reagan defense budget, less uncontrollable spending than was contained in the Reagan budget, and more controllable spending.

Is Reagan the Only Outlier?

The simulation analysis suggests that aggregate budgetary priorities during the Reagan administration differ substantially from the "composite president and Con-

gress" of the Eisenhower-to-Carter period, particularly when the 1981 tax cuts are included. This analysis, however, does not assess the extent to which the Reagan administration's departure from previous norms is unique. Have other postwar administrations departed from historical patterns to a comparable extent, albeit in other directions?

We analyzed this question by performing simulations for presidencies other than that of Reagan. For each presidency from Eisenhower to Carter, the basic model was estimated using data from the other administrations (including the Reagan administration). The resulting parameter estimates were then used to generate successive simulations for each year and each category for each administration. These results are summarized in Table 2, which reports the cumulative residual, i.e., the difference between actual and predicted values at the end of each administration, for each budgetary category. These residuals, measured as a share of GNP, provide a measure of the

extent to which each administration departed from the priorities of other administrations.

The first half of Table 2 presents residuals calculated for the actual 1982-86 tax regime. The residual during the Reagan years for defense is by far the largest for that category.¹⁷ The average absolute value of the residual for defense from Eisenhower through Carter is .44% of GNP, compared to a positive 1.30% for Reagan. The Reagan residual for controllable domestic spending also has the largest absolute value. The average absolute value of the residual for Eisenhower through Carter for controllable domestic spending is .61% of GNP, considerably below its (negative) value of 1.63% for Reagan. The absolute value of the Reagan residual for uncontrollable spending, however, is less than those of four other administrations, two of which, Johnson and Nixon, have a positive value. The average absolute value of the residual for Eisenhower through Carter is 1.15% of GNP, compared to a positive value of .72% for Reagan. The analysis in the top of the table indicates that Reagan altered budgetary priorities to a substantially greater degree in defense and controllable domestic spending than past administrations but also suggests that he was much less unusual with regard to uncontrollable spending.

The bottom panel of Table 2 presents residuals calculated from simulations that assume no tax changes under Reagan. These results contrast sharply with those in the top panel. When the revenue environment of the Reagan years is constrained to resemble that of previous presidents, the size of the Reagan residual for defense spending is not exceptional, declining from 1.30% of GNP in the top to -.35% of GNP in the bottom. Consistent with our discussion above, the distinctive feature of the Reagan military buildup is the fact that it occurred in the context of large tax cuts and budgetary deficits. If

taxes had not been cut, this analysis suggests that past administrations would have instituted a defense buildup of comparable scale. Analysis of the residuals for uncontrollable spending and controllable domestic spending yields results that are similar to those in the top panel of the table. Reagan's cuts in controllable domestic spending stand out, although the increases in uncontrollable spending conform broadly with the budgetary behavior of prior administrations.

This analysis of simulations for each administration yields two important conclusions. Aggregate budgetary priorities in the United States appear to have been relatively stable during the postwar period before Reagan. The average absolute value of the residuals in the bottom of Table 2 for Eisenhower through Carter are .55% of GNP, 1.10% of GNP, and .45% of GNP for defense, uncontrollable, and controllable domestic spending, respectively. These figures, in turn, represent 11%, 11%, and 9% of total spending in these categories (based on fiscal 1981 spending levels). Averaged across the tenure of each administration from Eisenhower to Carter, budgetary outcomes deviated from the levels predicted by the simulation by roughly 2% per year in any single budgetary category.¹⁸ The ability to shift spending 2% per year within aggregate budgetary categories suggests some degree of discretion but hardly is evidence of radical shifts in aggregate budgetary priorities in the postwar period prior to Reagan.

Second, consistent with Russett 1982, this analysis suggests that Reagan is different. The deviations of budgetary outcomes from predicted levels under the Reagan administration are substantially larger than those of prior presidencies, with a buildup in defense and uncontrollable spending and cuts in controllable domestic spending. The increases in defense spending, however, are unusual mainly because they occurred simultane-

ously with large tax cuts, rather than representing increases that were unprecedented in absolute magnitude or greater than those expected in a different revenue environment.

Presidential Impacts and Congressional Impacts

Faced with unprecedented deficits during his administration, Reagan has frequently argued that Congress has refused to go along with the budgetary priorities reflected in the president's budget. We have examined actual and predicted outlay levels that incorporate the effects of congressional action on the budget. The dependent variable thus incorporates the budgetary impacts of both the executive branch and Congress and does not separate the impact of the executive branch on the budget from that of Congress.

Although a large body of work in political economy, particularly the analysis of "political business cycles," has attributed budgetary outcomes solely to the White House, both the president and Congress affect budgetary outcomes, and the influence of each branch on outcomes is difficult to separate. In past work (Fischer and Kamlet 1984; Kamlet and Mowery 1987), executive branch action was assumed to be captured in the president's budget proposal. Unfortunately, this assumption yields imperfect measures of presidential intentions or ultimate presidential impact. The executive branch obviously influences the budget after its submission to Congress. Particularly under Reagan, the president's budget has sometimes been the first play of a multi-stage game, a document that establishes an initial bargaining position with Congress or yields favorable publicity with

the electorate. As such, the policy positions in the president's budget may be more extreme than "true" presidential policy preferences. Conversely, anticipated congressional actions may temper presidential requests, leading to changes in the president's budget that accommodate congressional goals, rather than expressing those of the White House.

Despite these difficulties of measurement, an analysis of the president's budget requests provides one means for analyzing the budgetary dimensions of presidential interactions with Congress. Kamlet and Mowery (1987) estimate a version of their model using outlays levels in the president's budget as the dependent variable. This model of the president's budget proposals can be used to generate simulations like the ones presented.

The Kamlet and Mowery (1987) model was used to simulate the president's budget for each year and budget category, assuming the tax changes that actually occurred under Reagan. This was not done sequentially, as were the other simulations, but separately for each year, reinitializing each year based on actual congressional action from the previous fiscal year.¹⁹ We ran a similar simulation for outlays after congressional action.

For 1982-86, defense spending in the president's budget exceeded predicted levels by an average amount of \$34 billion. The defense budget after congressional action was on average \$39-billion greater than predicted by the simulation of the budget after congressional action. Thus, during fiscal years 1982-86, defense spending after congressional action exceeded historical norms by as much as or more than defense spending proposed by the president. The president's budget for uncontrollable spending was on average \$8-billion greater than its simulated value, while uncontrollable spending after congressional action was \$13-billion greater than the simulated value. In other words, uncontrollable

spending after congressional action was slightly further from historical norms than was the president's budget. Finally, controllable spending was \$19-billion less on average in the president's budget than in the simulation of president's budgets, while controllable spending was \$29-billion less on average after congressional action than in the congressional action simulation. Measured relative to historical spending norms, congressional decisions on controllable spending represent deviations that are even greater in magnitude than those requested by the White House. Rather than offsetting presidential budget requests, Congress appears to have accommodated White House budget requests for controllable spending.

For defense and controllable domestic spending, outlays after congressional action deviated from simulated values by as much or more than the deviation in the president's budget. This is not to say that Congress always passed the budgetary allowances recommended by the president during this period. For instance, Congress cut Reagan's defense proposal by an average of \$11.8 billion during this period, increased controllable spending by \$14.4 billion on average, and increased uncontrollable spending by \$15.2 billion on average. Such congressional changes in the president's budget, however, are typical of executive-legislative budgetary interactions. The Reagan administration apparently convinced Congress to move its budgetary priorities as far from its pre-1981 priorities as Reagan's presidential budgets differed from pre-1981 president's budget priorities. The claims by Reagan that Congress is responsible for the deficit and for the shifts in the composition of the budget are not easy to square with this analysis. Congress has if anything been less assertive in revising presidential spending proposals during fiscal years 1982-86 than was true before Reagan.

Conclusion

This analysis suggests that although the Reagan administration has had a major impact on the composition of the budget, the strategy of "starving the budget" through tax cuts has been a mixed success at best. As measured by our basic model, aggregate budgetary priorities in the United States have been relatively stable from Eisenhower through Carter. The Reagan administration, however, appears to have departed dramatically from the broad budgetary priorities of other administrations. In particular, our analysis indicates that defense spending was higher and controllable domestic spending substantially lower under Reagan than would have been the case under pre-Reagan budgetary priorities. Most of this gap emerged in the first three years of the Reagan administration. Moreover, congressional budgetary decisions during fiscal years 1982-86 largely ratified the Reagan departure from historical spending norms.

At the same time, the simulation analysis suggests that Reagan's spending priorities directly conflicted with his strategy of reducing total spending through tax cuts. Defense spending is the area of the budget that has traditionally been most responsive to revenue pressures. The distinctive feature of the Reagan military buildup is the fact that it occurred in the context of large tax cuts and budgetary deficits. If taxes had not been cut, our analysis suggests that past administrations would have instituted a defense buildup of comparable scale. The Reagan administration's reluctance to let the tax-spend logic work in the one area of the budget in which it has been strongest—combined with an inability to reduce domestic uncontrollable spending—meant that total expenditures were not reduced significantly by deficit pressures. The budgetary "diet," imposed only on domestic controllable spending, could not offset the

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large revenue losses of 1981 and subsequent years. By fiscal 1986, total expenditures may have decreased by as little as \$28 billion as a result of the tax cuts, despite an additional cumulative \$398 billion in deficits between 1982 and 1986.

One effect of the Reagan budget strategy was the transformation of domestic controllable budgeting from an "independent process" (Kamlet and Mowery 1987), unconstrained by fiscal pressures and spending pressures from other programs, to one that apparently is far more constrained by "top-down," revenue-based spending constraints. This conclusion is consistent with the analysis of the Reagan budgets by Schick (1983) and Bozeman and Straussman (1982), which argue that "top-down" budgeting (Kamlet and Mowery 1980) has become far more important under Reagan. Such controllable domestic spending, however, represents only a small proportion of the overall budget. More significant is the opposite transformation under Reagan of defense spending into a budgetary policy stream relatively unresponsive to fiscal pressures. Reagan dissolved the link between revenue and spending decisions, and in so doing, appears to have flouted the dietary theory of budgeting far more systematically than his postwar predecessors.

Notes

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1. The *Fiscal Year 1982 Budget Revisions* (Office of Management and Budget 1981b) released on 10 March 1981, described the Reagan budgetary program as "based on the belief that Government policy must again become reliable and consistent. The approach embodied in the Administration's plan is a major departure from past policies, which consisted of rapid growth of Federal spending, excessive increases in the money supply, frequent changes of

direction, historically high and continually rising tax burdens, and unwarranted regulation" (p. 1).

2. Although adherents of supply side economics often attribute this shortfall to actions of the Federal Reserve System, we do not consider the supply side theory in more detail here.

3. Similar arguments are made by Moynihan (1988), von Hayek (cited therein), and Lawrence H. Summers, Interview by Karen Anderson, *Stanford Magazine* (Spring 1987), pp. 21-27.

4. Congressional Budget Office 1985 (p. 155). The study projected total spending of \$920 billion in fiscal 1985 under policies in effect when Reagan was elected, compared to \$938 billion in actual fiscal 1985 spending.

5. In April of 1986, the *New York Times* ran an article entitled "Reagan Acknowledges That Carter Had Proposed a 5-Year Military Buildup." The story reports a conciliatory speech by Reagan addressing former President Carter's charge that "Mr. Reagan 'habitually' misstated his record on military modernization." In the speech Reagan acknowledged that "my predecessor had called attention to the decline of military spending in the mid-1970's and had proposed a five-year expansion of the defense budget."

6. Uncontrollable domestic outlays consist of Medicare, Medicaid, Social Security, Civil Service Retirement benefits, Railroad Retirement benefits, veterans' pensions, unemployment compensation, AFDC, food stamps, school lunch programs, farm income stabilization programs, and General Revenue Sharing.

7. Kamlet, Mowery, and Su (1988) report the results of two-stage least squares estimation, which allows for spending outcomes to influence revenues and vice versa. The results of the two-stage least squares estimation are comparable to those obtained using ordinary least squares.

8. Schick (1980), Mowery, Kamlet, and Crecine (1980), and Schier (1987) portray the process of budgetary and tax policies as "decoupled," with tax policy not responding to spending pressures.

9. The model results reported here differ slightly from those in Kamlet and Mowery 1987. We omitted the "perceived Soviet threat variable" since we do not have measures of this variable for the Reagan period. We also included two additional programs—General Revenue Sharing and farm price support programs—in uncontrollable domestic programs.

10. In using actual economic conditions and actual revenues in the simulations, we ignore any secondary feedback effects, in which Reagan fiscal, tax, and budgetary policies affect macroeconomic performance, which in turn affects subsequent Reagan fiscal, tax, and budgetary policies. To the degree these feedback effects are important, the conclusions of the simulation analysis must be qualified.

11. See Pindyck and Rubinfeld 1981 (pp. 405-10) for a more detailed discussion of this methodology.

12. Measuring total defense spending from fiscal

1982 to 1986 as a percentage of GNP, the 95% confidence level is (.207, .258), compared with actual spending of .3032.

13. Measuring controllable domestic spending from fiscal 1982 to 1986 as a percentage of GNP, the 95% confidence level is (.236, .291), compared with actual spending of .1995.

14. Meyer (1986) noted that "ironically, although the administration's pronouncements stressed fiscal control and the need to protect only the truly needy, its policy decisions during the first term sacrificed fiscal control to the maintenance of a much broader system of both public expenditures and tax subsidies flowing to all income groups" (p. 73). Mills provides a similar assessment (1984, 121-23). Boskin concluded that "the reduction in means-tested benefits payments has been more than offset—in both absolute dollars and growth rates as a share of the budget and GNP—by growing benefit payments in programs that are not means-tested" (1987, 134).

15. Measuring total defense spending from fiscal 1982 to 1986 as a proportion of GNP, the actual level of .3032 is well within the associated 95% confidence level for the second simulation of (.2662, .3191), suggesting that this difference is not statistically significant.

16. To further examine the relationship of revenues and defense spending under Reagan, we ran the basic Kamlet-Mowery model for the period 1955-86, inserting a dummy variable for Reagan and interacting the Reagan dummy with the revenue term. Using defense outlay levels in the president's budget as the dependent variables, we found a statistically significant coefficient for the Reagan dummy variable and a negative, statistically significant coefficient for the interaction term of the same magnitude as the (positive) coefficient for revenues. Using defense outlays after congressional action as the dependent variable led to very similar coefficient estimates but ones with statistical significance below conventional levels. This result is consistent with the conclusions of the simulation analysis, which also indicate that Reagan expanded defense and dissolved the link between revenues and defense spending.

17. The relatively large negative sign for Johnson must be interpreted with some caution. Defense budgets in the model are heavily influenced by combat deaths. When the model is estimated excluding the Johnson administration, yielding the residual values for that presidency, the influence of combat deaths on the military budget is affected mainly by the Korean War and the Vietnam War under Nixon. The negative sign of the residual for the Johnson years (mainly 1966-68), during which combat deaths were relatively high, indicates that there was greater budgetary sensitivity to changes in combat intensity during the Korean War and the latter part of the Vietnam War than during the military buildup in Vietnam.

18. This estimated flexibility of 2% per year is

slightly more conservative than LeLoup's (1978) assessment that the "actions of the president and Congress can be evaluated as contracting, maintaining, or expanding the current services budget, but only within a range of about plus or minus 5% of the total in normal years" (LeLoup 1978, 462-63).

19. Cumulative simulations are less useful for the analysis of executive-congressional budgetary differences than these reinitialized simulations, since in our model the president's budget request is influenced by the budgetary decisions made during the previous year by Congress.

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PRESIDENTIAL PRENOMINATION PREFERENCES AND CANDIDATE EVALUATIONS

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Recent research has altered our understanding of how voters select a candidate in U.S. presidential elections. Scholars have demonstrated empirically that issues, candidate personalities, candidate evaluations, and party identification interact in a dynamic simultaneous fashion to determine vote choice. Other researchers have shown that prenomination candidate preferences play an integral role in structuring the general election vote. We join together these two important trends to introduce and test a revised model of vote choice, using 1980 NES panel data. The results reconfirm that candidate selection is part of a dynamic simultaneous process and reveal for the first time that prenomination preferences are woven tightly into this causal web.

The empirical study of vote choice in presidential elections entered a new era in the 1970s. Although the principal research question—What determines vote choice?—remained the same, significant methodological advances spawned a flurry of research that altered our understanding of the decision-making process culminating in the vote. Beginning with Jackson (1975), scholars used newly developed two-stage least squares regression to confirm what earlier researchers (Campbell et al. 1960; Goldberg 1966) could only hypothesize: that the forces structuring presidential vote choice were related simultaneously to one another and to the vote (Fiorina 1981; Markus 1982; Markus and Converse 1979; Page and Jones 1979). Along with employing two-stage least squares, Markus and Converse (1979), Fiorina (1981), and Markus (1982) also adjusted their models to reflect the dynamic nature of the vote choice process. These scholars

recognized that the attitudes and beliefs that influence candidate selection are the product of previously held attitudes and beliefs and thus that a properly specified model of vote choice must incorporate this dynamic structure. So by the early 1980s the act of voting for a presidential candidate was seen as the end result of a dynamic simultaneous interplay of forces.

At about the same time that scholars began developing dynamic simultaneous models of vote choice, the presidential selection process underwent substantial change. Starting with the 1972 campaign, the locus of power during the nomination process shifted from the party elite to the rank-and-file electorate. This change transformed presidential campaigns from primarily two-candidate affairs commencing on Labor Day weekend to multicandidate events spanning a year or more. As such, models of vote choice that focused on individuals' attitudes toward general election candidates to the exclusion of at-

titudes toward the major prenomination contenders may have missed an important determinant of the vote; that is, it seems quite plausible that individuals' orientations toward presidential hopefuls at the prenomination stage may influence general election choices.

Unfortunately, the dynamic simultaneous models of vote choice developed to date have failed to incorporate a term tapping individuals' attitudes toward prenomination contenders. The presidential nomination reforms did, however, spark interest in what has commonly been called the "carryover" hypothesis (Stone 1986). The hypothesis contends that prenomination battles often leave supporters of losing candidates bitter toward their party's eventual nominee, perhaps leading some actually to prefer the opposition party's candidate in the general election. Scholars searching for such a link between prenomination candidate preferences and general election preferences in presidential campaigns have found supporting evidence. Stone (1986), Southwell (1986), and Lengle (1980) used individual-level data to demonstrate that on average, people who supported candidates who failed to win the nomination were less likely to support their party's nominee in the fall election than individuals who backed the eventual nominee during the primary season. Kenney and Rice (1987) and Lengle (1980) corroborated these findings with aggregate-level data. The weight of the evidence, then, suggests that prenomination preferences influence general election vote choice. Regrettably, though, a review of the carryover research reveals that no attempt was made to incorporate prenomination preferences into a fully specified dynamic simultaneous model of the November vote.

The pioneers of the dynamic simultaneous models of voting failed to include prenomination preferences in their empirical tests and the scholars examining the effect of prenomination preferences neglected to

develop time-sensitive reciprocal models. Our goal is to develop a more comprehensive model of vote choice in presidential elections by merging these two trends.

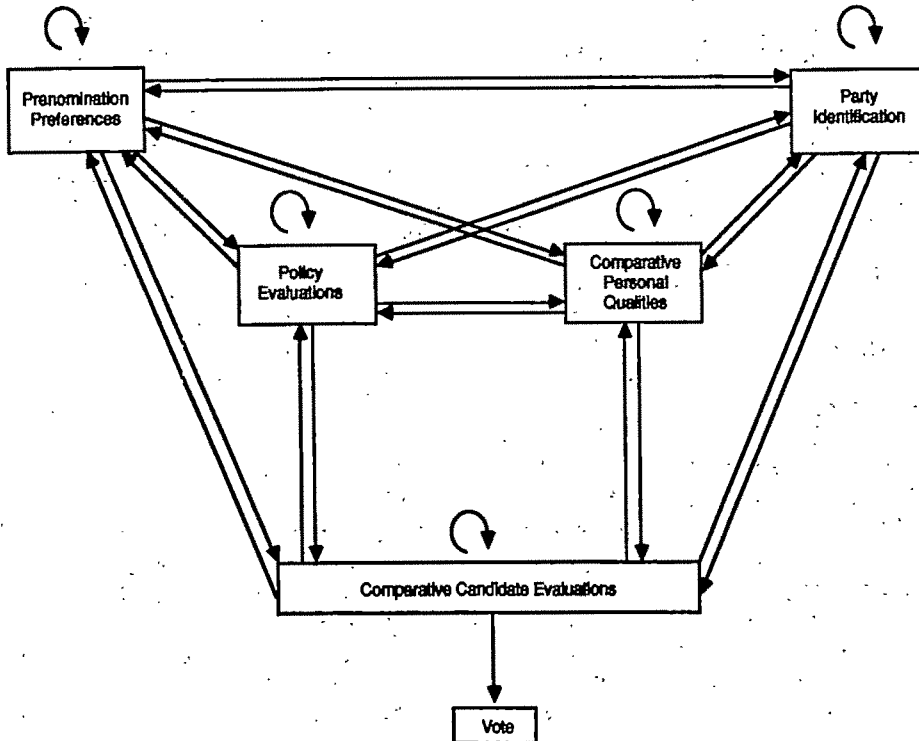
A Revised Model of Vote Choice

In Figure 1 we diagram a dynamic simultaneous model of vote choice that includes a prenomination preference term. The model borrows liberally from those developed earlier (Markus 1982; Page and Jones 1979). Indeed, aside from the prenomination preference term all the variables appear regularly in contemporary models of vote choice. And the rationale behind the reciprocal paths linking these standard variables has been delineated by a variety of scholars (Fiorina 1981; Markus and Converse 1979; Page and Jones 1979). Even the placement of the actual vote term parallels earlier models. As Markus and Converse (1979, 1057) contend, policy evaluations, personal qualities, and party identification are not linked directly to the vote, but instead "their confluence yields a set of overall candidate evaluations, on the basis of which a choice is made." All that remains is to justify the theoretical ties between prenomination preferences and the other variables in the model.

After careful consideration of the proper causal placement of prenomination preferences in a voter's decision-making process we concluded that preferences were quite possibly simultaneously related to all the other variables in the model. For instance, it seemed quite likely that prenomination preferences could influence policy evaluations and that evaluations could influence preferences. Page and Jones (1979) found such a reciprocal link between general election preferences and policy evaluations, and we see no reason why prenomination preferences and evaluations would not be causally intertwined as well. Likewise, prenomination preferences may help shape individ-

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Figure 1. A Revised Model of Vote Choice



uals' views of candidates' personal qualities, and perceptions of personal qualities could influence preferences. After all, Markus (1982) determined that general election preferences and candidate qualities were simultaneously related. Prenomination preferences and party identification may also be caught in this causal web. Individuals' party attachments may strengthen or weaken depending on how their preferred candidate fared in the nomination struggle (Florina 1981), and the strength of individuals' party ties may influence their choice for the nomination (Abramson, Aldrich, and Rohde 1986; Bartels 1987). Finally, prenomination candidate preferences—termed *comparative candidate evaluations* in our model—could well be knotted causally. Certainly, pre-

nomination preferences could influence general election preferences (Stone 1986). At the same time, though, opinions of the general election candidates may alter how individuals view candidates that were involved in the nomination contest. In all, then, much like general election candidate evaluations, prenomination preferences may well be related simultaneously to all other important variables influencing vote choice. This myriad of reciprocal paths is incorporated in Figure 1.

In addition to specifying the probable causal links between variables, we also render Figure 1 dynamic through the self-contained, looped paths associated with the variables. Each loop signifies that that variable is in part a product of past values on the variable (Markus 1982). For example, the prenomination preferences loop

means that current preferences are in part a result of past preferences.

Data and Methodology

We employed the 1980 NES panel data to test our model outlined in Figure 1. The panel study, representing one phase of the 1980 NES project, sampled the opinions of a set of respondents at four time points during the election year: P1 in January; P2 in June; P3 in September; and P4 after the November election. The P3 (September) survey served as our primary data base because it tapped respondent feelings after the party conventions but prior to the November election. The P4 (November) wave was unusable because it did not include the feeling thermometer, issue, or candidate quality questions necessary for variable construction. Moreover, it measured respondent opinion after the general election. The P2 (June) survey was rejected because it assessed opinion at the culmination of the bitter prenomination season, long before the general election campaign had had the opportunity to mend intraparty squabbles.

The causal paths in Figure 1 were estimated by party and then reestimated using a more comprehensive sample of respondents. The party-specific analyses allow us to compare the decision-making processes used by Democrats and Republicans in determining their choice for president in 1980. The full sample analysis provides a more generalizable portrait of the interplay of forces structuring candidate selection.

We now turn to operationalizing the variables in Figure 1, beginning with comparative candidate evaluations. Following the lead of Page and Jones (1979) and Markus and Converse (1979), we conceptualized comparative candidate evaluations as the arithmetic difference between the scores respondents gave the two major party general election candidates, Carter and Reagan, on the standard NES

feeling thermometer scales (Carter score minus Reagan score).

Among the determinants of comparative candidate evaluations is party identification. The standard NES seven-category party identification question was used to operationalize partisanship for empirical testing. On the basis of recent research showing that weak and leaning partisans behave similarly (Keith et al. 1986), we collapsed these categories into one. In addition, because the initial part of our analysis requires that respondents be segmented by party, the party identification question served to divide our analysis into Democratic and Republican constituencies. Only those respondents who identified themselves as Democrats or independents were included in the Democratic analysis, while the Republican analysis was comprised of the same independents and all Republicans. The independents were included in both analyses because their nonaffiliation prohibited us from knowing, *a priori*, which parties' prenomination battles should more strongly influence their general election preferences.

The issue orientations of a respondent may also influence general election preferences (Markus and Converse 1979; Page and Jones 1979). To test this possibility an issue variable was constructed that summarized respondent opinion on two of the most salient issues in the 1980 campaign: Carter's handling of both the Iran hostage affair and inflation (Markus 1982). Individuals were asked how strongly they approved or disapproved of the president's performance in these two areas. We simply summed these responses to form a policy evaluation score for each interviewee. Thus the final issue score for a respondent reveals how strongly he or she approved or disapproved of Carter's handling of two pivotal issues in the campaign.

In addition to partisanship and issues, respondents' perceptions of the personal

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qualities of the general election candidates help shape comparative candidate evaluations (Markus 1982; Markus and Converse 1979). Scholars have used the NES questions asking respondents how well the words and phrases *moral, dishonest, weak, knowledgeable, power hungry, inspiring*, and *provides strong leadership* describe the major party candidates to construct a comparative personal qualities index. The variable is calculated by summing each respondent's score for each candidate across the seven personal qualities and then figuring the arithmetic difference between the two candidates' scores. The resulting summary score quantifies which candidate's personal qualities the respondent most admires and by how much. We use this procedure to construct a comparative personal qualities index for Carter and Reagan.

Party identification, policy evaluations, and comparative personal qualities comprise the variables traditionally used to account for vote choice. To this trio we add a measure of prenomination preferences. Our measure, patterned after comparative candidate evaluations, concurrently assesses respondents' preferences for the two major candidates seeking each party's nomination. For respondents in the Democratic analysis the variable was formed by subtracting the feeling thermometer scores given Kennedy from the scores given Carter. The prenomination variable for the Republican analysis was figured by subtracting respondents' scores for Bush from their scores for Reagan. The feeling thermometer scores for Kennedy and Bush were employed instead of those for the many of other unsuccessful candidates for the nominations (e.g., Anderson, Brown, and Connelly) because these two candidates were the principal challengers to Carter and Reagan after the first couple of months of the primary season. For the interparty analysis Democrat and Republican respondents (including leaners) were pooled. The

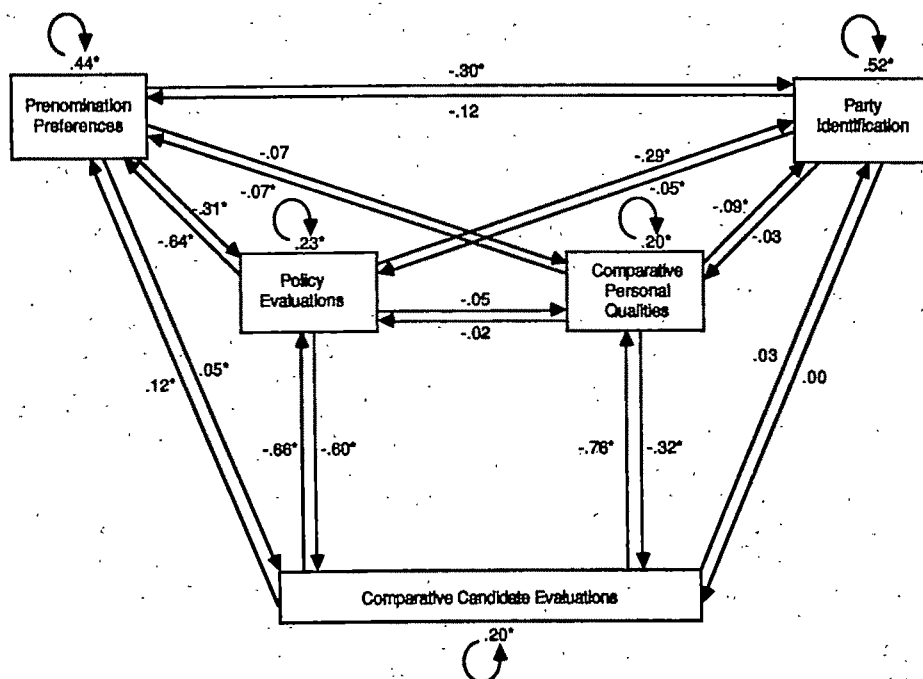
preference variable was then refigured to form a continuum running from ardent Reagan Republicans to strong Carter Democrats. Specifically, Republican respondents' feeling thermometer scores for Reagan were subtracted from the scores for Bush and 100 was subtracted from the difference. For Democrats, Carter's feeling thermometer scores were subtracted from Kennedy scores and 100 was added to the difference. The result is a variable ranging from -200 for Republican respondents who scored Reagan 100 and Bush 0, to 200 for Democratic respondents who scored Carter 100 and Kennedy 0. Pure independents were excluded because we had no ready means of determining which party's candidates to use for variable construction (Stone 1986).

Estimating the causal paths in our revised model of vote choice is possible now that the variables have been operationalized. As stated above, we employed data from the September (P3) wave of the panel. The simultaneous nature of the model requires using two-stage, least squares regression (Markus and Converse 1979; Page and Jones 1979). When each of the variables was estimated to correct for any reciprocal bias, one of the predictor variables was the same variable from the January (P1) wave. Including the lagged variable in the simultaneous equations assesses the impact of previous voter opinion on current opinion, thus building in the dynamic nature of the vote choice process (Markus 1982).

Intraparty Analyses

The path estimates for the Democratic and Republican equations are presented in Figures 2 and 3, respectively. The estimates are standardized, two-stage, least squares coefficients. All the coefficients are in the expected direction except for (1) those between policy evaluations and comparative personal qualities in the Democratic model and (2) the path

Figure 4. Interparty Analysis.



Note: $N = 655$; all entries are two-stage, least squares standardized coefficients.
* $p < .05$.

estimate running from prenomination preferences to party identification in the Democratic model.

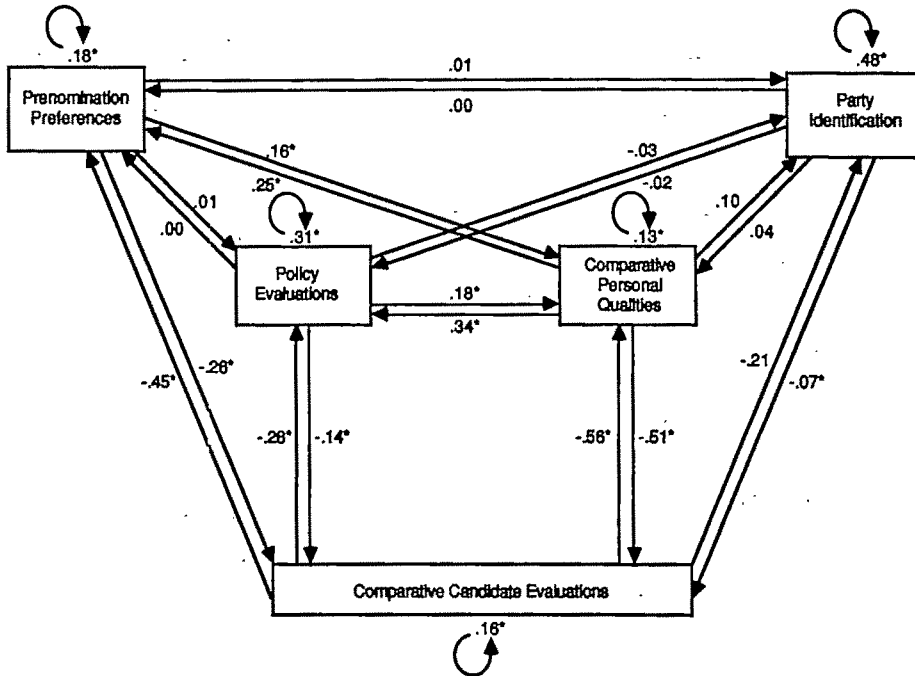
Examination of the paths flowing from the prenomination preference variables show preferences having a strong direct effect on comparative candidate evaluations as well as an important indirect influence. Focusing first on the Democratic figure (Figure 2), we see that the coefficient associated with the path from prenomination preferences to comparative candidate evaluations is significant, indicating that the higher a respondent ranked Kennedy compared to Carter on the feeling thermometers, the more likely

he or she was to rate Reagan high relative to Carter.

Figure 2 also suggests that prenomination preferences have an indirect influence on comparative candidate evaluations through policy evaluations. The healthy beta between preferences and policy evaluations implies that a portion of respondents who ranked Kennedy high compared to Carter vented their frustrations by criticizing the president's handling of the Iran hostage crisis and inflation. And the strong beta between policy evaluations and comparative candidate evaluations indicates that once critical of Carter's performance, many of these in-

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Figure 3. Republican Analysis



Note: $N = 490$; all entries are two-stage, least squares standardized coefficients.

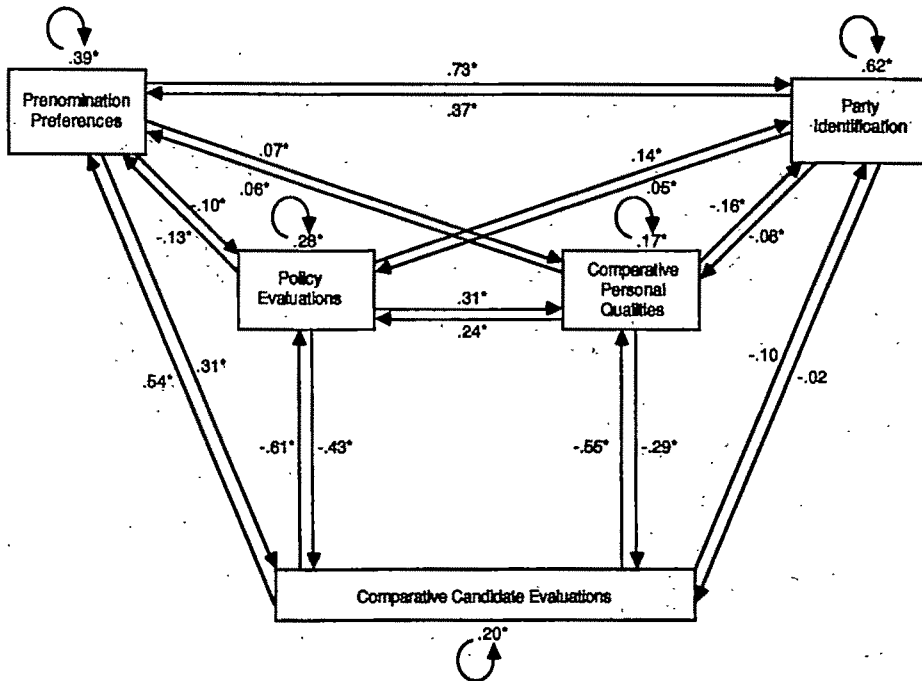
* $p < .05$.

dividuals proceeded to score Reagan near or above Carter on the feeling thermometers. The indirect effect of prenomination preferences on comparative candidate evaluations through policy evaluations is .19 ($-.31 \times -.60$), almost four times the influence of preferences on general election candidate evaluations directly. It would appear, then, that a good number of disenchanted Democrats and independents viewed the election as a retrospective evaluation of Carter's presidency (Abramson, Aldrich, and Rohde 1983).

The results of the Republican analysis suggest that prenomination preferences

played a more prominent role in the vote decision-making process among Republicans in 1980 than among Democrats. Figure 3 reports a robust coefficient of $-.26$ for the causal path leading directly from prenomination preferences to comparative candidate evaluations, over five times the magnitude of the same path in the Democratic model. The healthy coefficient implies that the more a respondent preferred Bush to Reagan, the more likely he or she was to score Carter high relative to Reagan. The coefficient may be stronger than its Democratic counterpart because most Republicans perceived Bush as more moderate than Reagan while

Figure 4. Interparty Analysis



Note: $N = 842$; all entries are two-stage, least squares standardized coefficients.
* $p < .05$.

Democrats quite often saw Kennedy as more liberal than Carter. Thus Republicans who preferred Bush to Reagan may have found it more acceptable ideologically to support the opposing party's nominee than did Democrats who preferred Kennedy to Carter.

Figure 3 also reveals that prenomination preferences had an important indirect effect on comparative candidate evaluations through personal qualities ($.16 \times -.51 = -.08$). This indirect influence suggests that the higher a respondent ranked Bush compared to Reagan, the more likely he or she was to evaluate Carter's personal qualities more highly than Reagan's. In turn, the higher a

respondent ranked Carter's qualities relative to Reagan's, the more likely he or she was to rank Carter near or above Reagan on the general election feeling thermometers.

Interparty Analysis

For a final test of our revised model of vote choice we estimated the paths with a combined sample of Democrats and Republicans. The estimates are presented in Figure 4. Once again, the vast majority

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of the coefficients are in the anticipated direction, the only exceptions being the betas associated with the causal links between party identification and comparative personal qualities and between party identification and comparative candidate evaluations and the path estimate running from party identification to policy evaluations.

The coefficient accompanying the causal arrow flowing from prenomination preferences to comparative candidate evaluations indicates that preferences had a strong direct influence on respondents' evaluations of Carter and Reagan. Indeed, the beta compares favorably with the coefficients running from policy evaluations and comparative personal qualities to comparative candidate evaluations and eclipses the path estimate running from party identification to evaluations.

Also of interest in Figure 4 is the sizable coefficient associated with the path from preferences to party identification. The magnitude of the beta lends support to recent work contending that party identification is susceptible to campaign-specific forces (Fiorina 1981), in this case prenomination preferences. The coefficient implies that Carter backers tended to move toward the Democratic end of the party identification scale during the nomination campaign and that Reagan supporters moved toward the strong Republican ranks. Kennedy and Bush supporters, meanwhile, may have weakened their party ties, with Kennedy backers moving away from the Democratic end of the party identification scale and Bush loyalists moving away from the Republican end of the scale. Evidently, the ramifications of a divisive prenomination struggle may go well beyond the general election. The primary battles may serve to weaken a party's standing among the electorate for the long term.

Lastly, it is pleasing to note that the comparative candidate evaluations

variable in Figure 4—which was constructed as part of the two-stage, least squares estimation process—proved a trusty predictor of respondents' actual general election vote choices. To figure the forecasting utility of the evaluations variable we dichotomized vote choice (Carter = 1; Reagan = 0; all other candidates and nonvoters = missing) and employed logistic regression. When the comparative candidate evaluations variable was regressed on the dichotomized vote measure, the resulting coefficient was .043 with a standard error of .005 ($p < .001$). Although interpretation of the coefficient is not straightforward because logistic regression produces a logarithmic odds function, it is possible to calculate the change in probability of voting for one candidate over the other for a unit change in the evaluations variable (Petersen 1985). For our equation the calculation produced a tidy conclusion: a positive change in the comparative candidate evaluations variable of 1% corresponded to a 1% greater chance of voting for Carter.

As far as prediction is concerned, if we assume that respondents vote for the candidate they ranked highest on the comparative candidate evaluations variable, our model predicts 89% of the cases correctly—an impressive performance, given that the evaluations variable was constructed from September data. The Markus and Converse (1979) model, using November data and adding the predictive ability of party identification, predicted 90% of the cases correctly for 1972 and 97% for 1976.

Taken together, the three dynamic simultaneous analyses provide solid evidence that prenomination preferences played an integral role in shaping respondents' general election evaluations of Carter and Reagan. Results showed that even after controlling for a variety of other forces, preferences remained strongly linked to evaluations.

Discussion

Despite widespread agreement that vote choice is the result of a dynamic simultaneous process and equally widespread agreement that a prenomination campaign influences general election vote choice, researchers have failed to meld these two important trends. We employed the 1980 NES panel data to accomplish this task. A variable tapping respondents' prenomination preferences was fitted into a fully specified dynamic simultaneous model of the vote. When tested, the analyses substantiated the importance of prenomination preferences in the formation of general election vote choice. Indeed, the findings provide perhaps the most compelling evidence to date in support of the carryover hypothesis. Specifically, the results revealed that the higher a respondent scored Kennedy compared to Carter on the feeling thermometer scales, the more likely he or she was to rank Reagan near or above Carter on the general election thermometers. Likewise, the higher a respondent ranked Bush relative to Reagan, the more likely he or she was to rate Carter high compared to Reagan.

The impressive performance of the prenomination preference variable does more than simply supply evidence in support of the carryover hypothesis, however. It confirms prenomination preferences as a vital determinant of general election vote choice. To be sure, the precise effect of prenomination preferences will vary from campaign to campaign, but our findings suggest strongly that the prenomination season does shape individuals' attitudes toward the candidates and that these attitudes do carry over to influence the general election vote decision. Scholars modeling vote choice, then, should take care to include prenomination preferences in their equations. Indeed, models of vote choice that fail to consider prenomination

preferences may well be specified improperly; thus their parameter estimates should be viewed with the utmost caution.

Note

The data analyzed were made available through the Inter-University Consortium for Political and Social Research, University of Michigan.

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SIMPLE EXPLANATIONS OF TURNOUT DECLINE

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Several recent studies have claimed to explain the post-1960 decline in U.S. presidential and congressional election turnout in terms of just a few variables. Abramson and Aldrich (1982) attribute the great bulk of the decline to aggregate declines in partisanship and political efficacy. Shaffer (1981) attributes still more of the decline to aggregate decreases in these same two variables plus age and following the campaign in the newspapers. And Kleppner (1982) attributes "virtually all" of the decline to aggregate decreases in the first three of these variables. We show how these studies measure explanation too generously and how the very short-handedness of their models makes them seem more successful than they are. These few variables, we conclude, leave most of the decline unexplained.

Among the most widely noted trends in U.S. political behavior is the disquieting decline in presidential and congressional election turnout. At the moment the curve may be bottoming out—the percentage voting edged up fractionally from 1980 to 1984—but from 1960 to 1980 it fell steadily. In the 1960 presidential election, 64% of those eligible voted; by 1980, the figure had fallen to 53%. Although the mobilization of blacks and women reversed the trend in southern states, the decline outside the South was correspondingly greater (from 71% to 54%).¹ Within the South turnout dropped among white males (Cassel 1979).

That turnout should have declined during this period is, as Brody (1978) has aptly termed it, a "puzzle." The most obviously relevant changes in the causal background—increased levels of education and the relaxation of registration requirements—logically should have in-

creased turnout. Yet proportionally fewer and fewer people have been voting. Other more powerful variables must be keeping people from the polls. But what?

Several recent studies have provided engagingly simple answers. Abramson and Aldrich (1982) point to aggregate decreases in strength of partisanship and external political efficacy (perceived government responsiveness). These two variables alone, by their estimate, account for some 70% of the 1960–80 decline. Shaffer (1981) finds that decreases in age and following the campaign in the newspapers, along with decreases in partisanship and efficacy, explain 86% of the 1960–76 decline. Kleppner (1982, 130) concludes that the decreases in partisanship, efficacy, and age explain "virtually all" of the 1960–80 decline.²

These two-, three-, and four-variable explanations are beguiling, and Abramson and Aldrich's, in particular, is widely cited; but they are too simple. Partisan-

ship, efficacy, and age have almost certainly contributed to turnout decline but are not the whole story. Much fuller accounts leave most of the decline unexplained (Ashenfelter and Kelley 1975; Cassel and Hill 1981; Reiter 1979); these slenderer ones can only appear to do better.

One source of this illusion is that these studies score explanation too generously, but a more basic source is precisely that their variable lists are too short.³ The exclusion of variables that have acted to increase turnout exaggerates the explanation of net decline. The exclusion of other variables that have acted to decrease turnout allows the included variables to take credit for explanation that is not theirs. Even the exclusion of variables that have neither increased nor decreased turnout but do affect individual turnout cross-sectionally can produce a similar misallocation, depending on the measure of explanation.⁴

We shall concentrate here on these specification issues. After briefly indicating how these studies' statistics stretch explanation, we consider and refute Abramson and Aldrich's and Shaffer's claims of immunity from misspecification bias. To illustrate the point we show how adding variables to the Abramson and Aldrich model reduces the role of partisanship and efficacy. Although we confine the analysis to presidential elections, the argument applies equally to congressional elections and for that matter to other time series "explained" by underspecified models. The conclusion in the present case is that Brody's puzzle has been only partly solved.

Measures of Explanation

Let us first see how these studies reach their results. Abramson and Aldrich base their claims partly on "algebraic projections" of what turnout would have been if

partisanship and efficacy had not weakened. To see how this works, let the proportion of the electorate in the i^{th} category of partisanship and j^{th} category of efficacy in the t^{th} election be s_{ijt} (with $t = 0, 1, \dots, 5$ representing 1960, 1964, . . . , 1980), the proportion of the people in this combination of categories who vote in the t^{th} election be r_{ijt} , and the t^{th} election turnout be T_t , so that

$$T_t = \sum_i \sum_j r_{ijt} s_{ijt}. \quad (1)$$

Playing on this identity Abramson and Aldrich estimate (1) the proportion of the electorate that would have voted in the t^{th} election had the joint distribution of partisanship and efficacy remained what it was in 1960 as

$$\hat{T}_t = \sum_i \sum_j r_{ijt} s_{ij0}, \quad (2)$$

(2) the decline that would have occurred under this null scenario as $T_0 - \hat{T}_t$, and (3) the proportion of the actual decline $T_0 - T_t$ explained by partisanship and efficacy as

$$A_1 = 1 - [(T_0 - \hat{T}_t)/(T_0 - T_t)] \\ = (\hat{T}_t - T_t)/(T_0 - T_t). \quad (3)$$

Shaffer and Kleppner form similar projections from each variable separately, substituting adjusted for actual turnouts within categories as a form of remedial partialing. To obtain the adjusted turnout for the j^{th} category of the k^{th} explanatory variable—say, the pure independent category of partisanship—in the t^{th} election, they regress individual turnout on the other explanatory variables.⁵ Since the residuals reflect the extent to which the probability of voting is higher or lower than it should be on the basis of the other variables, Shaffer and Kleppner define the adjusted turnout for pure independents r_{jkt}^* as the overall turnout T_t plus the mean residual within that category. Then they

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define (1) the proportion of the electorate that would have voted in the t^{th} election if the distribution of partisanship had remained what it was in 1960 as

$$\hat{T}_{kt} = \sum_j r_{jkt}^* s_{jk0}, \quad (4)$$

(2) partisanship's contribution to the decline as $\hat{T}_{kt} - T_t$, and (3) the proportion of the decline all the variables together explain as

$$SK = \sum_k (\hat{T}_{kt} - T_t) / (T_0 - T_t).^6$$

This exhausts the relevant portions of Shaffer and Kleppner, but Abramson and Aldrich try a second, more sophisticated analysis, based on a probit model of individual turnout as a function of partisanship and efficacy. They estimate the probit parameters on the pooled cross-sectional data from the 1960-80 NES election studies, then combine the estimates with the t^{th} election means of partisanship and efficacy to generate the predicted turnout

$$\hat{T}_t = \phi(\hat{\alpha}_0 + \hat{\alpha}_1 \bar{P}_t + \hat{\alpha}_2 \bar{E}_t), \quad (5)$$

where ϕ is the cumulative standard normal density function, the $\hat{\alpha}$ s are the parameter estimates, and \bar{P}_t and \bar{E}_t are the means of partisanship and efficacy. Then, finally, they regress the actual turnout T_t on this prediction, obtaining an estimate b of the slope b . Since the change in actual turnout that can be expected from a $\Delta \hat{T}_t$ percentage point change in the probit prediction, according to this linear regression, is $b \Delta \hat{T}_t$, and the proportion of the actual change ascribable to the predicted change is thus $\Delta \hat{T}_t / b \Delta \hat{T}_t = 1/b$, they set $A_2 = 1/b$.⁷

On the surface these measures seem plausible enough, and they certainly produce rosy figures. By A_1 , partisanship and efficacy explain 72% of the 1960-80 decline; by A_2 , they explain 67%. By Shaffer's version of SK , partisanship, effi-

cacy, age, and following the campaign in the newspapers explain 86% of the 1960-76 decline; by Kleppner's, partisanship, efficacy, and age explain 107% of the 1960-80 decline.

As this last, impossibly large number suggests, however, these measures run high.⁸ SK is particularly inflated, because variable-by-variable projections attribute some of the same explanation to more than one variable, and the adjusted turnouts do not eliminate the overlap.⁹ But A_1 and A_2 are inflated, too. They, too, as we show in Appendix A, can exceed 1.0. These are not proportions of the decline explained in the same way as the conventional R -squared is the proportion of variance explained. An SK , A_1 , or A_2 of .7 is less than 70% of what it could be.

Specification

The deeper problem, however, is under-specification. These analyses omit all the variables that have acted to increase turnout. They even omit some of the variables that have acted to decrease turnout. And they omit all the variables that affect individual turnout cross-sectionally (compare Ashenfelter and Kelley 1975; Cassel and Hill 1981; Conway 1981; Patterson and Caldeira 1983; Powell 1986; Reiter 1979; Wolfinger and Rosenstone 1980).¹⁰

Abramson and Aldrich and Shaffer concede some omissions but claim dispensations. A first claim, made by both, is that because they are interested only in explaining variation over time they can ignore variables that affect individual turnout but whose distributions have stayed roughly constant. A further claim, made only by Abramson and Aldrich, is that because they are interested specifically in explaining turnout *decline* they can also ignore any variable whose distribution has changed, but in a way that should have increased turnout.

The first claim has a plausible ring.

Intuitively, a constant cannot explain a variable. In both linear and probit models, variables that are really constants blend into the intercept. Variables that are only approximately constant may have some effect, but the bias from omitting them depends not only on their effects but also on their correlations with the included variables,¹¹ and anything that has varied only weakly over time can be only mildly related over time to partisanship, efficacy, age, and following the campaign in the newspapers, which have trended more strongly.

All this is true and is applicable to strictly time series devices like A_1 . In this one analysis, Abramson and Aldrich can legitimately exclude variables that affect turnout but are roughly stationary over time. But they also exclude variables that affect turnout and *have* varied over time: education, concern over the election outcome, age, residential mobility, contacting by political parties, and registration and voting requirements.¹² These variables are correlated over time with partisanship, efficacy, and one another; and their exclusion biases A_1 . Bringing them into the analysis would erode the effects of partisanship and efficacy. For A_1 , then, the first claim, though valid, does not cover the exclusions.

For A_2 and SK the claim is invalid to begin with. These statistics incorporate individual-level estimations, from which only individual-level, not temporal, invariance can excuse omissions. The omission of variables that affect individual turnout—even variables that have been relatively constant over time—inflates the probit parameter estimates in Equation 5 and the projected turnouts from Equation 4. The biased estimates from an underspecified model may manage in some sense to “explain” the decline, but some unknown portion of the explanation is really due to the variables the model neglects.

The second claim is simply false. It is

false even when we do not estimate parameters at the individual level, as in A_1 . Suppose, to proceed by analogy, that your bank balance falls by \$500 during a month in which you can recall making \$400 of deposits and only \$500 of withdrawals. How far do these known transactions account for the decline? Entirely, if we go by the ratio of withdrawals to net decrease, analogous to A_1 (or SK) confined to variables decreasing turnout. But this is obviously not right: \$400 is missing. We need to include the deposits. The real explanation of the decrease is only $(500 - 400)/500 = .2$. In broadly similar fashion the exclusion of variables increasing turnout from the probit equation underlying A_2 makes the correspondence between actual and predicted turnout artificially close. This too “neglects the deposits.” We think we have done better than we have at explaining why turnout declined if we ignore the forces increasing turnout.

Some Illustrations

As this discussion implies, we should be able to deflate these results by introducing (1) variables that have increased turnout over time, (2) other variables that have decreased it over time, or even (3) variables that have not affected it much (because they have not varied much) over time but that do affect individual turnout cross-sectionally. Let us see what happens to Abramson and Aldrich’s results when we add one variable of each type, one at a time.

For additions of type 1, the main question is how much the augmented model explains. Simply adding some such variable and recalculating A_1 and A_2 should yield lower estimates. Explanation, by these measures, can decrease with additional variables. Additions of type 3, on the other hand, should not affect explanation, while those of type 2 should increase

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it. What should decrease is the explanation allotted to partisanship and efficacy, once the new variable has been taken into account. To gauge this we define the multiple-partial measures A_1^* and A_2^* , described in Appendix B.

The variables we shall try adding are

1. *Education.* Along, perhaps, with the effective enfranchisement of blacks, the most salient variable increasing turnout over this period is education. How much of "education's" effect is education's per se is unclear (Cassel 1987; Luskin 1987), but education is at least correlated with the participatory norms, political knowledge, and cognitive and social skills that lighten the costs, and increase the perceived benefits of, voting. Previous studies suggest that it has a powerful influence on turnout (Campbell et al. 1960; Powell 1986; Verba and Nie 1972; Wolfinger and Rosenstone 1980).

What happens when we add education to Abramson and Aldrich's analysis?¹³ We begin with A_1 . To conserve cases within partisanship-efficacy-education combinations, we divide education only into "high" (high school graduate or better) and "low" (no high school degree) and collapse the categories of partisanship and efficacy, from 4 to 3 and 3 to 2, respectively.¹⁴ With partisanship and efficacy collapsed in this fashion, $A_1 = .83$: partisanship and efficacy explain 83% of the 1960-80 decline. When we add education, however, $A_1 = .24$: partisanship, efficacy, and education explain only 24% of the decline. A_2 beats a similar, if milder, retreat. Replicating Abramson and Aldrich's analysis, we find $\hat{b} = 1.51$, and $A_2 = .66$, only trivially different from their numbers.¹⁵ With the addition of education, however, $\hat{b} = 1.91$, and $A_2 =$ only .53. From this angle, too, we see how the exclusion of variables increasing turn-

out can exaggerate explanation.

At the same time, one might suspect with Abramson and Aldrich (1984) that controlling for education would increase the role of partisanship and efficacy. But let us look at our multiple-partial measures and see. $A_1^* = .49$; $A_2^* = .60$. Even this variable increasing turnout cuts into the explanation assigned to partisanship and efficacy. More direct competition can bring still larger drops in A_1^* and A_2^* , as we shall see.

2. *Concern.* Caring which party wins the election ("concern") should logically stimulate voting (Campbell et al. 1960) and has declined over this period.¹⁶ Although the reference to party suggests some overlap with partisanship (Abramson and Aldrich 1982; Cassel and Hill 1981), the correlation between the two is only about .35—low enough to show that they are far from the same variable but high enough that they compete for explanation.

Controlling for concern, $A_1^* = .61$. The percentage of the decline ascribed to partisanship and efficacy decreases by over 20%. In the probit estimation, $A_2^* = .56$. The percentage of the decline explained by partisanship and efficacy drops by 10%.

3. *Interest.* People who are more interested in the campaign are more likely to vote, in much the same way as more ardent basketball fans are more likely to attend basketball games. But interest has been roughly stationary, winding up only slightly higher in 1980 than in 1960, and can therefore have made little contribution to turnout decline.¹⁷

For this reason, controlling for interest hardly affects the strictly time series measure A_1 : $A_1^* \approx A_1$.¹⁸ A_2 , however, incorporates individual-level estimates of the probit parameters, and the ex-

clusion of variables that affect individual turnout biases the parameter estimates for partisanship and efficacy. With interest in the equation, these latter decrease, leaving A_2^* = only .42. The inclusion of interest, despite its lack of trend, slices 25% from partisanship and efficacy's explanation of the decline.

Even 42% is a lot of explanation from only two variables, of course, but this is still too flattering an estimate. A_1^* and A_2^* are still generous measures, still capable of exceeding 1. Even when less than 1 individually, moreover, the A_1^* or A_2^* for partisanship-and-efficacy-controlled-for-interest and the A_1^* or A_2^* for interest-controlled-for-partisanship-and-efficacy can sum to more than 1. They do not uniquely apportion the explanation among predictors.

But above all we have been adding variables only one at a time. A fair number of other variables have acted to increase or decrease turnout since 1960 or affect individual turnout. A truly well specified model, in which partisanship and efficacy would face multiple competition, would award them a still smaller share of the explanation. They might still account for a significant chunk—we suspect in fact that they would—but it would be considerably smaller than 42% even by this generous measure.¹⁹

Conclusion

All models are misspecified, to be sure. We deliberately make small omissions, trading completeness for parsimony, and must always live with the possibility of having unwittingly made larger ones. But we do think it fair to insist that a model be specified up to a certain disciplinary standard. Variables that theoretically ought to affect turnout, that empirically seem to affect it, and that are correlated

with other variables that affect it cannot simply be waved off.

Precisely what variables we are obligated to include depends on the level of abstraction. At a sufficiently general level a model with only a handful of variables—costs, benefits, and probability of affecting the outcome (as in Downs 1957; Riker and Ordeshook 1968)—may be perfectly adequate. But if we are interested in knowing what the relevant costs and benefits consist of and what affects the perceived probability of affecting the outcome, we must disaggregate these categories into variables like partisanship, efficacy, and age. At this more concrete level, however, these few variables are not enough.

The standards of specification also depend on the level of analysis. At the aggregate level a time series analysis may safely exclude variables that have not varied much over time. But even a time series analysis must be adequately specified at its level, and at the partisanship-and-efficacy level of abstraction this means adding variables like education, concern, age, mobility, and registration and voting requirements. That some of these variables may have increased rather than decreased turnout does not justify omitting them.

As a practical matter no decently specified time series model can be estimated directly. The variables we must include are too many, the elections for which we have attitudinal data too few. For the foreseeable future, we shall have to estimate at the individual level, then aggregate. This is statistically preferable in any case, since aggregation prior to estimation sacrifices some efficiency (Kmenta 1986, 369–70). But since timewise invariance does not justify individual-level exclusions, we shall have to work with longer equations.

Most of these criticisms stand as general cautions. It may be tempting, whenever the object is to explain some aggregate

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trend, to take at face value measures like the ones in these studies. It may be tempting to disregard variables that have countered the trend. And it may be tempting to ignore variables that have only a cross-sectional effect even when the estimation is partly at the individual level. These are all traps for the unwary.

In the present case we have shown how the explanation shrinks as we add variables that have acted to increase turnout and how the burden of explanation shifts away from partisanship and efficacy as we add other variables—even, depending on the measure, variables that have not varied much over time and therefore cannot have contributed much to the decline but that do affect individual turnout.

We do not doubt that partisanship, efficacy, and age have played a part in decreasing turnout. But it is a much less important part than these studies would have us believe. What accounts for the rest is unclear, although it is hard to believe that increases in residential mobility and decreases in concern over electoral outcomes have not made a significant contribution (see Cassel and Hill 1981; also Squire, Wolfinger, and Glass 1987). It may also be that the increasing number of hours the average citizen has spent watching television is involved.²⁰

It may never be possible to sort all this out. Nor can we realistically expect to explain 100% of anything. But we should not delude ourselves that these simple explanations are sufficient. Most of the post-1960 decline in turnout is still unexplained, and we shall have to do some hard thinking, then cast a wider net, to explain substantially more.

Appendix A: Demonstration That A_1 and A_2 Can Exceed 1.0

A_1 . It follows directly from Equations 1-3 that $A_1 > 1$ when

$$\sum_i \sum_j (r_{ijt} - r_{ij0}) s_{ij0} > 0,$$

that is, when the within-category turnout rates have generally increased, with the overall turnout decline coming from increases in the sizes of relatively low turnout categories. For example, let there be just one explanatory variable, with just two categories. Let the first category contain .4 of the electorate, voting at a rate of .2, at time 0 but swell to .8 of the electorate, voting at a rate of .3, at time t . Let the second category contain the remaining .6 of the electorate, voting at a rate of .8, at time 0 but correspondingly shrink to .2 of the electorate, voting at a rate of .9, at time t . Then $T_0 = .56$, $T_t = .42$, $\hat{T}_t = .66$, and $A_1 = 1.71$.

A_2 . The ordinary least squares estimate $b = 1 + \text{cov}(\hat{u}_t, \hat{T}_t) / \text{var}(\hat{T}_t)$, where \hat{T}_t and \hat{u}_t denote the predicted score and residual from the regression of the actual turnout T_t on the probit-predicted turnout \hat{T}_t , and var and cov denote the sample variance and covariance (see Luskin 1984). Thus $A_2 > 1$ ($b < 1$) when the covariance is negative, that is, when the regression tends to underpredict larger T_t s and overpredict smaller ones. For example, let there be a six-election series for which the turnouts are .60, .58, .56, .54, .52, and .50, and the \hat{T}_t s from Equation 5 are .59, .57, .56, .54, .53, and .51. Here $b = .79$, and $A_2 = 1.27$.

Appendix B: A_1^* and A_2^*

Note first of all that

$$\begin{aligned} T_t &= \sum_i \sum_j \sum_k r_{ikt} s_{ijkt} \\ &= \sum_i \sum_j \sum_k r_{ijkt} s_{ijkt} s_{kt} \\ &= \sum_i \sum_j \sum_k r_{ijkt} s_{kt} s_{ij} s_{ijkt} \end{aligned}$$

where s_{ijkt} = the proportion of the electorate in the i^{th} category of partisanship,

j^{th} category of efficacy, and k^{th} category of some third variable z at the t^{th} election; r_{ijkt} = the proportion of the people in that combination of categories who vote in the t^{th} election; $s_{ijt,k}$ = the proportion of the k^{th} category of z that falls in the i^{th} category of partisanship and j^{th} category of efficacy; s_{kt} = the proportion of the electorate in the k^{th} category of z ; $s_{kt,ij}$ = the proportion of those in the i^{th} category of partisanship and j^{th} category of efficacy who also fall in the k^{th} category of z ; and s_{ijt} = the proportion of the electorate in the i^{th} category of partisanship and j^{th} category of efficacy.

To define A_1^* , then, we estimate the t^{th} election turnout—with the joint conditional distribution of partisanship and efficacy within categories of z remaining what it was in 1960 and the distribution of z changing as it did—as

$$\hat{T}_{t,k} = \sum_i \sum_j \sum_k r_{ijkt} s_{ij0,k} s_{kt},$$

and the t^{th} election turnout, with the conditional distribution of z remaining what it was in 1960 and the joint distribution of partisanship and efficacy changing as it did—as

$$\hat{T}_{t,ij} = \sum_i \sum_j \sum_k r_{ijkt} s_{k0,ij} s_{ijt},$$

Hence we define

$$A_1^* (\hat{T}_{t,k} - T_t) / [(T_0 - \min(T_t, \hat{T}_{t,ij}))],$$

where for a z increasing turnout the denominator now represents the gross decline—the larger decline that would have occurred were it not for the operation of z .

To define A_2^* , we estimate the probit model using the third variable z as well as partisanship and efficacy, then simply fix the mean of z at its value for $t = 0$ (1960), setting $\hat{T}_{t,z} = (\hat{\alpha}_0 + \hat{\alpha}_1 \bar{P}_t + \hat{\alpha}_2 \bar{E}_t + \hat{\alpha}_3 \bar{z}_0)$ = the t^{th} election turnout we should predict if partisanship and efficacy varied as they did while z remained at its 1960 level.

Then we regress T_t on $\hat{T}_{t,k}$ rather than \hat{T}_t , obtaining the new slope estimate \hat{b}_z and defining $A_2^* = 1/\hat{b}_z$.

Notes

An earlier version of this paper was presented at the 1986 annual meeting of the Midwest Political Science Association, Chicago. We are grateful to Paul Abramson, Stephen Bennett, John McAdams, Herbert Walzer, and Herbert Weisberg for comments.

1. Turnout figures from the U.S. Bureau of the Census 1961, 349, 360 and U.S. Bureau of the Census 1986, 254–55.

2. Shaffer and Kleppner incorporate some partial controls for variables increasing turnout but use only these variables decreasing turnout to explain turnout decline.

3. For an earlier objection on this latter ground see Hill and Cassel 1983; for a response see Abramson and Aldrich 1984.

4. Turnout means the proportion of the electorate voting in a given election. Individual turnout means whether or not the i^{th} respondent has voted in the t^{th} election. Turnout is thus the mean individual turnout.

5. Kleppner uses "multiple classification analysis," a form of regression in which the regressors are automatically broken into dummies; Shaffer treats the regressors as interval.

6. This, at any rate, is the logic. Neither Shaffer nor Kleppner makes quite this selection of variables for the regressions that generate the residuals. Shaffer uses education in the regressions but not the explanation (excluding education's negative contribution to the decline from the numerator of SK); Kleppner uses education, income, and sex in the regressions but not the explanation and uses partisanship and efficacy in the explanation but not the regressions.

7. Strictly speaking, A_1 and SK should carry a t subscript, because even for a given starting point they yield different results for each election. The explanation of the 1960–76 decline is not the same as the explanation of the 1960–80 decline, and so on. In this respect, as well as in its ability to accommodate larger numbers of explanatory variables, A_2 is a clear improvement on A_1 and SK.

8. One reason for this specific embarrassment, as Kleppner recognizes, is that he excludes partisanship and efficacy from all the regressions to construct the adjusted turnout rates. The result is that the effect of partisanship is not controlled for efficacy, the effect of efficacy is not controlled for partisanship, and the effect of age is not controlled for either efficacy or partisanship. But there are also more intrinsic reasons. Even when perfectly applied,

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SK can exceed 1.0, as for that matter so can A_1 and A_2 . See n. 9.

9. The residuals from the regression excluding partisanship contain more than just the effect of partisanship. They also contain the effects of all the variables these studies neglect, the measurement errors in both individual turnout and the explanatory variables the regression includes, and the olio of idiosyncratic influences in a pure disturbance term. The residuals from the regression excluding efficacy, reflecting the same neglected variables, measurement errors, and idiosyncratic factors, are partly the same.

10. The cross-sectional analyses in A_2 and SK suggest the magnitude of the omissions. Estimating a linear model that includes education as well as the four variables he says are responsible for the decline, Shaffer obtains a mean within-election R-squared of .15. Abramson and Aldrich do not report the R-squared analog for their final, trivariate probit model, but for the bivariate model containing only partisanship they report an R-squared analog "roughly equivalent to a simple 'correlation' of 0.25" (p. 508). Since R-squared is a squared correlation, the R-squared analog, apparently, is $(.25)^2 = .06$.

11. Although we shall write loosely of "bias," the real issue in much of what follows is consistency (converging in probability on the true parameter as $n \rightarrow \infty$), since asymptotic properties are all the probit estimates can claim anyway. This in no way affects the argument.

12. In confining their analysis to whites, Abramson and Aldrich reduce but do not eliminate the effects of registration and voting requirements.

13. We follow Abramson and Aldrich in using self-reported rather than validated vote (which both they and Shaffer report makes little difference) and confining the analysis to whites. One small departure from Abramson and Aldrich is that we exclude 18-to-20-year-olds (except from Georgia and Kentucky) from all our analyses, whereas Abramson and Aldrich exclude them only from their probit equations. This, too, makes little difference (personal communication from Paul Abramson). By our calculations the percentages of respondents who reported voting for president in 1960-80, are 83.4, 80.4, 77.6, 75.5, 75.1, and 74.0 (based on n s of 1,574; 1,263; 1,166; 1,847; 1,861; and 1,088). These figures also exclude respondents with missing values on partisanship, efficacy, and vote. The 1960 and 1976 data are weighted.

14. We break partisanship into the same set of categories into which Abramson and Aldrich collapse partisanship in their probit model: independents, weak partisans (including independent learners), and strong partisans. We have also followed Abramson and Aldrich in coding efficacy, then combined the two lowest categories.

15. Abramson and Aldrich, in their Table 6, report $\hat{\alpha}_1 = .295$ and $\hat{\alpha}_2 = .374$ —close to our values,

but reversed. Since we arrive at the same \hat{b} , we suspect that Abramson and Aldrich's table has the labels turned around.

16. Concern is coded as follows. For 1960, 1964, and 1968, 1 = *care very much or pretty much*, and 0 = *pro-con-depends, care a little or some, don't care at all, or don't know*. For 1972, 1976, and 1980, 1 = *care a good deal*, and 0 = *don't care very much or don't know*. The means for the election years 1960-80 are .69, .65, .58, .61, .57, and .56.

17. We use the item that reads, "Some people don't pay much attention to the political campaigns. How about you, would you say that you were *very much interested, somewhat interested, or not much interested* in following the political campaigns this year?" With the responses coded (1,2,3) in order of ascending interest, the means for the election years 1960-80 are 2.57, 2.73, 2.56, 2.86, 2.58, and 2.82.

18. For this computation we dichotomize interest as high or low, again to conserve cases.

19. Controlling for education, concern, interest, and age simultaneously produces $A_2^* = .38$ (the algebraic method of A_1 and A_2^* is impracticable with so many variables), but these are still not all the relevant variables, and A_2^* is still too generous.

20. Shaffer's newspaper variable may be reflecting this.

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Forthcoming in March

The following articles, research notes, and controversies have been tentatively scheduled for publication in the March 1989 issue:

John H. Aldrich, John L. Sullivan, and Eugene Borgida. Foreign Affairs and Issues: Do Presidential Candidates Waltz before a Blind Audience?

Henry Bienen and Nicolas van de Walle. Time and Power in Africa.

Henry E. Brady and Stephen Ansolabehere. Rational Choice, Knowledge, and Preferences for Presidential Candidates.

Gary W. Cox and Michael C. Munger. Closeness, Expenditure, and Turnout in 1982 U.S. House Elections.

Kenneth R. Mladenka. Blacks and Hispanics in the Political System: Public Jobs, Minority Power, and Political Effec-

tiveness in 1200 Cities.

Clifford Orwin. Piety, Justice, and the Necessities of War: Thucydides' Delian Debate.

Robert Powell. Crisis Stability in the Nuclear Age.

George Rabinowitz and Stuart Elaine Macdonald. A Directional Theory of Issue Voting.

Michael J. Rich. Distributive Politics and the Allocation of Federal Grants.

Steven B. Smith. The Concept of Right in Hegel's Political Philosophy.

Philip D. Stewart, Margaret G. Hermann, and Charles F. Hermann. Modeling the 1973 Soviet Decision to Support Egypt.

George Tsebelis. The Robinson Crusoe Fallacy: The Abuse of Probability in Political Analyses.

REVIEW ESSAY

SKEPTICAL STUDIES OF LANGUAGE, THE MEDIA, AND MASS CULTURE

MURRAY EDELMAN
*University of Wisconsin
Madison*

Ronald Reagan, the Movie, and Other Episodes in Political Demonology. By Michael Rogin. Berkeley: University of California Press, 1987. 366p. \$25.00.

Contested Truths: Keywords in American Politics since Independence. By Daniel Rodgers. New York: Basic Books, 1987. 270p. \$19.95.

Shaping Political Consciousness: The Language of Politics in America from McKinley to Reagan. By David Green. Ithaca: Cornell University Press, 1987. 277p. \$29.95.

American Media and Mass Culture: Left Perspectives. Edited by Donald Lazere. Berkeley: University of California Press, 1987. 618p. \$48.00 cloth, \$15.95 paper.

Studies of political communication have taken two quite distinct paths, and the divergence is probably widening. On the one hand there are investigations of the kinds of language and symbols that currently attract political support and votes. This form of research is more pragmatic than theoretical. It relies on findings from surveys, voting statistics, and psychological experiments to reach conclusions about how votes, attitudes, and behavior can be changed or reinforced and has become a key influence in the planning of political campaigns, the marshaling of support for policies, and the shaping of postures toward social institutions, public and private. As far as many of their key uses are concerned, these studies yield knowledge in the service of power.

The other tendency is more ample in its theoretical reach and its historical depth and is likely to be critical and analytical respecting the foundations of power. It draws from a range of theoretical orientations of the last two centuries, notably

language philosophy, psychoanalysis, literary theory, western Marxism, and poststructuralism.

The four books I discuss here fall into the second category. Though their subject matters are diverse, these studies complement one another regarding some important themes: language as a crucial form of political action; the political potency of some forms of expression that present themselves as entertainment, culture, science, or fantasy; the pervasiveness of contradiction as a device for indoctrination; and the uses of science and cultural forms as buttresses of established political power.

Political Demonology

In his latest book Michael Rogin continues the insightful and disturbing probing of U.S. political demonology that distinguished his earlier study of American Indians. His essays spring in part from imaginative applications of Freudian theory

but more from Rogin's lively intelligence and his talent for recognizing latent meanings in movies and in language. Rather than accepting the common view in social science that the penchant for constructing blacks, Indians, political dissenters, women, or foreigners as demons is an aberration, an instance of political extremism, or a marginal political position, he sees demonology as fundamental to U.S. politics.

The research Rogin presents here calls for a formidable combination of knowledge, imagination, and logical rigor because it synthesizes aesthetic creations with conventional political action in ways that enhance understanding of both. While positivists will no doubt complain that the conclusions are not falsifiable, this book is marvelously enlightening about the wellsprings of U.S. political causes, passions, and leadership even if some of its specific conclusions are debatable. Rogin's work demonstrates again that it is precisely the ambiguities and diverse meanings of symbols that make them politically potent. It should encourage application of that insight to the ambiguities and diverse interpretations that characterize both falsifiable research conclusions and everyday political discourse.

A recurring theme is the inseparability of fantasies and symbols from substance and the related idea that beliefs about issues generally regarded as widely separated in content and meaning may be closely linked psychologically, metaphorically, or ideologically. While symbolic interactionist, phenomenological, Freudian, and poststructuralist theories have all suggested that perspective and applied it, I know of no work on U.S. politics that does so as provocatively as this one. When Rogin concludes that Reagan's film roles taught him who he was and documents the claim through an analysis of those roles, he is offering a striking instance of his conviction that the

whole culture is "increasingly impervious to distinctions between fiction and history" (p. 9).

The adverb is probably unnecessary. The imperviousness, I suspect, is not so much a characteristic of late-twentieth-century U.S. culture as of how human beings "know" history. The pervasiveness of film, the electronic media, and literacy today doubtless makes the fantasies to which people are exposed more uniform, more banal, and more politically manipulable than they used to be, as Walter Benjamin recognized in his celebrated essay on reproducible works of art. But the construction of reality through analogies and narrative is unavoidable. Indeed, the pretense in mainstream science and in common political discourse that the distinction between fact and fiction is clear is itself a major source of illusion and ideology because it insulates the constructions labeled "fact" from criticism, constructions that are likely to reflect the currently dominant ideologies because they build on accepted ways of seeing. The view taken by Michael Rogin and Jean Beaudrillard that fantasy can be "hyperreal" is more tenable. The realities created by movies, television programs, and other art forms become key influences upon interpretations of reported and witnessed events. Discrimination among them cannot be based upon the simple distinction between fact and fiction.

Rogin regards the various demons in U.S. history (nonwhites, women, the Catholic Church, the USSR, "terrorists") as creations of a "countersubversive tradition." They have often served a common political function and substituted for one another as well as succeeded one another. Rogin sees the war in Vietnam as the culmination of a policy of suppression, intimidation, and control toward Indians and other nonwhites—an example of his talent for recovering what is repressed in our discourse. He might have added that

intimidation of some groups is typically masked either by a vocabulary about "helping" those groups or by narratives about their progress toward freedom.

This kind of sociolinguistic and historical scholarship illuminates the ways in which liberal ideology has on occasion provided sets of symbols that rationalize repression. Rogin suggests that Indians living in a communal order and in harmony with nature threatened independent and lonely liberal men who saw such harmony as an impossible nostalgic dream. So Indians living the dream became a threat that had to be erased by killing Indians and making the survivors dependent on whites. The forced removal of Indians from their ancestral lands became a model bearing a resemblance to the forced removal of the poor in urban renewal programs and the forced urbanization of peasants in Vietnam. In an analogous sense he proposes that "the similarities between immigrants and Negroes initiated the reunion between North and South" (p. 195).

In an absorbing examination of the pioneering movie *The Birth of a Nation* Rogin interprets the film's portrayal of race war in the South as an evocation of the other social conflicts of the postbellum decades as well: class, ethnic, and sectional antipathies and the threat posed by the "new woman," whose sexuality was shifted in the film and in public anxieties from women to blacks in order to reempower the white man. There are comparable observations about transfers of meaning in a chapter on cold war films.

This kind of analysis breaks the boundaries between political issues conventionally defined as distinct from one another and also shows how texts about history and art forms presented as fiction construct one another. The exposition is an important contribution to political psychology. Like some work in semiotics and some in poststructuralist thought, it probes cognitive processes and political

impulses that probably cannot be examined in any other way.

Rogin makes his contribution chiefly by the astuteness with which he assesses the meanings of specific phenomena rather than by a pretence of discovering general laws. While the phenomena fall under a small set of rubrics like demonology and the transfer of meanings among diverse issues, it is the specific analysis rather than any grand generalization that is valuable. In that sense the sum of the parts of this book is greater than the whole.

Language and Politics

Both Daniel Rodgers and David Green are historians whose books examine U.S. political history by focusing on terms that have appeared often in political discourse. Rodgers, the author of a widely praised earlier study of the work ethic in the United States, examines the entire period since independence, while Green's book focuses on the years from McKinley to Reagan. In many ways the two studies complement each other, though they examine the twentieth century through different key terms. Rodgers' foci (*utility, natural rights, the people, government, the state, and interests*) denote philosophical themes or labels for institutions, while Green's terms (*individualist, paternalist, progressive, reactionary, liberal, conservative, and anticommunist*) designate ideologies.

Both studies embrace the premise that language is itself a crucial form of political action. ("The bigger, the more sonorous the words, the more private desires they can bind together, the more new desires they can create," as Rodgers put it [p. 4]). Both emphasize the capacity of widely used labels to evoke diverse and contradictory meanings.

This last point is crucial to understanding both the power and the limitations of language as a form of political ac-

tion. In considering it these scholars are applying an insight of the twentieth-century revolution in language philosophy. Words like these reify: they reduce the uncertainties, flux, and contradictions characteristic of abstract concepts and political maneuvers to a misleading clarity and a substantive denotation, at least for the moment and for the particular situation. But such problematic focusing and reifying does not displace the capacity of language to evoke a wide range of connotations, some of them contending with others and justifying diverse policies. Political language allows all the multivalence that characterizes groping for a stand on a controversial issue to express itself even as it rationalizes whatever specific course of action permits an individual to live with his or her situation. The social condition and the language define one another, a point repeatedly illustrated in these accounts. Both books illuminate the indispensable contribution historical analysis makes to the understanding of political language.

Rodgers and Green pay a great deal of attention to the language of political commentary, criticism, and analysis, and especially to the language of political scientists. They highlight some properties of language in general and of scientific language in particular that social scientists too often ignore at the cost of contaminating their studies.

Green calls attention to the risks of perversion of analysis inherent in the use of everyday political labels as descriptive or scientific terms rather than as objects of analysis in themselves. When we use words like *government*, *the people*, *liberal*, or *anticommunist* as if they were designations for discoverable entities or ideologies, we forget that the terms are themselves political weapons, *constructing* entities and ideologies rather than simply referring to them, and creating them differently as social contexts change. Their ability to construct

realities is, of course, precisely what makes such terms useful to politicians, public officials, and interest groups. A social scientist who accepts them as references to an objective reality without also treating them as devices for reifying a set of dubious premises becomes a purveyor of a doctrine. Skepticism regarding language forms and criticism of their specific uses by political actors and political analysts is vital. It does not yield objectivity; but it does generate awareness that belief in the objectivity of a form of language is a lethal pitfall for researchers, as it is for anyone who discusses the political scene.

Rodgers offers a history of the language of U.S. political science. In doing so he calls attention to the ideological role that political science has played, and he scrutinizes some concepts that have helped keep the public acquiescent to governmental authority. After the Civil War professional political scientists and constitutional lawyers began to formalize amateur talk about politics, their intent and their effect being to set limits on the powers of the general public. They relied on abstractions unfamiliar to that public, especially "the state," a constructed entity that is also a nonentity. "If words are instruments," Rodgers writes, "then one of the clearest uses of the sovereignty-endowed state was to blunt every dissident claim of right outside those rights the State itself chose to bestow" (p. 172). While Green examines the ideological implications of a pretense that common terms are also scientific concepts, Rodgers emphasizes the elitism implicit in inventing uncommon and abstract terms that win wide currency. Both are right because both usages rationalize and reinforce prevailing biases.

Political scientists have changed their preferred concepts from time to time in ways that reflected currently fashionable fears and hopes and so buttressed them. The focus on "the state" faded around the

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turn of the century into a preoccupation with "interests" and with empiricism. These terms continued to justify governmental authority to cope with "parochial" or "special" interest groups and parties and generated suspicion of words connoting unobservable entities or wholes. But with World War II and the cold war that followed it, political science rediscovered the virtues of "the state," now detecting a wholesome unity created by the very divisions into overlapping interests that had previously been regarded with suspicion.

Though Rodgers does not mention it, the widespread focus on the term *political system* (and its offspring, *political socialization*) in the political science textbooks and journals in the 1960s and 1970s reinforced the same inclination to value established institutions and to define their maintenance as a major goal of politics and political science. If language is a political weapon, the language of social science has proven an especially effective one and never more so than when it claimed to be professional, analytic, and scientific.

The focus on key words in the books by Rodgers and Green is both an enrichment and an impoverishment. Historical analysis of the functions of common terms highlights some important phenomena associated with public support for political leaders and candidates that conventional methods miss, notably the short-circuiting of thought implicit in political labels, their capacity to condense conflicting anxieties and aspirations into a potent symbol, their ability to concentrate attention on some concerns and aspirations while masking others, and the leeway for inconsistency and the exercise of policy discretion that the "ownership" of a popular label confers upon political leaders and parties.

But a paramount or exclusive focus on key words implies that they explain the thrust of policy in the eras and the ad-

ministrations that use them, and that view is certainly an exaggeration. Every controversial policy issue evokes its own distinctive language forms. A liberal or conservative label for a regime has only a tenuous bearing on support or opposition for its positions and actions on abortion, capital punishment, military action in the third world, or racism. Each such issue requires an exploration of the specific tropes, myths, scientific claims, and stories that create the issue and of the contradictions implicit in each of them. Analysis of the functions served by words that label major entities, ideologies, leaders, and parties sometimes becomes tedious because it deals with too little of the political scene and builds too superficial a historical account. It also can create a temptation to treat language as a magic force that is independent of the social and economic situation in which it is used, though these writers are aware of that pitfall and usually avoid it. In any case these books are valuable contributions that will influence future political studies by demonstrating that thoughtful scrutiny of language probes aspects of politics that more conventional research methods miss or misconstrue.

Media, Mass Culture, and Politics

The anthology edited by Donald Lazere brings together a wide range of critical writings on the media and on popular art and culture, including film, music, theater, sports, fairy tales, architecture, and pedagogy. They show that forms of culture and expression accepted as science, art, entertainment, or news are likely to be more powerful influences upon political postures and actions even than those that are explicitly political in their content and purpose. To define texts as "political" generates wariness about their ideological content; but works of art and culture diffuse ideology while evok-

ing reactions that are likely to be attributed to their merit as art, science, or entertainment. That response creates a kind of political communication that is privileged, both because it benefits from its association with a valued cultural form and because it generates presuppositions that are likely to escape or minimize criticism of their political content. Though few of the contributors to this collection are political scientists (Stanley Aronowitz may be the only one), they enrich political studies by identifying latent political influences.

Donald Lazere's introductions to each of the eight parts of the volume do a great deal to shape such a healthy perspective. Lazere displays an encyclopedic knowledge of the literature on culture and the media and an impressive talent for evaluating its diverse and conflicting themes.

The subtitle of this collection proclaims that it offers "left perspectives"; Lazere notes that they are generally to the left of mainstream Democratic party positions. But it is more important that their posture is skeptical, inclined to question the accepted wisdom about the roles of the media, social institutions, and cultural trends. No ideology dominates in this collection, though the Frankfurt school has been a strong influence on many of the contributions, and these tend to reflect that school's disdain for popular culture. The anthology is nonetheless marked by striking diversity in approaches, ideologies, concerns, and aspirations, as well as some unevenness in quality.

An article by Todd Gitlin, "Domesticating Nature," cogently explores one of the paramount properties of political discourses—their attempt to compensate with symbols for the values that people are denied in their everyday lives. In the case Gitlin examines, the symbols are not words, but rather flora and fauna. The posh hotel in San Francisco that places plants, a stream, and birds in its vast lobby creates the impression that

capitalist enterprise begets nature rather than destroying it and also discourages the nonwhites and the poor who inhabit the city from entering this artificial space and so shields its elite clientele from their presence and the feeling of guilt they might bring with them. Insulation, reassurance, and quiescence are certainly major political functions of cultural forms intended for elite audiences.

In another thoughtful paper Gitlin offers a sophisticated statement of how entertainment, culture, and news reporting are hegemonic in contemporary society. His thesis is that propagation of the dominant ideology is not so much an imposition as a collaboration. Influential communication incorporates discourse that challenges the prevailing order as well as justifying it, subtly inducing its audiences to cooperate in resolving the contradiction so as to let them live with their existing situations.

Some recent challenges to Gramsci and the concept of hegemony offer evidence that the disadvantaged are typically quite aware that they are being conned. Especially compelling studies that bear that message have come from James Scott (*Weapons of the Weak*) and Kristin Bumiller (*The Civil Rights Society*), both of whom see the acquiescence of the deprived as more tactical than ideological. Awareness that ideological influence rests upon contradictory premises that elicit the cooperation of the disadvantaged contributes provocatively to that debate.

A paper by Jack Zipes on classic and contemporary fairy tales exemplifies a form of analysis to which such art historians as Arnold Hauser and, more recently, Thomas Crow have made rich contributions—the uses of works of art as stimuli of the political imagination and as creators of political presuppositions. The Zipes article explores some fertile themes: the fairy tale as a reaction against the rationalization of modern life and its focus on technique; the power of the fantastic to

help us experience and cope with social reality by uncovering the repressed in human evolution; the bearing of fantasy on the impulse to restructure society to make it livable; the efforts of some contemporary fairy tale writers to express both a struggle against authoritarianism and the limits and possibilities of individual aspirations so as to help their readers reflect upon the realization of a better society.

Many of the pieces in this collection point to issues and forms of scholarship that are likely to be defined as vital to political science in the next several decades because they ignore artificial and inhibiting boundaries between politics and the imaginative life of contemporary human beings, enlarging on one of Rogin's central themes.

Conclusion

These four studies and some others like them remind us that consciousness is not so much a physiological attribute as a changing product of the language forms and cultural institutions around us. In the twentieth century the construction of political consciousness has facilitated an unprecedented sequence of atrocities and policies that failed, while rationalizing the persistence of social inequalities. Franz Kafka may have been the century's most insightful political analyst. Though there continue to be challenges to these depressing developments and though we know that they rest on contradictory cognitions, studies like these offer little hope that the political patterns of the twenty-first century will be markedly different.

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POLITICAL THEORY

Idioms of Inquiry: Critique and Renewal in Political Science. Edited by Terence Ball. Albany: State University of New York, 1987. 225p. \$44.50 cloth, \$14.95 paper.

While there are several books that survey the "state of the discipline" of political science or address intrasubspecialty concerns such as What should political theory be now?—the purposes of this book are unique. The editor hopes to engage mainstream political scientists in "conversations" with political theorists practicing within a variety of "idioms" of inquiry. These idioms and authors include situational analysis (Farr), rational choice theories (Hardin), hermeneutics (Ball), Jurgen Habermas's critical theory (White), Foucault's genealogy (Gibbons), political structuration (Dallmayr), realism (Isaac) and feminist theory (Ferguson).

Like many political theorists, Ball is puzzled by a major anomaly in the discipline of political science—its inability to escape "the behavioral strait jacket" despite the fact that "no one—not even latter-day behaviorists—seems to be especially enthusiastic about this turn of events" (p. 3). Ball suggests that political scientists and political theorists (should?) share at least one common question—Why has political science not progressed beyond behavioralism? Addressing this question could be the beginning of new, more fruitful and ongoing conversations between political scientists and political theorists. In pursuit of such conversations, the contributors tried to structure their essays so that some of the major presuppositions and tenets of the idiom would be clear to a nonspecialist. They also test the limits and utility of each idiom by applying it to a consideration of one or more political or theoretical problem. While a few of the essays are exceptionally clear or provocative, I doubt that the volume will satisfy its wider objectives.

Part 1, together with the last essay by Moon, are the least satisfactory portions of the book. These essays focus on questions of the meanings of rationality and progress in political inquiry and the contributions theory can make

to the practice of politics or political science. Ball argues that political scientists should adopt a variant of Imre Lakatos' philosophy of science. Scientific research is guided by a theoretical "core" protected by a "belt" of protective assumptions. In order for fruitful research to proceed, this core must often be left undisturbed and adjustments permitted in the belt. Ball argues that several promising research programs in political science, Marxism, for example, have been killed prematurely by "dogmatic falsificationists" (p. 34). Since there can be no research program and hence no intellectual progress without a theoretical core, it is in the interest of the entire discipline to nurture a variety of theories. However, Ball does not address the much more fundamental questions raised by Ferguson and Foucault as to the meaning of a will to science, of its connections to the will to power, and of the ambiguities of "progress" and the question whether political theorists should participate in shoring up a belief in the possibility or desirability of a "science" of politics.

These questions also apply to Farr's claim that we should reconceptualize political science as generating "rational" explanations rather than empirical "covering laws." This reconceptualization seems both thin and naive, given the problems with "reason" and the politics of knowledge raised by subsequent contributors. Hardin attempts to clarify different concepts of the "rational actors" in Arrow's and Neumann-Morgenstern's versions of rational choice theory. This essay is difficult to follow since its contrasting of the two theories does not successfully illuminate the basic premises of rational choice theories. Hardin did not provide a persuasive argument for more extended conversations with this idiom. He concludes that while game theories provide much "conceptual clarification," the "value of many of the most technical developments in directly resolving problems" is "limited" (p. 88). Even this hard-won "elegance" may have to be sacrificed if the theory's practical utility is to be increased.

The next section discusses interpretative and

critical theoretical approaches. Ball argues that the actual practice of political science more resembles literary interpretation than laboratory science. Political scientists would produce better interpretations or be happier with what they already do if they would comprehend the true nature of their work. However, Ball does not adequately support either of these claims, nor does he explore the complexities of assigning his interpretation of the practices of political scientists to them. White commends Habermas' "communicative model" as an interpretation of modernity and as an alternative research program. It is better than a purely interpretive one because it creates a critical distance on interpretative moves within a particular game by establishing minimal rules about what is to count as conversation. Unlike positivism, Habermas' research program is self-consciously and explicitly normative and ties discourse directly to political practices. However, White does not successfully deal with the limitations of Habermas' neo-Kantian concepts of rationality and the subject upon which, as he acknowledges, the entire theory is built. Nor does he question what it means or on what basis theorists can or ought to help "to orient the self-understanding" of progressive political movements (p. 132).

These are precisely the questions the authors influenced by Foucault address. Gibbons constructs a clear contrast between interpretive approaches and Foucault's. Foucault forces us to confront the relations of power, truth, and knowledge. His "subject" is constituted (problematically) in and through disciplinary and constraining practices. Foucault also challenges the desire for unified theories including the desire of those who believe such theories will contribute to more emancipatory practices. Ferguson extends Foucault's analysis to gender-based relations of domination and the ways they pervade and structure the discourses of political science, especially its notions of power and the "rational subject." Unlike virtually every other writer in the book, she attempts to locate political science within the most enduring power-knowledge relations of contemporary society. This location is a source of some of the nontheoretical reasons for the continuing grip of behaviorism in the discipline. The provocative insights of her essay support Foucault's claims about "submerged discourses" and their power to disrupt domi-

nant ones. But this same power impels the dominant discourses to ignore subjected ones—as mainstream political scientists and many political theorists continue to do in relation to feminist theories. The un-self-reflective discussions of "realism" by Isaacs or of the "contributions" Moon claims a "social science" can make to (undifferentiated) "modern society" seem ironic, unrealistic, and innocent in contrast to Ferguson's careful treatment of the ambiguities involved in attempts to generate or sustain nondominating discourses.

These essays are sometimes helpful in summarizing and evaluating the assumptions of important idioms of inquiry. Most of the essays are clearly written, concise, and contain a minimum of idiom-specific jargon. However, I wonder what mainstream political scientists would make of any or all of them. Perhaps there is a larger problem with the metaphor of conversation and its necessary correlate, the rational participant. It—like Habermas' theory—posits a "will to know" divorced from power. But if this is inaccurate, what are the implications for those who wish to change the subjects of conversation? This is an essential question suggested but not adequately acknowledged or addressed by most of the contributors to this volume.

JANE FLAX

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The Crisis of Liberal Democracy: A Straussian Perspective. Edited by Kenneth L. Deutsch and Walter Soffer. Albany: State University of New York Press, 1987. 304p. \$44.50 cloth, \$16.95 paper.

The legacy of Leo Strauss continues to inspire philosophical reexamination and political debate. Most of the papers collected in this volume of 15 essays were first presented at a 1983 conference on Strauss and liberal democracy held at State University of New York, Geneseo. Part 1 describes the central philosophical and political questions that occupied Strauss during his lifetime. Part 2 examines Strauss's teaching on liberalism. And the two essays in part 3 attempt to relate Straussian teachings to the U.S. political experience.

Those familiar with Strauss's writings will not be surprised to learn that liberal democ-

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racy—and its current “crisis”—has its roots in a quarrel between ancients and moderns. Modernity has lost its way concerning the issues that matter most to the moral character of civilization. Liberal democratic people have become neutral, noncommittal, or nihilistic regarding the nature of a good human life. This emphasis on the separation from permanent and absolute ethical standards is nicely captured by the editors when they write that “the crisis of liberal democracy is best understood as a crisis of moral foundations” (p. 1). Yet with the exception of contributions by Gunnell and Salkever, this Straussian axiom is never seriously examined or challenged.

Platt delivers an encomium to Strauss that both offers a glimpse of Strauss’s apparent power as a teacher “of the brightest and most many” (p. 24) and also serves to frame Strauss’s thought in terms of three recurring problems: ancients and moderns, philosophy and religion, and philosophy and poetry. Gourevitch and Masters reconsider Strauss’s teaching on natural right. Gourevitch provides a concise summary of Strauss’s views of historicism, conventionalism, and natural right. He suggests that Strauss is more skeptical of the foundation for a narrow theory of natural right than is sometimes thought, a view that nonetheless is consistent with a belief in “a permanent framework of permanent problems that the unaided reason may not be able to solve once and for all” (p. 42). Masters offers a nifty analysis of evolutionary biology, natural right, and natural science. He contends that Strauss’s view of the relationship between natural right and natural science suffers from a misunderstanding of natural science, the product of a misinterpretation of Aristotle’s *Physics*. This misunderstanding of classical science is traced to the slighting of Aristotle’s biology in favor of his physics. Masters argues that once the neglected place of biology is restored to its proper place in science, contemporary natural science can be seen as “not automatically hostile to a doctrine of natural rights” (p. 60).

Gunnell’s essay, published previously in *Political Theory*, explores Strauss’s views on liberal democracy in order to clarify the relationship between political theory and politics. To Gunnell, Strauss represents the tendency in academic political theory to remain aloof from politics, to speak about politics in a highly

rhetorical and abstract idiom. This tendency is a sign of the alienation of political theory from political life.

Part 2 includes essays by Cox on liberality in Aristotle and Machiavelli; Berns on freedom and equality in Aristotle and the moderns; Best on Lockean consent; Pangle on nihilism, democracy, and Nietzsche; and Eden on why Weber was not a nihilist. In a timely exchange due to the popularity of Bloom’s recent criticisms of higher education, Gildin and Gourevitch debate the intersections between Strauss’s conceptions of vulgar and philosophic virtue, liberal education, and liberal democracy. Gildin’s Strauss thinks that classical political philosophy and modern constitutional democracy are in principle compatible as long as the quality of liberal education is properly maintained. Gourevitch sees more tension in Strauss’s account of the fit between the classical virtues and liberal democracy, suggesting that Strauss gave the “gentlemen” a more prominent place than Gildin acknowledges.

In the volume’s third part, Salkever offers a revision of liberal democracy that owes more to practical thinkers like Aristotle, Tocqueville, and Madison than to the more “abstract” theories of early modern liberalism. Salkever’s excellent analysis casts doubts on the merits of considering modern liberal democracy as in “crisis.” A better and more critical understanding of contemporary liberal democracy would see the regime’s possibilities for the enrichment and preservation of democratic traditions. Salkever takes history more seriously than did Strauss, and he is therefore led to pay closer attention to actual political virtues found in U.S. political experience. The formulation of liberal democracy that emerges from this analysis is less high-strung than the Straussian “crisis” alternative and thus skillfully avoids the tendency to “understand ourselves in the misleading light of a contrast between some Nietzschean or Marxian nightmare of a radically privatized present and a dream of a republican community of virtuous citizens” (p. 263).

Bluhm reviews the contribution of social choice theory to democracy. Bluhm believes that the deepest problems of politics will not be successfully addressed by focusing on the *summation* of preferences but demand instead study of the *formation* of preferences.

The volume’s sectarian tone results from a

focus on the thought of Leo Strauss and on Strauss's writings on liberal democracy rather than on more catholic understandings of contemporary democratic theory and practice. Indeed, readers seeking to deepen their understanding of Strauss's political theory have the most to gain from examining this volume. Others less interested in Strauss or Straussian debates will find less reward.

GREGORY LEYH

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The Self-organizing Polity: An Epistemological Analysis of Political Life. By Laurent Dobuzinksis. Boulder: Westview, 1987. 226p. \$37.50.

This is an ambitious book. The author promises to provide an account of cybernetics that will be both free of the defects of systems analysis and sufficiently powerful to redirect the focus of political inquiry, to transcend the chasm between positive and normative political theory and to rescue politics from social determinism and idealistic voluntarism. In order to accomplish this transformation of the subject matter and the methodology of contemporary political science, Dobuzinksis harnesses theorems from biology and thermodynamics to theories of continental thinkers like Morin, Piaget, and Habermas. The result is a conception of political life depicted in terms of the complexity of self-generating systems situated in changing environments and confronting dialectical tensions between autonomy and evolutionary adaptations.

In addition to tracing the development of cybernetics and its deployment in the natural and social sciences, the first three chapters attempt to demonstrate the superiority of "second-generation" models of cybernetics over earlier conceptions and to defend the utility of "new" cybernetics as a research hypothesis for political inquiry. Citing its excessive preoccupation with equilibrium maintenance and its technocratic commitment to an ideal of centralized and rationalized social control, Dobuzinksis acknowledges that there are good reasons to repudiate systems analysis. But he suggests that the problems with systems analysis are related to an impoverished conception of "negative feedback," which limits a system's

response to external disturbances to the restoration of the status quo ante. In contrast to restrictive homeostasis, second-generation cybernetics emphasizes "positive feedback," a homeorhetic response that accelerates internal changes in order to ensure the stability of an autonomous formative process. In other words, homeorhetic response involves a process of "autopoiesis," or "self-production." In response to external pressures, the complex system possesses a capacity to maintain its integrity by transforming itself, producing structures and processes that did not exist before.

According to Dobuzinksis, the great virtue of the new cybernetics lies in the recognition of such autonomy. In affording a holistic approach giving substantive content to the notion of organized complexity, cybernetics provides an account of the "logical and even physical prerequisites to the knowledge and practice of self-production"; and in so doing "it helps us to comprehend freedom as a concrete reality" (p. 42). Moreover, as the "science of control," cybernetics refocuses political inquiry on power relations that are too frequently lost in market models of politics; it restores a balance between concern with the system and interest in the singular event; and it sensitizes analysts to the irreducible uncertainty introduced into political life by the autonomy of self-generating systems.

Through a wide-ranging discussion of autopoiesis in organisms as diverse as cells and stars, Dobuzinksis suggests that his cybernetic model may also provide a more sophisticated understanding of evolution, for "biological evolution, systemic differentiation and growth are isomorphic processes answering to an overarching logic, namely the ontogeny (i.e., individual development) of autopoietic unities" (p. 73). Dobuzinksis suggests that a consideration of the epistemological implications of this isomorphism culminates in recognition that there is a human telos. Whether understood in terms of individuals or of the species, the human telos is linked to the progressive development of cognitive and moral capabilities identified by Piaget and Habermas.

Dobuzinksis suggests that the evolutionary potential of the autopoietic polity is manifested in a "national psyche" that generates and legitimates the structures of the nation-state. As self-producing systems, nation-states

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create not only laws, institutions, and procedures to adapt to environmental disturbances but also the internal and external crises that warrant subsequent responses. They create the conditions for their own transformation and, perhaps, transcendence. Dobuzinkis cautions that "to analyze the evolution of the species in terms of a developmental logic that includes a moral dimension does not imply that history moves towards the triumph of good over evil. It simply means that we can—but will not necessarily—become more and more conscious of, and act upon, the hierarchy of values sustaining political life" (p. 98).

If evolutionary cybernetics does not imply an irreversible logic of history, it does nonetheless suggest specific tasks for politicians and political theorists. Arguing that legitimate state action must be conceived in terms of the production and reproduction of national identity, Dobuzinkis suggests that politicians must "cross-examine" the national community, affording opportunities to reexamine values and to confront the implications of conceptions of the good incorporated in existing institutions and policies. Similarly, the task of political theory is "to confront actors not only with the unintended, but also the unavowed consequence of their action, to challenge their motivation and to assess the significance of the norms that are applied to the evaluation of available options" (p. 196). Utilizing empirical, hermeneutic, and psychoanalytic methods, political theorists may thereby enable political actors to realize the evolutionary ethical imperative of "fraternalization" and to transform their nations accordingly.

The Self-organizing Polity touches upon a number of important issues. It recognizes that positivist commitments preclude analysis of important political questions pertaining to human nature, the relation between individual identity and collective identifications, and the role of indeterminacy and autonomy in situations of choice. It recognizes that a new methodology for the discipline must be non-reductionist, sensitive to issues of language and interpretation, sophisticated in its treatment of the interpenetration of the normative and the empirical, and attuned to history in examining claims concerning invariance and change. It recognizes the importance of preserving a conception of truth that sustains distinctions between ideology and science. It recognizes that

the natural sciences may still afford metaphors of enormous heuristic value for the social sciences.

Yet the treatment of these issues is far from satisfactory. Dobuzinkis develops a cumbersome jargon that is more obfuscatory than illuminating. He recommends a tolerance for paradox even in cases where apparent contradictions could be easily dispelled by more rigorous analysis. He endorses a combination of positivist and postpositivist analytic methods without resolving their fundamental incompatibility. He offers an evolutionary teleology of politics that rests upon a number of conceptual errors. And perhaps most importantly, he offers a defense of the new cybernetics that dismisses rather than refutes some of the most telling criticisms of the model and hence affords no real reason to return to a defective systems approach.

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Critical Social Science. By Brian Fay. Ithaca: Cornell University Press, 1987. 242p. \$35.00 cloth, \$12.95 paper.

Brian Fay's *Critical Social Science* is a clearly written, lucidly argued, and remarkably different book on the problems and contributions of critical social analysis. Despite its title, which indicates a book oriented to the philosophy of social science, Fay does not focus on well-worn epistemological and methodological issues such as whether or not a social theory can be scientific and critical at the same time. Rather, he has written one of the most complete accounts of the *philosophy* behind a broad range of critical, scientific analyses of social practices and structures, as well as many social movements from Marxism to feminism. Fay's central point is that all forms of critical social science properly conceived share a common set of deep, underlying assumptions that both ground their endeavors philosophically and give a direction to their calls for social transformation. Such a philosophical analysis yields insights into the self-understanding of all social critics who aim at human emancipation. It is also useful for teaching this type of philosophy and social science, as well as for reflecting on the successes and failures of projects of

social change in modernity. The book is written from a sympathetic yet critical perspective and assesses both the strengths and weaknesses of the ideals and values of emancipatory theory and practice. Whether one agrees with Fay's conclusions or not, it is clear that critical social science cannot do without this type of reflection. Fay has successfully revealed the sometimes unexamined presuppositions of many of the "theories capable of transforming human life."

Fay begins his book with a fairly uncontroversial set of requirements, which delimit the type of social science that interests him. In light of this definition, Fay draws the more controversial inference that many prominent critical theorists—like Marcuse and Adorno—are to be excluded. The purpose of Fay's strict definition of the term *critical social science* is to give a description of critical theory in its most complete and adequate form. To qualify, critical social science must meet four criteria: it must be scientific in the usual sense; critical with regard to the present state of society; practical, hence oriented to changing the circumstances it finds so defective; and, finally, nonidealist, in that it does not make ideas alone sufficient for social change (p. 26). Adorno and Marcuse are neither sufficiently scientific nor really practical (p. 4); Nietzsche's genealogies are not oriented to change or enlightenment (p. 33). One could also use these criteria to show that Foucault is also, in many important ways, not a critical theorist. By giving this set of necessary and sufficient conditions of critical social science, Fay then hopes to be able to elucidate the "ontology" required to make this type of enterprise possible. Fay means by *ontology* something like a philosophical anthropology, "a basic view of existence."

What is this basic view of existence in critical social science? Fay argues that the enterprise must be based in a specific, fairly detailed set of beliefs, which he calls "the humanist version of the self-estrangement theory" or "the activist conception of human beings" (p. 17). This ontology becomes what it is in modernity—an anthropology and not a theology or metaphysics. This is the same anthropology that is present in Charles Taylor's recent work. It sees human beings as self-interpreting, self-creating creatures with certain dispositions and capacities. Correspondingly, society must be

basically artifactual and conventional. It is not a fixed or natural order but one capable of being radically changed and transformed. Both imply an ethics. In anthropology it is an ethics centering on the value of self-clarity. The theory of society suggests the value of collective autonomy. Fay then uses this ontology to arrive at a "basic scheme" of all critical social science, that is, a number of theories and sub-theories that elucidate the present human conditions of suffering and frustration, their dependence on "false consciousness," and the political means and potential audience available to engage in self-creative, free activity to change it.

Taken at a very general level, I find this type of analysis quite fruitful. It does seem that a theory of change "ontologically" presupposes a being capable of change, and so on. But once Fay begins to specify the specific assumptions of this supposed shared "ontology" of all critical social science, I have my doubts. It seems doubtful that everything Fay includes in the ontology is a necessary presupposition of critical theory. As Quine points out, theories underdetermine their ontological commitments, so that Fay seems to give only one of many possible, empirically equivalent "ontologies" of critical social science. This underdetermination of all ontologies suggests that Fay has perhaps taken the wrong approach if he wants to get at the deeper underlying assumptions of critical theory. What is needed is not an "ontological," but a transcendental, analysis of the conditions of possibility for social criticism and social change (if that is possible). While Fay certainly indicates a number of capacities and facts about human beings presupposed by critical theory, he needs to distinguish more clearly between necessary and empirical conditions of possibility. He often confuses them, since they are both "ontological" in his sense. Thus, for all the insights Fay's ontological approach generates, particularly concerning the excessive assertions about human powers, I do not think he succeeds in identifying either fundamental, necessary assumptions or basic, nonalternative values that all critical social science inevitably shares.

Perhaps the most challenging and important part of the book is Fay's analysis of the limits of critical social science: the ontology of critical social science gives it the pretension of executing unlimited possibilities of rational

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change, self-clarity and collective autonomy. Fay is right to criticize some of the overly Kantian, rationalist emphases of most critical social science. Unexamined, large-scale assumptions about the human species should be eliminated and replaced by modest, empirically specific ones. Here hermeneutics and neo-Aristotelianism might supply much-needed correctives, for they give insight into the finite, embodied, historical situation of human speakers and actors. However, they are corrective only as long as we do not adopt their equally problematic a priori ontologies of finitude, which Fay finds "inherent" in reason, meaning, and action and which make critical social science impossible. Here again I am troubled by just this a priori status Fay gives to the limits he identifies as part of the human condition. Sometimes it seems that Fay takes them to be inherent, hence insuperable, limits. Fay's discussion never answers a crucial question: How can we tell an "inherent" from a historical limit? Do these limits really entail that clarity or autonomy are not worthy goals, particularly on a more modest scale? Values of critical social science retain their importance even if we recognize trade-offs between them and a plurality of other social values and goals. Autonomy and self-clarity need to be important values in themselves. Otherwise the trade-offs between them and happiness would really not be as tragic as Fay suggests they are. In the end Fay really shows the essential tensions of these values with others, not their inadequacy or limits.

For Fay, the analysis of these limits serves an important heuristic purpose: the upshot of the book is that the inadequacies of these "impossible" ideals demands a rethinking of the ontology of critical social science. This program may be desirable for a complete critical social science, and Fay goes some way in carrying it out. But it is so not because of inherent, but because of historically relative limits on reason and freedom. Like Gadamer, Fay often seems to be confusing historical and ontological limits. The relation to tradition is a historical question of relative dependence or independence, not an ontological question. As Marx put it, man makes history, but he does not make it as he pleases. This remark, and many others like it, already do what Fay finds so lacking in critical social science: they relativize some of the one-sidedness and bad effects of their ideals

through self-critical reflection, not by changing ontologies. Moreover, the absence of any reference to the long tradition of critical social science's reflection on its own activities, on their own limits and bad effects, is a major weakness in Fay's book. Rather than really changing an underlying ontology, it is more appropriate to characterize what Fay is doing as bringing the values of critical theory into tension with aspects of historical existence.

All in all, Fay's book is an important contribution to critical social science, particularly as critical social science has come to question its "grand theories" like historical materialism, as well as its "grand projects" and "narratives" like infinite progress and complete emancipation. His analysis points in the direction of correctives needed to overcome the one-sidedness and exaggerations of much critical social science. These correctives suggest a less grand, more modest, and fallibilistic vision of the tasks of criticism and change. Fay's book succeeds when it helps us appreciate the general truth of Kant's insight into the justification of claims to knowledge: that the best way to defend a rational enterprise is to see its limits clearly and to point out its excesses. Fay ultimately provides just such a powerful defense of critical social science.

JAMES BOHMAN

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Lives, Liberties, and the Public Good: New Essays in Political Theory. Edited by George Feaver and Frederick Rosen. New York: St. Martin's Press, 1987. 272p. \$29.95.

This is a collection of eleven essays offered in tribute to, and by colleagues of, Maurice Cranston upon his retirement from the chair of political science at the London School of Economics. In addition to the essays the book contains a complete bibliography of Cranston's works and a postscript by Cranston recounting his intellectual journey, his disposition toward Lockean liberalism, and his long-standing concern with the problem of liberty in the modern world. It is this problem that provides the integrating theme for these original essays, most of which are of very high quality, with even the least satisfying of them having something

interesting to say about liberty. Also, given Cranston's devotion to the life and works of John Locke, it is not surprising to find that most of the essays speak with a decidedly Lockean accent.

Two of the better essays in the collection are on Hobbes. The essay by Mansfield is an attempt to appreciate the influence of Hobbes on modern notions of the role of the executive power in a republican government. Mansfield's main point is to show how Hobbes, despite his insistence on absolute sovereignty, prepared the way for an acceptance of an executive power that was less than absolute. Mansfield argues convincingly that Hobbes' sovereign is less than absolute, in part due to "Hobbes' own contribution to his sovereign and on what his sovereign owes [both] to him" and to others to whom he must turn for counsel (p. 31). That no philosopher of the modern executive power, from Locke on past Hamilton, read Hobbes in this nuanced way casts doubt on Mansfield's historical claim that Hobbes influenced the modern accommodation to a strong yet limited executive. Nevertheless, the strength of Mansfield's essay is in teasing out of Hobbes the latent weaknesses of a sovereign presented as absolute.

Orr's essay on Hobbes follows nicely upon Mansfield's in exploring the validity of Hobbes' claim to have offered to prospective sovereigns the first true science of politics. Orr offers several examples where Hobbes deviates from pure geometric demonstration in his arguments, concluding that "he gave us more of a miscellaneous polemic than he cared to admit" (p. 52).

The best—and longest—essay in the collection is Wokler's "Rousseau's Two Concepts of Liberty." With the only essay showing a pronounced anti-Lockean flavor, Wokler does a masterful job defending Rousseau against those who read him as a friend of totalitarianism. He accomplishes this through a lucid display of Rousseau's multidimensional understanding of liberty. Wokler makes it clear that the general will emerges from independent-minded, introspective, and free individuals. For Rousseau, "in order to be a citizen of a *res publica* one must look deep within oneself for a personal commitment to a collective goal" (p. 86). Wokler's essay should be read by anyone seriously interested in Rousseau.

In one of the most interesting essays, Ken-

neth Minogue explores the degree of loyalty a liberal state positively requires of its citizens. After considering factors that have diminished the power and dignity of the modern liberal state, Minogue concludes that "the liberal state must be held together by a process of negotiation involving all sorts of groups" (p. 223). The object of the public's passion for the liberal state must be its "procedure for dealing with disagreements," or "the rules of the game" (p. 223).

William Letwin's essay is a tame discourse on the exercise of discretionary judgment in constitutional government. He considers the limits that constrain the judgments made by public administrators, Supreme Court justices, and constitution makers. Letwin's discussion is clear, employs interesting examples, and would be especially useful in undergraduate courses on law for its perspective on the nature of a constitution and of public officials' responsibility to a constitution.

A more complete treatment of law and its relation to liberty is contained in Shirley Robin Letwin's essay. She provides a compact but remarkably insightful historical overview of the philosophy of law, showing how the ancient ideal of the rule of law has, to our great misfortune, come to be seen as the enemy of justice, and how "The history of the idea of law in modern times may be read as an odyssey in search of a way to restore a rational moral element to law without returning to the classical cosmos" (p. 237). Letwin shows how moderns like Kant, Savigny, Marx, legal positivists, critical legalists, and Dworkin have failed to provide this rational moral element, and argues for a return to "government by a stable system of rules" that would articulate principles of moral virtue.

Lakoff, in speculating on the influence of Rousseau on Tocqueville, explores the different ways a cautious historian and a speculative philosopher approach the topics of liberty, equality, and democracy. Rosen treads on some fairly well worn ground in an essay on Bentham's influence on J. S. Mill, with a specific focus on "the extent of Mill's indebtedness to Bentham for the underlying structure of *On Liberty*" (p. 121). Rosen argues that it is in the connection each makes between security and liberty that we see Mill's dependence on Bentham most clearly.

Feaver offers a psychobiographical study of

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the personal relationship of John Stuart to Harriet Taylor Mill and that of Sidney Webb to Beatrice Potter Webb, attempting to show connections in each case between their political ideas and the quality of their relationship. He makes an interesting case that Mill came to defend individual liberty because of his sensitivity toward society's disapproval of his marriage. But—as is typical of this approach—the general argument is all too neat, and one's instinct is to resist facile claims like "Sidney's disappointment in love had helped to destroy whatever lingering hold the theory of social and political individualism might otherwise have had on him" (p. 156).

Essays by Cotta and Polin each employ problematic philosophical abstractions to make what are in essence conservative attacks on Marxism and on modernity. Both are provocative, yet unsatisfying.

I would recommend this collection to anyone interested in some fine new essays on liberalism and to scholars interested in the writings of Maurice Cranston.

PHILIP ZAMPINI

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Democratic Education. By Amy Gutmann. Princeton: Princeton University Press, 1987. 321p. \$19.95.

This is more than just another contribution to the genre of philosophy and public affairs. *Democratic Education* is exemplary. Political theorists ritually acknowledge the theme of education as critical, but they seldom tackle it. By virtue of its subject alone this book is important. From an analytical perspective, Gutmann's arguments defending democratic control of education against rival claims are scrupulous and ultimately compelling. The things we want from education are various and complex, she insists, pointing up the inadequacies of conservatives who are exclusively concerned with protecting the rights of parents against the state and of liberals who are so preoccupied with educating people to be autonomous and impartial that they risk disillusionment with morality. The organization of the book is very tight. Chapter by chapter Gutmann chips away at these rival positions, exposing their limitations step by careful step.

From a policy perspective, the book delivers advice and invites controversy on a host of subjects: vouchers and private schools, federal regulations, student decision making, teaching of creationism, sex education. The list goes on.

But what makes this study exemplary is that analytic arguments and policy proposals are in the service of an overarching political vision, that authority over education should be democratic because the purpose of education is conscious social reproduction, which in the United States means the reproduction of democratic citizens.

Gutmann derives constraints on democratic authority over education not from liberal premises but from democracy itself. Because social reproduction should be conscious, education must be nonrepressive. Because education aims at reproducing universal citizenship, it must be nondiscriminatory. Majority rule can be constrained to ensure democratic sovereignty in the future. She also considers securities. For example, educational professionals are a check on democratic authority and at the same time the most dependable source of democratic curricula. In contrast to other professional groups whose claims of expertise insolently challenge democracy, teachers strengthen it. For this reason they should be granted more of the rewards of professionalism, including merit pay and greater autonomy in such matters as graduation requirements and working conditions.

Within the limits of nonrepression and non-discrimination, Gutmann charges democratic authority with deciding most things. The trouble with book banning is not only that it is repressive (in fact, it is not always repressive) but also that it is done arbitrarily. The same is true of the actual practice of textbook control by states. The latitude she allows democratic authority emerges most clearly when it comes to distributive justice. The principle of non-discrimination prevents exclusion from primary education, but beyond establishing a threshold it does not say how much should be spent on education, who it should be spent on, or how children should be distributed within and among schools. These are for democratic processes to decide in light of people's understanding of what citizenship requires and their willingness to pay. "Equal opportunity" offers little guidance here. It could mean equalization (but at what level and at what price to over-

come special disadvantages?), meritocracy (communities might not want to justify any inequalities above the threshold), or the implausible principle of maximization. Gutmann would give citizens discretion. Moreover, she looks at the structures of authority that would make democratic control effective.

Thus in a section on financing schools she shows that local communities will not be sufficiently empowered unless citizens decide not only how much to spend on education but also how this relates to spending on other goods. Local referenda on school budgets register only the desire to slow spending and taxation, not how to budget resources overall. Paradoxically, community control of schools turns into an argument for participation in financial and distributive decisions at the state, and often at the federal, level. Where some contemporary democratic theorists evoke "community" rhetorically, Gutmann is concrete.

Communitarian theorists today also want to move beyond liberal individualism by showing that community "penetrates the self." But typically their discussions are vague about the processes by which character is formed, values internalized, and habits engendered. Gutmann points realistically to the several contexts in which personality develops, setting schooling alongside (and sometimes against) parents. She measures education's limits vis-à-vis other formative forces, like television. She shows that if the kind of self we want is one genuinely identified with citizenship, then education contributes certain things more reliably and efficiently than other institutions. What kind of pedagogy creates democratic identity? Gutmann offers an account of the "morality of association" as it operates in schools, a morality based less on principles than on habitual interactions. This discussion could have been expanded. It is one of several in which she exploits the psychological lessons of conservatism in the service of democracy.

The author sets boundaries to her subject, saying only as much about democratic theory as she must to defend democratic authority over education against alternative views. As a result, her picture of citizenship is sketchy and must be culled from her examples. They suggest that Gutmann is wonderfully attuned to the diversity of political processes, to pluralism and complexity. But this political insight poses a problem for her theory of education. Once

we are alert to the various ways in which democratic authority is exercised, it is not enough to say that education should produce citizens with the capacity for deliberation and social commitment. For participatory democracy is not of a piece: adversarial democracy calls up different attributes and skills than consensus building; electoral politics requires different qualities from both voters and candidates than does representation on planning boards or school committees. How important are powers of speech and persuasion relative to negotiating or organizational skills? The point is that "deliberation" is not one thing, and from a pedagogical perspective the various demands of participation may be incompatible even if politically we need them all. *Democratic Education* succeeds in its contest with liberal and conservative theorists over who should control education and why. It calls for a complementary account of democratic citizenship.

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Rousseau's Exemplary Life: The "Confessions" As Political Philosophy. By Christopher Kelly. Ithaca: Cornell University Press, 1987. 262p. \$29.95.

It has been said that for the study of Aristotle's philosophy it is important to know the following details about his life: he was born, he spent his life in philosophizing, and he died. Aristotle himself, one senses, would have heartily endorsed the sentiment.

Rousseau, evidently, did not. For he left us his *Confessions*, his longest book, in which he reveals in far greater detail than any philosopher before or since the most personal and at times excruciatingly intimate facts of his life. He tells us, for example, not only of his early life as a vagabond, a lackey, and a music teacher, of his transformation into the most celebrated philosopher in all of Europe, and of his later decline into a restless fugitive fleeing persecution both real and imagined but also of his preference for spanking, of his erotic relations with a woman he called "mama," and of his desire to expose himself. The question arises, Why did Rousseau write this book?

Despite Rousseau's own assertion that the

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work "will always be a precious book for philosophers," the prevailing tendency in the interpretive literature has been to understand the *Confessions* as itself a product of some of the more bizarre psychological traits depicted within it: it is just Rousseau exposing himself again; or it is the desperate attempt by a victim of persecution and paranoia to vindicate himself against his enemies. (A particularly interesting exception to this tendency is the recent *Modern Self in Rousseau's Confessions* by Ann Hartle.)

Christopher Kelly's superb new book on the *Confessions*, while not altogether dismissing such psychological factors, reveals and elaborates in a convincing manner the philosophically serious meaning of this strange work. He argues that it must be understood in terms of three distinct although interrelated purposes.

First, the *Confessions* is meant to be a treatise on human nature. Through this intimate history of the development and corruption of his own character, Rousseau is providing the necessary supplement to the hypothetical history of the species given in the *Second Discourse* and the imaginary history of the individual given in *Emile*—all of these "histories" serving the purpose of demonstrating Rousseau's new theory of man's natural goodness (and thus refuting the theory of original sin classically defended in the *Confessions* of St. Augustine) by displaying concretely how all human vices are acquired over time from society. Furthermore, just as the *Social Contract* and *Emile* go on to show how the state and the ordinary individual (respectively) can be restored to goodness if guided correctly by an extraordinary individual (the lawgiver and the tutor), so the *Confessions* goes on to explore the necessary question of how the extraordinary individual might cure himself.

On the second level the *Confessions* is an epistemological treatise. The truth about human nature, according to Rousseau's well-known theory, has been hidden from us by the many layers of artificiality and corruption we have acquired in society. And because reason itself is not natural to man, the progress of this faculty, so far from leading us back to self-knowledge, has only deepened our alienation from nature and ourselves. Now, if Rousseau is correct in this theory, it is incumbent on him to explain how it was possible for him, for any

civilized human, to have discovered it. In the system of a thinker like Aristotle, who maintains that the truth is by nature and in principle available to all men, the details of his own life are of no philosophical significance. But Rousseau must write an epistemological autobiography, much as Hegel writes the *Phenomenology*, in order to explain how access to the truth became possible through him.

Finally, the *Confessions*, like all of the major writings of Rousseau, also has a practical, reformist purpose. The corrupt peoples of old Europe, whom Rousseau considers too enervated ever to be formed into public-spirited citizens, could still be given a somewhat healthier existence if they could be induced to withdraw still further from public life, withdraw into the narrower, less-corrupting circle of intimate friendship and domesticity. But how does one promote such reform? Most men live their lives by example, molding their tastes and actions to the pattern of certain "exemplary lives." Rousseau thus endeavored to reform men through his personal example, by living a life of conspicuous withdrawal. And he sought to extend the influence of his example by writing his *Confessions*, which by its form—autobiography—as well as by its content would confer new dignity on self-absorption and impress upon men the idea that the private, subjective, inner world of feelings and imagination has more reality than the corrupting public world of honor and status.

Kelly's book is a detailed analysis of the *Confessions* in terms of these three purposes, and what it comes up with is both fascinating and convincing. I particularly admire the clarity, ingenuity and surprising precision with which he is able to explain the succeeding episodes and unfolding stages of Rousseau's life. Most worthy of mention is the superb treatment of the imagination; for the two books, Rousseau's and Kelly's, are in a sense nothing but long, complex meditations on the unappreciated danger and promise of this faculty. All told, it is an impressive accomplishment. Kelly has finally succeeded in explaining why it was necessary for Rousseau to write the *Confessions* and for us to read it.

ARTHUR M. MELZER

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The State in Modern Society: New Directions in Political Sociology. By Roger King, with a chapter by Graham Gibbs. Chatham, NJ: Chatham House, 1986. \$25.00 cloth, \$12.95 paper.

The aim of Roger King's book is

to suggest that a major dissatisfaction with orthodox political sociology has been its neglect of the state. New approaches in political sociology have recognised the need to return to earlier concerns with the state as found in both Marxism and Weberianism. In contrast to classical interpretations, however, contemporary political sociology is characterised by a more sophisticated account of the relationship between state and economy than found in earlier Marxism, and recognises the state as a structured source of inequality and power, and not simply a reflection. Moreover, there is a less satisfied conception of the nation-state than found in early Weberianism and increased awareness of international and "world" systems. (p. xiii)

Consequently, King believes that there has been something of a "*rapprochement* between Marxism and Weberianism" (p. iii), and that the orthodoxy has withered in the face of a double-barreled challenge from theories of the state.

According to King orthodox political sociology was rooted in postwar behavioralism and urged a revision of classical notions of participatory democracy, which were held to be wildly unrealistic for mass industrialized societies. King reviews this line of reasoning and the response by advocates of participatory democracy, relying heavily on others' characterization of a familiar debate. This may explain the sketchiness of his account. Curiously, he does not comment on Giovanni Sartori's *Democratic Theory*, one of the more influential revisionist statements of the period. Nor does he consider the evolution of the orthodox position, which is evident in the new version of Sartori's book, and especially in Robert Dahl's (1984) *Preface to Economic Democracy*, which stands some distance from his earlier *Preface to Democratic Theory*. As a result King's assessment of the orthodox position and its weaknesses is not entirely persuasive.

King rightly senses that the concept of power is central to his argument. Recent theories of the state reject a behavioral understanding of power as inadequate for analyzing

modern societies. Unfortunately, King postpones this discussion until much later in the book, where he reiterates Lukes's position on the "three faces of power," albeit with some reservations and interesting asides on its applicability to community power studies. By delaying this discussion, however, King deprives the critique of orthodox political sociology of much of its force and is led simply to declare the orthodoxy dead so as to get on with the main business of the book, which is to outline certain "new directions in political sociology," all emphasizing the structure of power and the place of the state within it.

At one level this involves an effort to describe the evolution of the modern state, which King does by comparing the historical emergence of the state in different countries. The point of his comparison is to show that capitalism is not always associated with, nor supported by, liberal democratic states. In making this case King draws freely on recent work by Anderson, Badie and Birnbaum, Poggi, and others whose conclusions are presented as more or less definitive accounts of the emergence and development of the modern state. Regrettably, the comparative dimension of King's analysis is quite underdeveloped, as he concentrates on the British case, which, interestingly enough, was one in which capitalism and liberal democracy were commingled.

At a second level King's discussion centers on theories of state development. Indeed, he alternates between chapters that are mainly historical and chapters that focus on efforts to comprehend historical change in theoretical terms. The latter sort of chapters are by far the strongest in the book. King's thematic treatments of corporatism, neo-Marxism, and to a lesser extent pluralism are by turns sympathetic and critical, stressing useful insights and problematic contentions. The breadth of his commentary is impressive, and the book will undoubtedly prove very useful for advanced undergraduate and graduate instruction.

King's treatment of the "local state" in Great Britain is especially interesting—not surprising, given his expertise in this area. His explication of the dual state structure in Britain is incisive and balanced, and it shows how corporatist forms of intermediation at the national level may be linked to pluralistic forms at the local level. In turn, this raises questions about the extent to which the local state is simply a

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branch or extension of the capitalist state. For King such questions highlight the sense in which the local state, while constrained in important ways, is also open-textured. In important ways the local state is an indeterminate one, though that indeterminacy is far from unlimited.

There follows "The State in an International Context," a chapter written by Graham Gibbs. The leap from local to international political economy is not as great as it seems, mainly because Gibbs—like King—eschews theoretical orthodoxy in favor of a contextual analysis of state autonomy. Gibbs rejects approaches that emphasize "internal" explanations of state forms and functions (like modernization theory), but he also criticizes dependency and world systems theorists who simply "read off" the activities of the state from their understanding of an international political economy. Instead he advocates a state-centered approach that is sensitive to the fact that states mediate internal and external forces in unique ways. As Gibbs says, "There is no typical state reaction because in the end there is no typical state" (p. 239), a conclusion tempered by his insistence that "the internationalisation of capital imposes structural imperatives on states which lay down limits" on what state elites may do (p. 238).

On this point, King and Gibbs are in agreement, and it would have been extremely helpful if they had elaborated this theoretical position. Unfortunately, King makes no attempt to map out this *rapprochement* between Marxism and Weberianism in any systematic way. He ends the book with a mercifully brief and decidedly tendentious postscript on the new cold war. If nothing else, this chapter illustrates the tendency among those who study the state to colonize virtually all regions of contemporary political discourse. On the other hand, by refusing to explicate his own theoretical position, King misses an opportunity to make the book more appealing to scholars working in this area.

RUSSELL L. HANSON

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Political Discourse: Explorations in Indian and Western Political Thought. Edited by Bhikhu Parekh and Thomas Pantham. Newbury Park: Sage, 1987. 314p. \$27.50.

Discourse analysis has emerged as an approach to the study of meaningful human action that challenges prevailing orientations in fields as diverse as literary criticism, anthropology, sociology, and politics. With intellectual sources as formidable as Saussure, Heidegger, Chomsky, Barthes, Habermas, and Levi-Strauss, among others, discourse analysis has seemed capable of offering a powerful and unifying opposition to approaches extending from behaviorism to the New Criticism. In the social sciences, the routinization of the behavioristic revolution, with its strong tendency to deal convincingly with only a limited range of human activities, has created a vacuum that might be filled by a theoretical apparatus suited to the most obvious and universal of meaningful actions: language.

This collection of essays promises to make its contribution to filling the vacuum in a most interesting and cosmopolitan manner. Abstract conceptual essays are juxtaposed to the analysis of specific cases taken from Indian political and intellectual experience. Unfortunately, the book is so uneven in quality and soft in its focus that its overall impact is to reduce rather than strengthen one's hopes for discourse analysis as a distinct alternative to currently dominant approaches.

The 20 essays in this volume are divided into three parts. The first, "Political Theory after the Linguistic Turn," contains 8 pieces on the relationship between the analysis of political discourse and such well-established traditions as phenomenology (by Hwa-yol Jung), liberalism (by Terchek), and critical theory (by Bhargava). In fact, essays in this section cover a strange and not very coherent array of topics. Minogue, for example, provides an imaginative defense of parliamentary democracy as a "talking shop," and Badi comments with insight on our tendency to tolerate sycophancy in private relationships but not in political communication. Elsewhere in this section, however, we have essays on Hobbes (Ball), Locke (Terchek), and Marx (Nielsen) that are mainline political theory and neither owe much nor add much to discourse analysis.

The second part, "Critical Discourse and Political Praxis in the West," contains 6 essays of even less coherence. Bay, for example, takes the opportunity to denounce, with no particular originality, U.S. national security policy. Greven does little more than summarize the controversy between Habermas, the utopian seeking a discourse free of domination, and Luhman, the realist reconciled to the inevitability of power. But in the midst of these is Ashcraft's closely argued and extremely useful essay explaining and assessing the developments that led the critique of positivism to focus on the problem of meaning and communicative action and the necessity, as he sees it, to construct an empirically grounded approach to the interpretation of meanings in everyday life.

The last part of the book is called "Emancipatory Discourse and Satyagraha in India." In this section one expects to see the application of abstract theory to the specific case of Gandhi's political activity. While the essays are sometimes of considerable interest, they are mostly analyses of Gandhi's thought or that of his associates, and—apart from the use of the word *discourse*—they owe little to any tradition except general conceptual analysis. Nevertheless, one can learn much from these essays, and Parekh's study of Gandhi's strategy of reform is especially rewarding.

This volume of essays is held together less by a clear and coherent commitment to analysis of discourse as a distinctive intellectual approach than by a shared antipathy to empirical social research—"positivism," as it is invariably labeled. In some cases, for instance, the essay by Nandy, "Cultural Frames for Transformative Politics: A Credo," the antipathy extends to science in general, even natural science. *Discourse* becomes a flag rallying opponents not only of behaviorism but of technology and Western power. Most of the authors are not guilty of this, but the looseness of the concepts of "discourse" and "positivism," as used in this book, invites the juxtaposition of essays of widely different intention and intellectual quality. There is much of value in this unusual collection but less than is needed if the analysts of discourse are to achieve the impact they seek.

FRANK MYERS

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Another Liberalism: Romanticism and the Reconstruction of Liberal Thought. By Nancy L. Rosenblum. Cambridge: Harvard University Press, 1987. 225p. \$25.00.

Another Liberalism is a richly textured, elegantly written, complex, and at times provocative book. Assuming familiarity with the liberal tradition and drawing from a broad range of English, French, and German romantic literature, Rosenblum makes two principal arguments. First, the sentiments, values, and mode of life of those she calls "romantic sensibilities" require a liberal political setting. Despite their antagonism toward some of its central tenets, chastened romantics must make their peace with, or "come home to," liberalism. Second, those who critique conventional liberal thought from the romantic perspective considerably enrich it. "Another liberalism" is liberalism "reconstructed" in response to this challenge: "It does not refer to a formal tradition of thought; its coherence is psychological, and not a matter of intellectual history" (p. 3).

Part 1 accentuates the clash between romantic yearnings and liberal values. Romantic militarism, with its nostalgic hope of escape from the ennui of life in civil society through the attainment of heroic glory in battle, conflicts directly with the liberal emphasis on peace and security. The descendant of such heroic longings—military self-assertion—also conflicts with liberal politics: whereas liberalism looks toward the maximizing of the general happiness through the reconciliation of interests, the militant individual's happiness and sense of self depend on distinction from others and conflict with society as a whole. The romantic "law of the heart"—the anarchic desire to obey only the commands of one's feelings and the accompanying hostility to legalism and impartiality—is presented as the most powerful attack on liberalism. Rosenblum's evident sympathy with the romantic sensibility both causes and enables her to extract to the utmost the political relevance of this literature. Yet she is far from uncritical, noting both the pervasive narcissism and the antisocial nature of romantic visions and also the "dark side" of each even when assessed only from the romantic's point of view.

Chapter 3 and part 3 develop Rosenblum's argument that liberalism benefits from the reconstruction it is forced into by its romantic

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critics. She argues that distinctively romantic notions of individuality—stressing spontaneity, inwardness, and detachment—necessitate rethinking the distinction between public and private. Whereas *private* in conventional liberal thought signifies the social as contrasted with the political, once the influence of romanticism is felt, it comes to denote the realm of the personal and intimate. Part 3 examines three separate facets of the romantic approach to liberalism. The first is heroic individualism as exemplified and celebrated by Thoreau; the second is the notion of “shifting involvements,” which values the sphere of public life as a necessary escape from private life, and vice versa; the third is communitarianism, cast in a romantic light and assessed for its contributions to liberal theory.

While presenting all three challenges to conventional liberalism with equal thoroughness and insight, Rosenblum clearly prefers “shifting involvements.” In this variant of pluralism, private life as a sphere of intimacy and detachment provides relief from political attachment just as the sphere of political activity and connectedness provides relief from the excessive self-absorption of privacy. As Rosenblum acknowledges, the contributions of heroic individualism and communitarianism are far more problematic. Indeed, the excellent chapter on contemporary communitarianism is so subtly devastating that it leaves little to be salvaged from such theoretical tendencies.

Rosenblum is utterly persuasive in her argument that romantics need liberalism. I find the argument that liberalism has benefited greatly from romantic critiques and insights less persuasive for three reasons. First, it depends on giving “conventional” liberalism less than its due. Rosenblum says, for example, that “classical liberalism . . . does not recognize the emotional intrusiveness of public life” (p. 72). But even Locke, in the *Letter on Toleration*, is concerned about the protection of deeply personal privacies, such as those of friendship, domestic life, and the inward state of the soul. He did not need romanticism to point out the value of these things. Second, it is not always clear that what the romantics discussed really contributed to liberalism. Liberals are usually sympathetic with Thoreau’s stand against the state because the cause he stood up for *happened* to be a profoundly liberal one. But what if his romantic anarchist appeal to his con-

science had been *against* some liberal cause? On civil disobedience, and in general, the nature of Thoreau’s contribution to liberal thought remains murky. Third, it is not apparent that the contributions of some liberal-romantics to liberalism originated from their romantic values. In the cases of Humboldt and Constant, Rosenblum makes a very good case that their most important contributions were inspired by their romanticism. But in the case of Mill I am less convinced. Rosenblum calls concern with self-cultivation “deliberately antiutilitarian” (p. 132). But Mill argues strongly in *Utilitarianism* that the cultivation of the “nobler feelings” is related in two ways to the achievement of the greatest happiness of the greatest number. First, without the development of such sentiments, each person will be incapable of experiencing the highest state of happiness possible. Second, the development of the sentiment of sympathy for others is crucial for motivating persons to act in accordance with the principle of utility. Mill’s emphasis on feelings of concern for the happiness of *all* those affected, whether by a person’s actions or by a public policy, is far from a romantic preoccupation about sentiment.

This raises a more general question about the relation of romanticism to liberalism. Rosenblum displays abundant awareness of the clash between romantic self-preoccupation and the need of any convincing theory of liberalism to be able to universalize its maxims. She points to the exclusivist, elitist, and necessarily masculinist nature of some romantic visions and to the typical unconcern with justice of others (pp. 21, 40). She notes that the romantic valuing of social diversity can justify cruelty and impoverishment (p. 210, n. 65). Clearly, she is not in agreement with such implications of romanticism, and she knows that we cannot fully assess its actual or potential contributions to liberal theory without confronting these limitations. While romantic needs may be “not felt by everyone” (p. 190), they are needs that *may* be felt by *anyone*, regardless of class, race, or sex. Thus *Another Liberalism* raises important new political questions. Before we can conclude that romanticism of any but a self-centered kind can be reconciled with liberalism, the question that is raised at the end of the book needs to be considered further: What distributions of goods and benefits (education and day care, employ-

ment opportunities, and decent housing are clear examples) would a society have to ensure for all of its members to enjoy the opportunities for self-development and free self-expression so highly valued by romantics?

Another Liberalism makes us confront fully what the romantic perspective, with all its failings, has to contribute to political thought. In doing so, it made me think harder and question some of my conceptions of liberalism more than anything I have read for a long time. At the same time, the book is a great pleasure to read for both its erudition and its style. I highly recommend it to all students of politics with a serious interest in past or present theoretical debates.

SUSAN MOLLER OKIN

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The Causal Theory of Justice. By Karol Edward Soltan. Berkeley and Los Angeles: University of California Press, 1987. 265p. \$30.00.

Don't let the title fool you. *The Causal Theory of Justice* is firmly within the classical tradition of political theory. Following the footsteps of Cicero and Machiavelli, Soltan mingles empirical analysis with normative theorizing. However, unlike theorists of an earlier time, he makes explicit his combination of fact and value, the empirical and the normative. Moreover, in common with many classical authors, he defends natural law as the appropriate foundation for political philosophy, turning his back on the relativity of the postmodern mind.

The book is divided into two parts. Both are of interest to the student of modern political theory and political economy, though the empirical section alone may also prove useful to those interested in wage equity and personnel systems.

Responding directly to Rawls and Habermas, Soltan proposes an appreciation of our natural understandings of justice rather than the construction of veils of ignorance or ideal language acts. To test normative prescriptions, he argues for empirical research into the universal justifications and standards of rightness that individuals find compelling when and if they are free from the forces of self-interest.

Norms that are found to be effective under such conditions are deemed "right," as they are capable of receiving widespread consent. The assertion of a natural logic or reasonableness, then, is the anchor of his treatise. Like Habermas, he believes that human beings share principles that are, to be sure, clouded by appetites. Like Rawls, he hopes for simple principles of justice applicable to large and complex organizations, though he embraces natural law rather than a contract as the most reliable basis for such theorizing. That is, he would make Rawls empirical and expand Habermas beyond the language act.

While these suggestions in part 1 respond to current theoretical debates, it is less clear why Soltan proceeds with his second or empirical exercise as he does. The reader anticipates the evolution of a set of normative principles that would then be shown to reside in the mind of the average man when freed from self-interest and other external forces. Rather than this deductive pattern, an inductive approach is taken. There is little logical or rational analysis preceding the empirical exercise. Thereafter, a series of broad-brush empirical presentations concludes that job rankings in the U.S. emphasize their importance and difficulty. We are left uncertain how importance and difficulty are tied to a sense of justice. In fact, the finding that such principles alone summarize common reflection on the issue of the comparative worth of jobs makes one think that the principles we naturally share are few and broad indeed. Yet here again the comparison with Cicero and those who shared his passion for natural law may be relevant. Classical natural law analysis was eclipsed because it could not locate natural principles of sufficient power and discrimination. If all we can know of justice is the basic principles we share when not dominated by self-interest and if those principles are only that one defers to assignments that are important and difficult, this may be too thin a theory of justice to merit ultimate concern.

Indeed, one wonders if the classical line of pursuit, the pursuit of theories of justice or sets of impartial principles, is profitable. Soltan himself suggests rather tellingly that his and similar analyses do indeed reject the possibility that "the highest moral aspiration of humanity may well be a morality of love based on empathy and intuition" (p. 65). Further, he notes

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that his theory of justice is a second-best morality, possibly most adaptive to powerful, modern, large-scale organizations. As with Rawls and Habermas, Soltan's sense of justice is bent to the service of modern institutions much as Cicero's was bent to the service of one classic institution, the Roman Empire. Could it be that no theory of justice, causal or other, is worthy of the name where it seeks a level of generality appropriate to large bureaucratic structures—that justice is only definable in face-to-face communities? The Greek tradition as opposed to the Roman—Plato as opposed to Cicero—suggests this conclusion. One wonders if the nation-state can be just in any but a thin sense.

This is a provocative book and a worthy contribution to studies that synthesize the best of modern theorizing and relate empirical analysis to such theorizing. It is a metadefinition as to whether or not principles of justice applicable to large institutions—and therefore, of necessity, at a high level of generalization—are worth the pursuit. To the extent they are, Soltan adds to the ongoing dialogue. It does seem that his empirical sections suggest more than test a sense of justice. A more consistent approach might have put deduction before induction here. Still, this is a book to be read and thought through as a significant way station in our evaluation of neoclassical thought.

ROBERT BENEDETTI

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The Possibility of Cooperation. By Michael Taylor. Cambridge: Cambridge University Press, 1987. 205p. \$39.50 cloth, \$12.75 paper.

Michael Taylor published an earlier version of this work, entitled *Anarchy & Cooperation*, in 1976. His basic thesis, lucidly presented in both editions, is that the Hobbesian problem of order can be interpreted as a repeated *N*-person prisoner's dilemma, wherein order can be maintained (if the future matters) by decentralized conditional cooperation. Thus Hobbes, having failed to analyze the full potential of decentralized regimes, embraced the solution of the coercive state prematurely.

Though the fundamental argument is the same, the new version has been reworked in

several significant ways. First, Taylor provides a more detailed exposition of the differences between collective action problems and the prisoner's dilemma. This is one of the most valuable parts of the book. Formal modelers can easily become fixated upon a particular game, such as the prisoner's dilemma, and try to analyze every collective action problem in these terms. Chapter 2 analyzes circumstances (payoffs) that constitute a prisoner's dilemma; circumstances that create other games, notably chicken (which, unlike the prisoner's dilemma, does not exhibit a Pareto-inferior equilibrium); and still milder problems such as coordination games (which, unlike chicken, need not involve conflict of interest). Thus readers are sensitized to a wider array of collective action problems.

Second, he has strengthened some of his earlier results on the sustainability of cooperation. In the first edition a typical result was that cooperation by all parties could be enforced only if a player's immediate advantages of defecting today were outweighed by the long-run costs of foregoing future cooperation. Now Taylor has established sufficient—as well as necessary—conditions for maintaining cooperation as an equilibrium. The difference is substantial. For necessity, he had to show only that a vector of conditionally cooperative strategies was secure against a specific deviation, for instance, someone defecting forever. To prove sufficiency, Taylor proves that a cooperative vector is secure against any player deviating to *any* other strategy.

Third, the technical chapters have been rewritten to make them easier for nonmathematical readers. (Such readers may, however, regret the deletion of the old version's chapter 5, which was an informal summary of the mathematical analyses.)

Fourth, there is a bit more emphasis on the difficulty of securing cooperation by a decentralized structure. Taylor's more sober assessment, signaled by his careful choice of a title, partly reflects a comparison with the optimism of Axelrod's *Evolution of Cooperation* (1984). But his concerns predated Axelrod's book; indeed, they are most developed in Taylor's earlier work, *Community, Anarchy, and Liberty* (1982).

In other respects the book is unchanged. Perhaps more importantly, the same strategies (an *N*-person version of tit for tat, permanent

defection, permanent cooperation, a class of trigger strategies, and a "nasty" tit for tat that initially defects) are examined. In some ways this is not a serious limitation. With just these, Taylor can explore the symmetric extreme outcomes of universal cooperation and universal defection and specify when they are in equilibrium. He can also explore a few asymmetric possibilities, for instance, where two blocs of players alternately defect and cooperate. But the strategy restriction can lead readers into underestimating the number and variety of equilibrium outcomes. An important result in game theory, the Folk theorem (so named because it is part of the oral tradition of game theory) states that in repeated games of this kind, *any* outcome can be sustained as an equilibrium if the future is sufficiently important, so long as each player gets at least his "security level," that is, the payoff a player can guarantee himself by defecting permanently. This implies that a very asymmetric outcome can be an equilibrium. Taylor recognizes that there are many equilibria and that players consequently face the problem of coordinating on the same equilibrium, but the Folk theorem indicates that the situation is much worse than he implies. (One may want to use Schelling's idea of focal points to rule out some of these equilibria as bizarre theoretical possibilities, but as Taylor notes, this argument takes us well outside the confines of game theory.)

The second point of continuity is that the new edition continues to be an equilibrium analysis. Unlike Axelrod's ecological examination of strategy populations, *The Possibility of Cooperation* says little about dynamics. This would be unimportant if (1) there were only one equilibrium in the repeated game or (2) there were many but they could be Pareto-ranked, so that one could plausibly assume that the players would choose the Pareto-superior equilibrium. However, because neither condition holds here, the issue of dynamics—toward which of many equilibria will a system move?—becomes crucial.

Finally, the formal models continue to assume perfect information: everyone in a group knows how many members cooperated. Yet as in the earlier work, Taylor's informal intuition is that "such monitoring becomes increasingly difficult as the size of the group increases" (p. 105), an intuition that has been validated by recent formal work.

Taylor's appropriately cautious points about the fragility of decentralized methods of securing cooperation leave the reader in some confusion about his conclusions. On the one hand, his careful exposition of the nature of collective action problems, showing that they are not all prisoner's dilemmas, encourages him to argue that he is analyzing the most difficult type of collective action problem. He therefore claims that the analysis is biased in the right direction: even if one assumes the worst, the repeated game analysis shows that decentralized strategies can sustain cooperation under certain conditions. Thus, even granting Hobbes the most pessimistic assumptions about the state of nature, one can show that the Leviathan is not the only solution.

On the other hand, the multiple equilibria and monitoring problems noted above must also be entered in the ledger book, and they appear to count against decentralized solutions. And because Taylor's formal models de-emphasize the first problem and omit the second, they are not in fact a worst-case analysis of collective action problems. Hence it remains an open question whether Hobbes' opposition to a purely decentralized regime (anarchy) was unsound.

Of course this does not rescue Hobbes' coercive alternative. Indeed, had Taylor turned his game-theoretic tools on the centralized solution, he might have revealed that in addition to well-known empirical and normative problems there are interesting strategic weaknesses in Hobbes' argument. For example, how would players in the state of nature agree on a *particular* sovereign? (John Ferejohn has argued that there is an underlying coordination-on-an-equilibrium problem here, given that different rulers could implement different outcomes.) And could the players credibly commit to obeying the ruler? (This issue has been recently explored by Jean Hampton in *Hobbes and the Social Contract Tradition* [1986].)

But it is unfair to ask an author to write another book. By showing how a game-theoretic analysis of political institutions both sharpens our understanding of classical arguments and advances new ones, Taylor has already made a significant contribution.

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Political Ethics in Public Office. By Dennis F. Thompson. Cambridge: Harvard University Press, 1987. 263p. \$25.00.

Political Ethics in Public Office collects several essays previously published by Thompson that have been reworked to yield a volume with considerable continuity of theme and subject matter. Thus it can be read from front to back with greatest profit, but individual chapters stand on their own and can be studied out of order without real loss. This versatility makes the book doubly valuable since many of the chapters in it will be starting points for anyone who wants to reflect seriously on the duties of legislators, the responsibilities of bureaucrats, and standards of behavior for public officials.

The broad topic of the book is the ethical requirements and tensions of public office, but the common theme that threads through most of the essays is responsibility: What is it? Who has it? How do we locate it? and How is it affected by political democracy? The first chapter argues that an officeholder in a democracy who acts as authorized by the office or position has no greater responsibility than do citizens for transgression of moral principles (p. 22). Chapter 2 directly addresses the problem of assigning responsibility to individuals in bureaucracies where decision making and policy execution are diffused through many offices. Chapter 3 asks whether we can hold officeholders criminally as well as morally responsible for the consequences of their behavior. Chapter 4 surveys answers to the question of whom the legislator is supposed to represent. Chapter 5 looks at how much privacy we can expect public officials to surrender. Chapter 6 rehearses the difficulties in justifying paternalistic regulation of individual conduct. Chapter 7 points out the responsibility of government officials to ask certain questions and allow for certain unexpected contingencies before involving people in income maintenance studies and other similar social experiments.

The discussions throughout are careful and measured, conversant with a wide literature, and full of useful distinctions that allow many stalemates and logjams in the public understanding of political ethics to be bypassed or broken through. Even if readers are uncon-

vinced by Thompson's particular views, they will find in the essays indispensable tools for mounting alternative conclusions.

If responsibility is Thompson's motif, democracy is his leitmotif. In a democracy citizens are unusually implicated in the bad behavior of their leaders because they directly or indirectly authorize or consent to much of it. Moreover, the standards of conduct we apply to the behavior of officials are driven, in part, by democratic accountability. This is especially so regarding assignments of responsibility. Thompson prefers one account of responsibility ("personal responsibility") over two other accounts ("hierarchical responsibility" and "collective responsibility") because it "offers a stronger foundation for democratic accountability" (p. 64).

"Personal responsibility" conforms to the presuppositions of traditional moral responsibility: individuals must have both caused something to happen and been able to do otherwise (pp. 47, 48). One of Thompson's criticisms of "hierarchical responsibility"—which makes the highest official in the chain of command responsible—is that it does not "coincide with moral responsibility" (p. 43). Thus, "hierarchical responsibility imparts scarcely any moral force," encouraging the "ritual" of leaders quite facilely declaring themselves "fully responsible" for mistakes under their administration (p. 43). However, the emptiness of this ritual, I suggest, stems less from the fact that such facile admissions do not track with moral responsibility than from the fact that no material costs attach to them. The president, for example, does not lose office for the bungs and crimes of his underlings, so why not "take full responsibility"? The real solution is putting some punch into such "taking." If we think about officeholders with democratic accountability in mind, why should we be more concerned to see that their liability to penalty coincides with their moral responsibility than that it most effectively spurs them to minimize bungling and crime under their administrations? Thompson acknowledges that there may be reasons for severing liability from responsibility (p. 43) but assumes that democratic accountability does not especially fuel such a separation. The liability of officeholders to merited praise and blame may do less for democratic accountability than would their liability to get the boot

whether they are personally blameworthy or not.

That the reader may be led to try out such speculations is part of the pleasure of reading *Political Ethics in Public Office*. Its arguments allow dissenting explorations. Yet its own views have such resilience that all dissenters and speculators may find they have returned to the fold in the end.

ROBERT K. FULLINWIDER

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The Grammar of Justice. By Elizabeth H. Wolgast. Ithaca: Cornell University Press, 1987. 219p. \$29.95 cloth, \$9.95 paper.

With this book Elizabeth Wolgast joins the contemporary debate about the limits of liberal theory and the implications that those limits have for our ways of thinking about ethics and politics. Taking exception to a Rawlsian general theory of justice, Wolgast joins Peter Winch, Alasdair MacIntyre, and Ludwig Wittgenstein in emphasizing the contextual, cultural background conditions of justice. While liberal theory holds out the theoretical vision of justice as a universally applicable norm, Wolgast contends that in reality our moral experience informs and limits what we can formulate abstractly. Practical dilemmas and problematic facts give rise to our philosophizing about justice. And our actual moral reasoning about justice in such situations is heavily influenced by our culture, language, history, and personal background. Following Wittgenstein, Wolgast refers to these factors as the "grammar" of justice.

Addressing U.S. political tradition and culture, Wolgast presents us with a series of tightly argued philosophical investigations of terms central to U.S. thinking: *social contract*, *respect*, *individual rights*, *freedom*, *majority rule*, *political representation*: "The goal of these essays is to get a clearer view of how our moral reasoning works and how it in turn illuminates political judgments" (p. xiii). She particularly wants to show how basic liberal assumptions shape and limit our political and legal thought.

According to Wolgast, the U.S. colonists inherited from seventeenth-century English and Continental philosophers a model she calls

"social atomism," a fundamental conception of society as a simple collection of independent, self-motivated units. Wolgast argues that atomism may not be the only or best way to understand human society. It assumes the primacy of self-interest and the natural unconnectedness of people. On the atomistic view, community is said to be merely the aggregate of self-contained, self-interested individuals who are equal in the sense that they are interchangeable. As Wolgast notes, the application of this concept of equality to human beings, given sex differences, is problematic. The burden of her argument here is that it is unjust to treat dissimilar cases in similar fashion.

So far, there is nothing particularly new or startling in Wolgast's discussion of social atomism. She simply reiterates the by-now customary critique of the methodological individualism that is a presupposition of liberal thought. However, she now begins to expand the critique of liberalism by examining controversial issues of contemporary society.

For example, in chapter 2, entitled "Wrong Rights," Wolgast points to one consequence of the atomistic model: the singular emphasis we put on individual rights as a way of addressing injustices. In liberal thought rights are viewed as possessions, and the individual person, as bearer of these rights, must claim them as his or her due. This is all well and good in situations of equality among peers, she contends. But rights-language is limited in situations of inequality, for example, in patient-doctor or parent-child relations. In these situations society should not expect the weaker party to assert its rights but should place the burden of discharging duties upon the more powerful person. However, since, under the model of social atomism, individuals can be counted on to act only or primarily in terms of self-interest, the notion of duty and responsibility may not sit well with the doctors and parents. For this reason theories of individual rights derived from an atomistic conception of society do not go very far toward realizing justice in society.

In subsequent chapters, Wolgast examines, from the same critical perspective, the concepts of democracy, majority rule, representative government, and punishment. In chapter 5, for example, she presents a novel argument that incorporates what has come to be called a feminist critique of pornography. This argu-

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ment is significant and merits detailed analysis.

Wolgast, a liberal supporter of First Amendment freedoms of speech and press, nevertheless understands sympathetically the radical feminist objection to pornography that portrays bondage, torture, and murder of women for sexual stimulation and pleasure. What is objectionable is not just these representations of brutal sexual treatment of women, she argues, but the lack of a context in which they are understood to be reprehensible and condemnable. "Without some such context, the representations carry the message that such treatment of women is all right" (p. 107). Wolgast contends that this kind of objection carries enough legal weight to justify control of the objectionable materials.

Wolgast maintains that the justification for censoring pornography is not rebutted by an appeal to the First Amendment. Central to her argument is her contention that the appeal to the sensibilities of "the average man" to judge whether pornography is offensive is exactly that: an appeal to the average *man*, not the average woman. Wolgast acknowledges that pornographic materials may be seen differently by one group than by another. They may be felt as insulting by one group but inoffensive by another. It is crucial for her argument that such differences in perception be acknowledged as a social reality and that our understanding of what it is to treat everyone with respect should allow for such differences in the perception of respect.

Seeking to translate her moral argument against pornography into constitutional terms, Wolgast notes that the connection between respect and constitutional action has already been made in *Brown v. Board of Education* (1954). Because racial segregation was perceived to be demeaning and disrespectful, the Court rejected the justice of "separate but equal" facilities. The insult perceived by blacks took priority over protests of innocence by those charged with offending. Similarly, the

insult perceived by women has priority over the protests of pornographers who claim no intent to demean or offend women. Insofar as pornography is felt by women to be demeaning and insulting, its protection by the government under the First Amendment cannot be easily argued in a liberal society committed to equal respect for individual persons. In this manner Wolgast uses the premises of liberalism to argue for appropriate censorship of pornography and against the libertarian case for laissez-faire. This is an imaginative, constructive argument made all the more provocative and paradoxical by Wolgast's appeal for support to Mill's *On Liberty* and *On the Subjection of Women*—essays usually cited by libertarians to support noncontrol of pornography.

Wolgast's main contribution is her use of Wittgenstein's philosophy to illuminate our understanding of morality and justice. Although liberalism has many positive aspects, the liberal ideal of universally applicable moral norms is plainly unrealistic and untrue to actual life because it ignores language, history, and culture. Her critique has implications for ethics, political theory, and moral education.

Wolgast's analysis is marred occasionally by a lack of historical perspective. She ignores the republican tradition of civic humanism in U.S. politics. She also fails to take into account the influence of capitalist ideology on U.S. political and social thought. At the same time, her Wittgensteinian critique of the abstract conception of justice present in much contemporary liberal theory is convincingly made. And her discussion of pornography and censorship is a valuable contribution to contemporary debate. Students of contemporary political thought, social philosophy, and civil liberties will find her work stimulating and thought-provoking.

MARY C. SEGERS

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AMERICAN
POLITICS

And We Are Not Saved: The Elusive Quest for Racial Justice. By Derrick Bell. New York: Basic Books, 1987. 288p. \$19.95.

"The harvest is past, the summer is ended, and we are not saved." Having "taken his text," as in the tradition of the black preacher, from the lament found in Jeremiah 8.20, Derrick Bell has written a most absorbing and compelling book. The book is many things but is foremost a profound commentary on the question of race in the United States, expressed in an examination of the various political and legal strategies adopted in pursuit of achieving full rights and protections for black citizens. The book takes the unusual form of a series of smaller allegories within a larger allegory. It is thus a highly imaginative and largely successful use of fictional characters and circumstances to illumine by means of critical analysis problematical issues in the implementation of racial public policies and to dramatize by means of intriguing events the persistent barriers and impediments to realization of the goals of these policies.

Bell's chronicles, as the smaller allegories are called, provide insightful assessments of a range of strategic considerations proffered or engaged in by blacks as a way out of their subordinate status in the United States. The analytical fecundity of the chronicles is made more engaging by their titles, which include such suggestive allusions as "The Sacrificed Black School Children," "The Ultimate Voting Rights Act," and "The Twenty-Seventh Year Syndrome." The chronicles are the feedstock for an extended discourse between two learned and experienced lawyers that yields thoroughly insightful and compelling analyses of the development of legal precedents in structuring and sometimes subverting the scope and meaning of racial public policies.

To the adherents of contending strategies for black liberation, the author offers a sobering, consensual conclusion: "While the central motivating theme of black struggle is faith, the common thread in all civil rights strategies is

eventual failure" (p. 248). With that truism as a prelude, the ending of Bell's book might be characterized by some as a cop-out. The ending quite arguably lacks the creativity of insight and incisive analysis that is shown in much of the rest of the book. For many, the perspective and the language of the final segment will lack the cogency they expected, and the absence of an analytically compelling wrap-up will no doubt be disappointing given the highly suggestive title of "The Ultimate Civil Rights Strategy."

Clearly, a word is due about the method employed in this book and its appropriateness for scholarly presentation. The extended use of fictional scenarios as the exclusive method for the presentation of scholarly analysis is not without precedent but is nonetheless not a commonplace approach. To be certain, Bell's scholarship is no less sound for use of the allegories. The characters' dialog contains specific and extensive references to actual source materials. Thus the work is fully documented in accord with the most exacting standards of scholarship. Indeed, the use of the allegories make intriguing and understandable what might otherwise have been a tedious, legalistic treatise of interest to a very limited audience of scholars. Perhaps the major disadvantage of the method is that scholars and others will not quote the author *per se* so much as they will likely remember the allegories and their morals. The author's personal perspectives on specific issues are camouflaged by the characters, who give contrasting and opposing viewpoints. The advantage of this is that the arguments presented in the chronicles become less polemical and more didactic. The substance of the book leaves no doubt about who Derrick Bell is—preeminent legal scholar and social critic, frustrated playwright, and ever hopeful christian mystic. This book will be widely read and long remembered.

Bell's book is clearly directed toward a broad audience of scholars and concerned laymen. It is a mix of scholarly and literary styles. It is neither wholly political science, legal

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exegesis, social criticism, or historical analysis but a blend of all these. Thus there is no single explicit thesis so common in disciplinary-specific works. The book obviously provides grist for constitutional law classes but moreover provides an excellent context for the discussion of many themes of particular significance to the field of black politics. In this regard it provides a kind of updated perspective on concerns raised in the twin volumes by Matthew Holden, *The White Man's Burden* and *The Politics of The Black Nation*. Bell's book makes a major contribution to the cross-disciplinary literature of changing race relations and joins the ranks of such stellar works as Harold Cruse's *The Crisis of the Negro Intellectual*, Joel Williamson's *The Crucible of Race*, and Cruse's latest work, *Plural but Equal*.

GEORGIA A. PERSONS

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Policy Analysis by Design. By Davis B. Bobrow and John S. Dryzek. Pittsburgh, PA: University of Pittsburgh Press, 1987. 246p. \$27.95 cloth, \$13.95 paper.

Policy design refers to a process through which ideas are converted into fully developed policy alternatives or to the products ("designs") that emerge, such as laws, regulations, and programs. Policy design has been a much-neglected topic of study, and Bobrow and Dryzek's book contains the most comprehensive discussion produced to date.

In the first part the authors examine how policy design proceeds under five different frames of reference: welfare economics, public choice, social structure, information processing, and political philosophy. The authors' presentation is enriched with the use of an example in which policy analysts, operating from each frame of reference, design alternatives to resolve the air pollution problems in a hypothetical city, Smoketown.

Bobrow and Dryzek's interpretation of welfare economics and public choice are relatively standard, albeit interesting, treatments. Each, however, is judged insufficient as a comprehensive guide to policy because of unwarranted and simplistic assumptions that are seldom met in practice and (for public choice) a

too-rigid view of the policy process. In the social-structural approach, policy alternatives are not judged by their efficiency or responsiveness, as in welfare economics or public choice, but by their impact on the relative standings of individuals and groups in the community. Hence, this approach admits many values and is better able to illuminate policy consequences in terms of who gets what—including money, power, status, expertise, environmental quality, and other values that are being distributed. In the political philosophy frame of reference, policies are judged by ethical premises that emphasize the intrinsic rightness or wrongness of policy alternatives. Bobrow and Dryzek contend that neither the sociological nor the philosophical frames of reference are sufficient guides to design—for a variety of reasons but especially because they are better suited to raising issues than to resolving them.

Information processing approaches differ from welfare economics and public choice in their rejection of the assumptions that individuals are self-interested utility maximizers. Instead, these approaches assume that individuals are far more complex and rely on decision strategies that often introduce bias into their decision making. The authors present several strands of the information processing approach, ranging from an optimistic version characterized by the belief that policy analysis can improve the rationality in decision making and thereby improve policy choices to pessimistic views that only incremental policy changes are possible or desirable. Neither of these is sufficient for all policy situations, according to the authors. The more optimistic approaches may not be acceptable or realistic in some political contexts, but the pessimistic views, they contend, "are destined to leave the world pretty much as they found it" (p. 95).

In the second part of the book the authors turn their attention to epistemology and discuss positivism, piecemeal social engineering, eclectic policy analysis, forensic policy analysis, relativism, accommodation, and critical theory. As in the first section, the authors find fault with all approaches, although acknowledging that each has some redeeming values.

The authors' approach, presented in the final portion of the book, rests on the premise that an appropriate frame of reference and an appropriate methodology will vary depending

upon the values and the context. Their suggestions about values are quite appropriate. One must consider timing (when should the end be sought), amount (how much is "good enough"), and relative priority of competing values. Context needs to be captured, according to Bobrow and Dryzek, in terms of its level of complexity and uncertainty, the feedback potential, degree of control available to individuals in the policy process, stability of actors and interests, and the characteristics of the audience, which includes those who adopt, implement, and react to the policy.

Some values lend themselves to a particular frame of reference and to a particular methodology. If the primary value sought is economic efficiency, welfare economics may offer a useful set of deductive principles to guide policy design, and causal analysis may be appropriate for determining effects. But where there are multiple values, complex situations, ambiguous results, and controversy, then other approaches must be used. When goals are unstable and conflicting, the authors suggest that incremental change would be more appropriate, combined with forensic or critical policy analysis.

This is a valuable book on policy design. It brings into focus in a clear and compelling manner the implications of alternative frames of reference and methodologies. The authors argue that there are no simple prescriptions for policy other than that the approach selected must have a good "fit" with prevailing values and with the context within which the policy must operate. They list the conditions needed for each of the frames of reference and epistemologies but acknowledge that these seldom hold in practice. They do not, however, stand the process on its head and identify the most appropriate approaches to design, given a particular constellation of values and contextual issues. Perhaps that should become part of the research agenda for policy design.

ANNE SCHNEIDER

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Policy Evaluation for Local Government.

Edited by Terry Busson and Philip Coulter.
Westport, CT: Greenwood, 1987. 266p.
\$39.95.

The purpose of this book, say editors Terry Busson (director of the Institute of Government at Eastern Kentucky University) and Phil Coulter (director of the Institute for Social Science Research at the University of Alabama) is to present some of the best innovative research in local government policy evaluation. They subscribe to the notion that such evaluation can contribute to policy success, especially in the face of current declining public resources, by helping managers decide what services should be reduced or cut out, which staff should be reassigned, and where changes in operations should occur.

Lest you think that the editors are not aware of the literature that concludes that most policy evaluation does not have much immediate impact on these kinds of decisions, I should hasten to add that they discuss the perils and pitfalls of evaluation. They discuss such examples as policymakers' using evaluations as a screen for their political decisions and the resistance of administrators to being evaluated. Still, they are optimistic about the use of policy evaluation as a practical tool for management.

How has evaluation been used? Morgan and England describe how citizens were used in Norman, Oklahoma to evaluate the effectiveness of the city's community development block grants program. They find that citizens gave it lower ratings than city staff did. They also find that citizens are not very knowledgeable about these programs. Stipak discusses the use of subjective evaluation measures (such as citizen opinions) and says they should be balanced with the use of objective measures. Gardner and Feorestana describe social evaluation techniques but agree that they should be balanced by the use of technical (objective) measures. Elaine Sharp finds that a small-sized city did not use subjective or objective measures in deciding how to allocate grants to non-profit organizations that deliver city services. They based their decision on United Fund allocation patterns. Lorenz and Rogers find that the federal government had little effect on building local government capacity. Coulter looks at the distributional effects of local gov-

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ernment decision rules. Toulmin finds that Savannah, Georgia was able to target its urban public services more equitably. Rubin, Rubin, Dobson, and Grush look specifically at the factors promoting utilization of evaluations and come up with a list of nine points that should be followed if evaluations are to be utilized. Downes finds that a local government in Oregon used politics rather than evaluation in their reaction to fiscal stress. Thompson measured the outcome of an economic development project in Battle Creek, Michigan and found it did not create new jobs. Finally, Brudney finds that coproduction is not a simple solution to fiscal stress.

Most of the chapters are well done and contain a lot of research and data. But not all of the articles address the main theme of how program evaluation can or cannot help local governments make decisions in the face of fiscal stress. For example, the Brudney article, while a fine piece of research in its own right, is not really related to the main theme, nor are the Thompson and Downes articles. Nevertheless, several of the chapters would be useful in courses in policy analysis and evaluation or in urban politics. It is true, as the editors note, that government agencies are turning to evaluation for a number of reasons. Thus the book is timely. It contains a lot of original research pieces and these should be of interest to scholars and practitioners alike.

DENNIS J. PALUMBO

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The Personal Vote: Constituency Service and Electoral Independence. By Bruce Cain, John Ferejohn, and Morris Fiorina. Cambridge: Harvard University Press, 1987. 268p. \$25.00.

This very good book is certainly essential reading for anyone in the fields of legislative studies, electoral behavior, comparative legislative studies, and British politics. Its focus is on constituency service in the United States and Great Britain but it addresses a remarkably broad range of concerns. Its strength is that it is very rigorously constructed and very vigorously argued. Thus from the focal point of constituency service, Cain, Ferejohn, and Fiorina look out to the evolving institutional

structures of both political systems and help us to understand how they work.

The authors argue that constituency service, the representative's ombudsmanlike efforts to help constituents both individually and collectively, is an important form of representation with significant political consequences. Indeed, this sort of advocacy is the most ancient form of representation and certainly deserves a prominent place alongside policy representation and symbolic representation. However, this form of representation may have negative consequences for the process of policy-making: a drift from party responsiveness toward bargaining among parochial interests, a loss of coherency in legislative programs, and a decentralized pluralism among interest groups, which can produce stalemates. Entropy drives representatives away from national institutions and policy concerns and binds them to local and particularistic interests. At the heart of this entropic process lies the electoral incentive.

Based on a series of carefully designed interviews with representatives (administrative assistants of representatives in the United States and MPs and party agents in the United Kingdom) and with their constituents, the book begins by examining the substance of constituency service. The authors find that constituents expect representatives to help their districts and their people. They find that representatives do this work primarily through large staffs whereas MPs do it primarily by themselves. In consequence, more casework gets done in the United States, albeit in Britain MPs spend more time on such activities. They find that representatives in both countries are aware that constituency service can win votes and that the level of effort that representatives devote to casework is related to their electoral vulnerability, to their margins of victory in the previous election. They also find that as partisan ties have declined over the past several decades, constituents have come increasingly to care about constituency service when evaluating their representatives. Correspondingly, representatives have increased their constituency efforts.

The second of the book's three parts investigates the theoretical guts of the matter, the connections between the constituency service activities of representatives and the evaluations and voting decisions of their constitu-

ents. Here Cain, Ferejohn, and Fiorina begin by arguing, in general, that "when research results conflict with theory, common sense, or even political folklore, scholars should entertain the possibility that something could be wrong with the research" (p. 123) and arguing, in particular, that where previous research has found little or no evidence that constituency service has electoral consequences, these negative results have been due to unsatisfactory concepts, measures, and methods. They show that voters notice the service activities of their representatives in Britain and the United States and that vigorous performance of these activities enhances the representatives' local reputations. Finally, the authors further show that such activities are not only noticed and appreciated but also contribute to a very significant personal vote in the United States and to a noticeable personal vote in Britain.

The final part of this excellent book is the most creative and stimulating, for it attempts, on the foundations of the analyses that have gone before, to explore the larger implications, including linkages between institutional rules and individual behavior and the consequences for democratic political systems. The authors find democracy in the trend toward increasingly faithful representation of particularistic constituency interests. Yet they conclude that these trends can, unless constrained, produce crises of immobilism and "corrode the conduct of democratic government by undermining the ability of that government to act in ways that improve the lot of its citizens" (p. 229). It is the rules of the game of single-member-district electoral systems, such as those of the United States and Great Britain, that produce electoral incentives for developing a personal vote—and produce thereby inherent tendencies toward decentralization, toward neglecting the interests of the whole for the interests of the parts.

Although this book is unquestionably a genuine and very important cross-national study, it has more to teach us about U.S. politics than about British politics. There is, for example, a usually misleading tendency to assume that the British system is becoming like the U.S.—policy-based voting for MPs, increasingly important incumbency effects, MPs cross-voting in order to pursue personal votes by voting their constituents' policy beliefs. Cain, Ferejohn, and Fiorina treat these ideas as

speculative projections rather than as realities. But the fact that they get the realities right does not eliminate the bias: a tendency to interpret other systems as similar to our own or as becoming similar to our own. Another bias that I see in the book is a tendency to overlook the complexity and variability of the motives of political actors; to take seriously only those motives that are individualistic, selfish, and antisocial; and to assume that such preferences are more or less constant across different countries and across different time periods. I must hasten to emphasize, however, that these criticisms, which may sound very critical, are, in fact, very trivial in their consequences for the book's analyses and conclusions. The authors are creative and careful scholars who do not make careless errors. Their book is an absolutely first-rate study. It deserves to be read. And it deserves to be taken seriously.

DONALD D. SEARING

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Biocracy: Public Policy and the Life Sciences.

By Lynton Keith Caldwell. Boulder: Westview, 1987. 229p. \$33.50.

This highly readable collection of essays by one of the leading scholars in the field of environmental politics and policy provides a surprisingly coherent discussion of the dynamic relationships among scientific discoveries in the life sciences, public policy, public opinion, political theory, and ethics. I say *surprisingly coherent* because the essays were written over a twenty-five-year period. All, however, are "new" in that even those previously published or delivered as professional papers have been revised for this volume. The coherence is also derived from the single-mindedness of the author to make the case for developing a theory of politics grounded in ecological and biological fact and theory. This volume is a welcome addition to the steadily growing body of literature in "politics and the life sciences" and adds significantly to published or forthcoming book-length works by Peter Corning, Roger Masters, Glendon Schubert, Thomas Wiegale and others.

Caldwell's primary purpose is to alert his readership to the vital importance of recent developments in the life sciences and their

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implications for society. The challenge he describes is one of reconciling the potential discontinuities between biology and culture, as manifested in the new knowledge being presented by life scientists and the existing practices of politics, public policy, and popular culture. In the preface Caldwell states that each essay "is in essence a different version of a common message: Advances in the life sciences are bringing about social changes and conflicts that, in the interest of human welfare now and in the future, should receive more serious and widespread attention than is presently evident" (p. x). This is a strong message, one that is being broadcast with ever greater frequency in the popular media as events and predictions including the nuclear accident at Chernobyl, the spread of AIDS, "nuclear winter," and the "greenhouse effect" increasingly direct our attention to the sometimes precarious but always vital relationship between humankind and the physical environment. Caldwell's concern is nothing less than the "politics of survival." He asserts that the various social responses to biological facts will determine whether we survive in an ecological and biological sense and what form that survival will take. As he points out, "A new level in an ascending hierarchy of knowledge has become necessary, at which the integration of the discrete sciences of man and of life can be accomplished" (pp. 39-40). Caldwell is neither a naive optimist nor an extreme pessimist. Indeed, all his essays reflect a political realism that leads him to assume a cautiously positive posture based largely on a confidence in the ability of humans to recognize the compelling challenges that newly acquired knowledge of nature presents and make principled choices that will promote survival rather than extinction. But hard, informed choices must be made: "If humans fail to find answers to the problem of their behavioral-ecological relationships and to be guided by this knowledge, their future may be too brief to find answers to most of the other perplexing questions about human behavior" (p. 43).

The concept of "biocracy," the origin of which he attributes to a 1933 paper by the biologist Walter Cannon, is the central concept in the book. *Biocracy* is actually defined in several ways by Caldwell. First, in chapter 2 he states that the term "was invented to signify a sociopolitical system in which biological

knowledge and technology strongly influence social behavior and thus ultimately affect policy" (p. 19). Later he suggests that Cannon's early formulations constituted an "incipient theory of natural government" (p. 57). Clearly, Caldwell feels that a "naturalistic" theory of politics is what is required. Politics, then, must be made consistent with biological fact and must be seen as an extension of biological theory. This is a challenging proposition for students of politics and is at the heart of those advocating a political science that is a "life science."

Chapter 3, "Health and Homeostasis As Societal Concepts" is a minor tour de force in which Caldwell succinctly reviews a considerable body of literature to make the case for the rigorous application of these concepts to societies and collectivities of societies. Chapters 5, "Life Sciences As Problems for Politics and Law" and 6, "Biocratic Interpretations of Ethical Behavior" provide an excellent introduction to the complex and important issues resulting from new life science knowledge, which dramatically confront our established systems of values (religions; legal systems, governments). A great strength of the volume is Caldwell's insistence on linking political theory and public policy to an ethical foundation, specifically a bioethical foundation. He does not really provide that foundation; rather he makes absolutely clear the need for such a foundation and goes on to sketch a bioethics, the ultimate goal of which is species survival. In this discussion in chapter 6 he addresses three aspects of survival: fitness, homeostasis, and criteria of "right and wrong" in survival (p. 122-33).

For all of its many strengths, the volume does have a few weaknesses. In particular, there is still some confusion in the use of several concepts (biocracy, bioethics, biopolitics, biopolicy, biotechnology, fitness, health, homeostasis, survival), with those concepts never being linked together or cast as a framework of inquiry or explanation. That is to say, there is no formulation of theory provided—only a series of related concepts. Much more work needs to be done on the subject of bioethics and Caldwell has barely begun to incorporate into his formulation the considerable philosophical literature in bio- and biomedical ethics. For example, survival is posited on a collective or group (species) level without

confronting the problem of reconciling or making compatible the behavior of individuals and groups or coming to grips with the fundamental tension between competitive and cooperative behavior.

Despite his own awareness of the limitations of previous efforts that have been built upon a synthesis of knowledge gleaned from the life sciences to influence policy (e.g., the efforts of the Club of Rome or the authors of the *Blueprint for Survival* [1985]), Caldwell fails to offer realistic, incremental, concrete, tactical and strategic political recommendations that could move us closer to meaningful public policy. He speaks well to an academic audience, but not to a readership of politicians (though one could hope!). It remains to be seen how advocates and political activists might go about achieving the political goals manifest in a politics of ecology.

SAMUEL M. HINES, JR.

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Governing Prisons: A Comparative Study of Correctional Management. By John J. Dilulio, Jr. New York: Free Press, 1987. 349p. \$24.95.

John Dilulio's *Governing Prisons* is superb. It addresses an important question, Under what circumstances are good prisons possible? It examines prison systems in three states; California, Michigan, and Texas. It is grounded in observations at several prisons in each state and illuminated by interviews with scores of prison staff ("keepers"). The interviews are eloquent, the observations keen, the comparisons enlightening, the author's judgments balanced, and the range of scholarship expansive.

The book arrives at a precise, powerful, and important conclusion, "Prison management matters" (p. 256). It also explains why some incidental matters matter less; that is, Dilulio teaches why he downgraded the significance of factors that "sociologists have argued" as causing prisons to be horrible — violent characteristics of the inmates, low level of expenditures, crowding, high inmate-staff ratios, little for-

mal staff training, architecture, the inmate social system, race prejudice, and repressive custodial techniques. These factors did not explain the insecurity and inhumanity afflicting prison life. Rather, they were the conditions out of which good prison managers made good prisons. The secret of good prisons is order, and the secret of order is tireless, careful, rule-abiding, formalized, purposeful, and stout-hearted leadership.

In Dilulio's book the Texas prison system emerges the hero, in the superior and tragic senses of the word. *Superior* because for much of the past two decades Texas's was the safest, most humane, and most productive of the three systems. Texas was "where prison managers effect a strong administrative regime that enforces adherence to the norms and values of a civilized, noncriminal, 'straight' way of life — dressing neatly, washing regularly, being punctual, working hard, speaking respectfully to peers and authorities, delaying gratification for the sake of future rewards" and where "serious disorders are less frequent, meaningful treatment programs more plentiful, and recidivism rates less startling" (p. 7). *Tragic* because the Texas system collapsed into disorder, demoralization, and controversy in the 1980s when less meticulous, forceful, and single-minded administrators were appointed.

The book is outstanding for five reasons. First, it demonstrates that leadership is important and teaches what real leadership is. Dr. George Beto (the Texas director of corrections), E. J. Estelle (Beto's successor), and Wayne Estelle (warden of the best California prison and, incidentally, E. J. Estelle's brother) were meticulous in detail, had vision, set goals, and inspired their staffs to carry out the difficult work of keeping the inmates safe from individual and group misconduct.

Second, it establishes that the philosophies men carry about in their minds are important. Drawing on Aristotle, Burke, and John Stuart Mill, Dilulio shows that the keepers based their practices on distinct and consistent sets of general ideas, which in turn nurtured and honored their adherence to standards of practice. In short, it was ideas about prison conditions, not the material conditions themselves, that affected how staffs behaved. Ideas were the stuff of managing prisons, and what proved truly decisive in making a prison good or bad was the set of answers the corrections

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staff formulated to profound questions: "Should the inmate be viewed more as an incarcerated citizen or as a confined criminal? Is the rule-abiding prisoner entitled to all, some, or none of the privileges of free, law-abiding citizens? Should privileges be given to him gradually as a reward for good behavior, or all at once upon entering the prison? Do inmates have any right to rebel? Do keepers have a duty to sculpt the moral character of the inmates, or is it enough to prevent overt violence and to encourage rule-abiding behavior? Is criminality caused by such things as personality development and poor economic conditions? Or are most criminals rational actors who believe that crime pays? Or is there a spiritual dimension to criminality such that some criminals can be understood as wicked souls who delight in praying upon the life and property of others? Texas, Michigan, and California keepers," Dilulio continues, "each held a set of ideas on these topics that bound them together and distinguished them from their peers in the other two states" (p. 174).

Third, the book has a comparative and historical perspective. The comparisons revealed that some prisons were good, most horrible. Some succeeded in being orderly and productive, while others failed by being anarchic and brutish. The best were administered with discipline, the worst with such indifference that the convicts took over. Dilulio's powers of description make the reader feel the difference between the security of Huntsville in Texas and the chaos of Huron Valley in Michigan, between the hopefulness of California Men's Colony in San Luis Obispo and the despair of Soledad a mere 180 miles up the California highway.

Fourth, it celebrates knowledge, not the superficial notions of remote theoretics, but the profound understandings of the keepers derived from their practice. Dilulio teaches the reader how prison life differs from normal society in "the level of human trust and the likelihood of spontaneous, uninduced cooperation among people" (p. 239), and he itemizes the havoc wrought by a federal district judge who, in attempting to cure real ills, disregarded those differences and disparaged the practice of the best of the keepers who heeded them.

Finally, the book is important because it rejects fatalism and determinism in human af-

fairs. It records what real men really did to shape events. "I have suggested," writes Dilulio, "that good prisons are possible and denied that bad prisons are inevitable. I have attempted to identify the administrative conditions under which better prisons are possible and have raised serious questions about the importance of money, training, and numerous other factors that are claimed to have a major bearing on the quality of prison life. I have doubted most of what sociologists have argued about prisons and made recommendations that run counter to most contemporary ideas about how best to handle incarcerated felons. Furthermore, I have claimed that prison order is both good in itself and a necessary precondition for [humane treatment of prisoners]" (p. 255).

At the end of the book Dilulio points to Alexis de Tocqueville: "If [his and Beaumont's *On the Penitentiary System in the United States*] had been studied and its general lessons heeded, most, maybe all, of the last century and a half of America's penological follies might have been avoided" (p. 320). Tocqueville's spirit animates every sentence of *Governing Prisons*. That spirit is the reason why I believe Dilulio's book should be assigned in every introductory U.S. politics course, for it puts flesh upon Tocqueville's concluding paragraph of *Democracy in America*, which warned against the kind of pseudoscientific determinism that enfeebles men by excusing their faint-heartedness. "I am aware," wrote Tocqueville in 1840, "that many of my contemporaries maintain that nations are never their own masters here below, and that they necessarily obey some insurmountable and unintelligent power, arising from anterior events, from their race, or from the soil and climate of their country. Such principles are false and cowardly; such principles can never produce aught but feeble men and pusillanimous nations. Providence has not created mankind entirely independent or entirely free. It is true that around every man a fatal circle is traced beyond which he cannot pass; but within the wide verge of that circle he is powerful and free; as it is with man, so with communities. The nations of our time cannot prevent the conditions of men from becoming equal, but it depends upon themselves whether the principle of equality is to lead them to servitude or freedom, to knowledge or barbarism, to pros-

perity or wretchedness." *Governing Prisons* honors those words devoutly.

WILLIAM K. MUIR, JR.

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Telecommunications in Turmoil: Technology and Public Policy. By Gerald R. Faulhaber. Cambridge, MA: Ballenger, 1987. 186p. \$26.95.

Teledemocracy: Can Technology Protect Democracy? By F. Christopher Arterton. Newbury Park, CA: Sage, 1987. 224p. \$14.95.

As the "information age" or postindustrial era grows beyond infancy, inseparable interrelationships between communications and politics become all the more blatant. Consequently, a cadre of scholars has emerged, creating subfields in various disciplines known as "media and politics," "political communication," and so on. Some members of this corps of intellectuals are unabashed advocates of ideological or policy positions. Others approach the subject from what they claim is a strictly professional, detached, or even scientific point of view.

The two recent works under review are cases in point. Faulhaber's *Telecommunications in Turmoil* is a clever legal brief for an extreme viewpoint. Arterton's *Teledemocracy*, taken at face value, is a scholarly analysis of a number of innovative field experiments. However, both of these books are themselves vivid illustrations of the interface between communications and politics. Both are political tracts with definite agendas. The difference is that one's agenda is ostensibly clear, while the other's is opaque. At the heart of each is a politically conservative message.

Let's start with Faulhaber, currently a professor at the University of Pennsylvania's Wharton School. It is neither irrelevant nor immaterial to observe that he was previously a "director of strategic planning and financial management at American Telephone and Telegraph" and prior to that held the job of research head at Bell Labs. Thus it is not too surprising to learn that the "turmoil" in the telecommunications industry of which he speaks is due solely to government interference, that is, federal regulation.

Although not an avowed libertarian, Faul-

haber's argument will win the minds of Adam Smith devotees and the hearts of Ayn Rand purists. What he wants is a total and abject government withdrawal from anything to do with the communications industry: "What we need is to rid telecommunications of regulation—not 'regulatory reform,' not 'managed transition,' and surely not 'managed competition'" (p. 164). The only way this can be done is through an act of Congress and only through this complete legislative retreat to the nineteenth century can "everyone be a winner" (p. 165).

How can Faulhaber be so sure this will work? He uses no tables, charts, trend analyses, aggregate data, futuristic forecasting techniques, crystal ball, or divining rod. What he does have is the power of example. Unfortunately for him, the one he uses is the deregulation of the airline industry.

From his secure and lofty perch as an industry-efficient analyst, the author applauds today's "efficient hub-and-spoke" airline operations, with airline seats full of passengers and lower salaries for pilots. By following this exemplary model, he predicts that the U.S. telecommunications industry can provide us, one and all, with a functional equivalent of the "better service" (pp. 166–67) the airline companies have demonstrated since deregulation.

Perhaps Faulhaber travels exclusively by train. Perchance his air jaunts are confined to AT&T corporate jets. But as a commercial airlines tourist class passenger myself, I tremble to think how a future telecommunications industry's "better service" would equal the ever-narrowing seats, ever-longer delays, increasingly lost, misrouted or brutalized luggage, and the frequently missed connections that have accompanied—if not characterized—the deregulation of the U.S. airlines industry.

Thus what is billed as—and first appears to be—an examination of communications "policy" turns out to be an elegant, albeit transparent, polemic on one of the fundamental system-process issues of our time: state capitalism or statism versus unbridled laissez-faire. Communications policy is merely the focus.

F. Christopher Arterton's book, *Teledemocracy*, also deals with system-process change, although in style and substance it is mildly conservative instead of wildly reactionary. The stated purpose of his brief treatise is to investigate 13 "policy-neutral" U.S. teledemo-

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cratic projects in terms of how well they increased citizen participation (and "protected democracy"?). The author constructs a framework of 11 criteria to evaluate the degrees of success or failure of each project.

Arterton derives these standards from a summary and analysis of the continuing debate on the (in)effectiveness of contemporary U.S. democracy that he sketches artfully in his first chapters. These criteria include (1) "the range of citizens able to participate"; (2) "whether or not citizen participation [had] a direct influence on public policy"; and (3) "the burdens, financial and otherwise, imposed on citizens in connection with their participation" (p. 63). Then Arterton classifies and describes each project and subjects them to a close scrutiny against these 11 criteria. Each is found to be seriously deficient on a number of dimensions. Many are judged by Arterton to be "disappointing" or to fall "far short" of his goals.

This is not to say that the author had nothing good to say about any of the projects. Indeed, he finds at least as much good in them as bad and concludes that teledemocracy offers a promise of "slight" or "modest" improvements to the present system of U.S. democracy (p. 204).

So what's conservative about that? How can this staid and temperate empirical inquiry be considered in the same league as Faulhaber's overtly political tome? Just as reputedly scientific public opinion polls may be constructed to bias their results, so may apparently balanced academic investigations produce, ever so subtly, an expected or preordained result. This can be accomplished by the research design itself or through its execution. For example, suppose an academic researcher was funded to study experimental theatre in the United States. Instead of a research protocol that probed, systematically and in depth, the views and impact of these experiments on the writers, directors, actors, critics, and audience, suppose that the design (1) compared the number of people who saw Broadway shows and movies to those who attended experimental dramas and (2) speculated subjectively about the effect of these experiments on mainstream theatre. The inevitable conclusion: the consequence and promise of experimental drama are disappointing. This is precisely what Arterton has done.

Next Arterton imposed an onerous and strict

ex post facto set of evaluative criteria on the experimenters but, oddly enough, failed to be rigorous in his own system of evaluation. For example, in noting that one project had no effect on public officials, he based his conclusion on two interviews of nonelected officials (p. 91). In verifying the "reach" of another project, he conducted a random survey of only seventy people (p. 123).

Arterton may not have *consciously* intended to convey the impression that these projects did not accomplish much, but his book damns by faint praise teledemocracy's promise for the future of U.S. politics. Unfortunately, that is often the latent function of academic research in the United States: to criticize and analyze something to death, particularly if it involves theory or field research that may lead to a radical restructuring (democratization) of the political system. The result is part of an old story—an academic establishment, comfortable and cynical and fundamentally at odds with those who struggle in the "real" world to solve systemic problems that the very same academics concede to be serious.

And that is status quo—conservative—politics, no matter what the rhetoric.

TED BECKER

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Referendum Voting: Social Status and Policy Preferences. By Harlan Hahn and Sheldon Kamieniecki. Westport, CT: Greenwood, 1987. 150p. \$35.00.

This short book is a welcome addition to the limited literature on municipal referenda and focuses upon the impact of social class—as measured by housing values—on voter decisions. Thirty-eight nonfinancial referenda in 15 U.S. cities and 14 similar referenda in 5 Canadian cities between 1955 and 1970 were examined. In addition, 228 tax-and-expenditure referenda in 29 cities during the same time period were analyzed.

The principal focal point is an evaluation of the theory of "decreasing scarce resources," which posits that high-status voters may "support relatively expensive policy proposals consistent with the amenities that they wish to enjoy, while the obverse—or 'increasing scarce resources'—may provoke similar patterns

among lower-status voters, with implications for the increasingly constricted support of community programs" (p. 47).

Examined referenda included legalization of gambling, water supply fluoridation, blue laws, a municipal franchise, and a property requirement for voting. The authors reported that higher-status groups do not support the legalization of gambling but appear to have "greater tolerance for pornography" (p. 95) and favored sporting event regulation and daylight savings time. The findings relative to nonfinancial propositions are interpreted as a challenge to the "public-regardingness" theory, which earlier had been dismissed as an inadequate explanatory theory and support for the "decreasing scarce resources" theory.

Examination of referenda in 29 cities generally supported the findings of earlier studies that higher-status voters tend to approve financial proposals. Wealthier voters are assumed not to be concerned about making a choice between different spending proposals since the additional costs they will bear are minor relative to their resources. The authors maintained the type of tax was not a major concern to the wealthy voters.

Hahn and Kamieniecki also concluded that high-status voters "are not prepared to subsidize the disadvantaged" in referenda on health and welfare propositions and suggested the reason for this finding is that high-status voters have a different view of "fundamental economic values" and the manner "in which a person achieves social status in American economic life" (p. 124).

The structure of a city government—reformed or nonreformed—as measured by an index of reformism—is identified as a major variable with status affecting the vote to a lesser extent on expenditure and tax issues in reformed cities. The authors noted that the tax referenda were held chiefly in the decade of the 1960s, "when there may have been somewhat less concern over the issue of tax limitation than in the 1970s" (p. 123).

Readers are advised that care should be exercised in drawing conclusions about individual voting behavior from aggregate data and interpreting the data collected in the period 1955–70. Relative to the latter, they are confident that the voting propensities of the electorate on referenda issues, as reported in their study, are valid.

Questions can be raised as to whether the referenda results are valid only for the time period in which the referenda were held. Although the wealthy voters were found not to be concerned about the type of tax, congressional ending of the federal income tax deduction for state and local sales taxes, effective in 1987, may have affected the attitude of these voters toward the levying or increasing of the rate of a sales tax. The authors also failed to mention the influence on referenda results of the 1978 U.S. Supreme Court decision in *First National Bank v. Bellotti*, which struck down provisions in state corrupt practices acts prohibiting corporations from contributing to referenda campaigns if corporate interests were involved.

The findings relative to the influence of reformed structure on referenda results would have been strengthened if evidence had been provided relative to the presence or absence of an active citizen association supporting or opposing propositions.

The weakest chapter is the concluding one, which is little more than a rehashing of findings and a lamenting of the absence of redistributive propositions on the referendum ballots. Briefly discussed are "some possible reforms," including the use of surveys to determine which issues should be placed on the ballot to avoid overburdening the electorate, postal registration of voters, public financing of referenda campaigns, more effective enforcement of spending limits, and judicial evaluation of propositions prior to their placement on the ballot.

The study would have been of considerably more value had a comparative focus been employed with results from the 1955–70 period contrasted with the results of post-1978 referenda. In addition, the book would be better if it contained an expanded methodology section or a methodological appendix and listed all cities visited to obtain information.

The above critical comments do not detract from the value of this book, which is the first to examine in a systematic manner the relationship between the votes cast in referenda and social class. The authors succeeded in their avowed goal of resurrecting to prominence the social status theory of voting.

JOSEPH F. ZIMMERMAN

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A Slippery Slope: The Long Road to the Breakup of AT&T. By Fred W. Henck and Bernard Strassburg. Westport, CT: Greenwood, 1988. 288p. \$37.95.

The Fall of the Bell System. By Peter Temin with Louis Galambos. Cambridge: Cambridge University Press, 1987. 378p. \$27.95.

Two new books on the 1984 AT&T divestiture deserve special notice among those on the industry. While both ring with great authority, one was written and fostered by participants in the long trail leading to the breakup. The other was a "commissioned work."

A Slippery Slope: The Long Road to the Breakup of AT&T, is probably the better of the pair. It was written by Fred W. Henck, longtime editor and publisher of the now-54-year-old *Telecommunications Reports*, and Bernard Strassburg, who for 10 years was chief of the FCC's Common Carrier Bureau. While journalist and regulator provide joint accounts in most of the 288 pages, each treats some events and trends in notations denoting individual views. The other key work on the breakup is *The Fall of the Bell System*, by Peter Temin with editing by Louis Galambos. Temin is an MIT economics professor. Galambos, a history professor at John Hopkins, had earlier edited the papers of Dwight D. Eisenhower. The Temin-Galambos book originated in the desire of AT&T Chairman C. L. Brown "to commission a scholarly study of the breakup of the Bell System, a book that would serve as a point of reference for other studies." Brown got what he wanted.

Both works end with reflective sections in effect viewing the future with nostalgia. The authors recognize that the divestiture was an inevitable move to a new way of doing telecommunications business, one that is far less comfortable than the "Pax Americana," which existed for so many years in the industry. Why indeed did the breakup occur? Telephone service had been the world's envy, efficient, affordable, and ubiquitous. Before and after the Bell System had overcome its late-1960s run of service problems (due to poor demand forecasts), complaint levels were low. Both books credibly develop the theme that the new (some say contrived) competition in the industry was unable to coexist with regulation.

Henck and Strassburg correctly trace the breakup to its origins 50 years earlier—at

passage of the Communications Act in 1934. The next year, the new FCC launched the telephone investigation and gave the final report to Congress in 1939. A decade later, the Justice Department used that report as the basis for its antitrust suit asking breakup of the system.

Strassburg's "Outlook" section sees signs of returning to monopoly or perhaps cartelized oligopoly status in the long distance business because duplication via multiple suppliers is at odds with economies of scale and the general public interest. Given Strassburg's dominant place in the lineage of competition in the industry, such a publicly uttered projection is surprising. He clearly expresses his doubts that the consumer is benefiting from the competitive status of the industry.

Henck's epilog notes, "In telecommunications, those best equipped to look out for themselves have benefited from deregulation and competition" and observes that "wide open competition does not seem to be in the cards." Henck sees the critical question for the future as "whether the nation's telephone customers will benefit, not just whether the competitors can survive."

Besides the advent of competitive offerings of equipment and services, authors Henck and Strassburg identify and track five key sources of the breakup of the system. First was the FCC, "almost always the focal or departure point of the policy changes that have come about." Next was Congress, with its "amorphous role," which served "as a galvanizing agent in compromise policy changes administratively fashioned by the FCC and acceptable to diverse forces which could not agree on the precise terms of legislation."

Third were numerous federal judges "who did more to influence the direction of government policy than either the FCC or Congress," namely, by causing changes in FCC decisions and hearing the many lawsuits. Fourth on the list came "a militant group of anti-trust specialists in the Department of Justice who concluded that the time had come to restructure the communications industry more in keeping with a competitive environment." Finally, "the responses of the industry itself were critical to the outcome. The Bell System often chose not to yield any part of its monopoly domain to the new entrants or to adjust its policies to the advent of a competitive marketplace. Eventually, in the face of the forces arrayed against

it, this led to its dissolution." Intransigence, resistance to change and the relish for noisy confrontations on the part of some high AT&T officials are well documented by Henck and Strassburg.

The Temin book concludes by generally citing the same causes leading to the breakup of the system. First was the advent and growth of competition in the industry. Second was the pervasive change in ideology dictating both the government's decisions on competitive entry and AT&T's choices on its internal structure. Third was that implementing change was greatly complicated by the system's internal culture, the legacy of Theodore N. Vail. The book explores the system's "rigidity . . . which hurt AT&T in the political process."

Temin notes that while the public policy and corporate strategy implications of divestiture cannot yet be evaluated, some strong indications have come forth: "[Former chairman Charles L. Brown's] twin aims of protection of the stockholders' interest and the maintenance of service seem to have been largely accomplished." The person holding AT&T stock gained appreciably by keeping everything. Business customers found the range of services increased but residential users found the quality of their repair and installation services impaired. AT&T employees, as indicated by the number of layoffs and strikes, did not fare so well. Each of these points is well developed in Temin's work.

He ends the book by observing that the breakup must be viewed as "an enormous gamble," namely, in organizing a previously regulated and unique industry.

Because Temin had full access to AT&T documents and persons in and out of the company, including *A Slippery Slope's* coauthor, Bernard Strassburg, the account of the breakup reads as an insider's story. This book more than adequately footnotes the interviews and primary sources for treatment of the many events and cases leading to the breakup.

JOHN MORGAN

Communications Workers of America

If I Had a Hammer—The Death of the Old Left and the Birth of the New Left. By Maurice Isserman. New York: Basic Books, 1987. 259p. \$18.95.

Social scientists, historians, and political actors have long been asked why there is no socialism in the United States. Most of the literature that has been produced in pursuit of the answer to this question focuses on structural explanations having to do with the composition, dispositions, and organization of the U.S. working class, with the nature of the U.S. political system (widespread suffrage, federalism, localism, civil liberties, etc.), with the attractions of upward mobility in U.S. capitalism, or with the hegemony of a political culture rooted in the individualism of classical liberalism. Isserman, while aware of this literature, seeks to locate much of the failure of Left politics in the United States in a series of wrong strategic choices and other mistakes of commission and omission on the part of the principal organizations of the Left.

Isserman's major focus in this endlessly fascinating book is on the myriad ways in which the rise of the New Left in the sixties was rooted in the organizations, personalities, debates, and ideas of the Old Left. In the course of this scholarly journey, he demonstrates how the 1950s were not entirely a time of consensus, one described by C. Wright Mills as "the great American celebration," but was rather a period with strong currents of Left opposition that would flourish in later years. Isserman also convincingly demonstrates that the New Left was never entirely new, as often assumed by those most responsible for the intellectual and organizational construction of the New Left; for while much of the Old Left had disappeared by the late 1950s, remnants carried on through the darkest days of the cold war and contributed tangibly to the construction of a New Left. In passing on its legacy, however, Isserman argues that the old saddled the new with many of its traditional problems, while the new failed to learn properly from the bitter dead-end experiences of the old. The great strength of Isserman's book is in the rich detail of the telling of this complex story. In its pages we learn of the many and even heroic efforts of Communist party reformers, Shachtmanites, the intellectuals around *Dissent*, socialist campus organizations, and the peace

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movement during the fifties to define an alternative vision of the United States.

If detail is the strength of the book, the weakness is in Isserman's failure to move beyond detail and to tie his rich historical description to a structural analysis of the United States during the fifties and sixties. While he occasionally touches base with structural issues, he does not take the impressive structuralist literature on the state of the Left seriously enough or try to conduct a dialogue between this literature and the details of individual and organizational action that constitute most of his book. One is left with a story that, in my view, is much too "volunteerist." In the course of making the case that choices by Left organizations about strategy mattered, he leaves the impression that it was virtually all that mattered, that had Left organizations only made the correct strategic choices, a vital and important movement of the Left could have been fashioned. In so doing, while telling a fascinating tale, the work remains unconvincing in the end, for it fails to consider very seriously what was possible in a structural sense. I remain unpersuaded, that is, that the outcome of the story (namely, the decline of the Old Left and the rapid rise and fall of the New Left) would have been much different in light of better strategic choices. The failure of the Left in the United States, in my view, is better located in issues of class structure, cultural hegemony, and the nature of the capitalist state.

EDWARD S. GREENBERG

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Black Political Mobilization: Leadership, Power, and Mass Behavior. By Minion K. C. Morrison. Albany: SUNY Press, 1987. 303p. \$49.50 cloth, \$19.95 paper.

Whose Votes Count? Affirmative Action and Minority Voting Rights. By Abigail M. Thernstrom. Cambridge, MA: Harvard University Press, 1987. 316p. \$25.00.

These books are valuable contributions to the growing literature on black U.S. politics. *Black Political Mobilization* draws most of its data from three small towns in rural Missis-

issippi, but the author suggests that the recent political history of these communities is representative of communities throughout the South where blacks are well represented in the electorate. *Whose Votes Count?* is a serious attempt to assess the impact of the Voting Rights Act of 1965 and some of its key amendments.

The Morrison book begins with critiques of pluralist and elite theories of democracy. The author goes on to suggest a new eclectically derived mobilization model based on an appreciation for the methods that blacks were forced to use in order to make U.S. democracy work for them. Morrison argues that black mobilization in Mississippi after World War II was analogous to nationalist movements in the third world. This mobilization culminated in the 1970s, with a virtually complete transfer of institutional power to blacks in those communities where they had gained political consciousness and were guided in their political objectives by visionary leadership and a vigorous strategic organization.

The towns in this study were Tchula (population 1,739), Bolton (population 816), and Mayersville (population 500). Morrison couches his analysis first in the context of historical black political participation in the South and then hones in on the communities under study. The author does an excellent job of describing black political consciousness from the slave era to the present. He focuses on three small towns that were characterized by heightened political consciousness in the civil rights era among a numerically dominant black population, who in turn were led by youthful, educated, charismatic, interventionist local black leaders. The movements that resulted led to blacks gaining local political power for the first time.

Although this book is based on a very limited number of survey interviews, Morrison's contribution to the literature lies in his penetrating analysis of the context of present-day life and politics in rural Mississippi. He points out that merely gaining local political office is not enough for blacks. As a group, they continue to be poor, economically weak, and vulnerable. This gives rise to a dependency syndrome that will afflict black communities for some time to come. Morrison's study shows that black political mobilization has its limits and that blacks remain only on the fringe of national ruling coalitions.

Although he does not specifically say that the next stage of the black struggle is largely a class struggle, this implication is evident from the analysis. This book makes a timely contribution to the literature on black political participation.

In contrast to the Morrison book, *Whose Votes Count?* is grounded in an analysis of the Voting Rights Act and numerous court cases that have shaped the character of voting rights law over the past two decades. Thernstrom does a fine job of tracing the history of the Voting Rights Act and spelling out the various revisions of the law. Her main purpose, however, is not merely to document how blacks and Latinos have increased their voting power. She also wants to question whether the Congress and the courts have gone too far in guarding against race inequality in the electoral system. She asks, "How much special protection from white competition are black candidates entitled to?" "Are blacks and Hispanics entitled to safe minority seats reflecting their proportion of the population?" "Are minorities better off with fewer seats, but more diffuse influence?" Thernstrom contends that the true intent of the Civil Rights Act has become obscure to federal legislators and judges over the years. It was designed only to ensure equal access to the ballot, not to guarantee a certain proportion of minority representation in government. The author argues that the 1982 revision of the statute has given minority voters unprecedented power to insist on methods of voting that facilitate minority office holding. At-large voting arrangements are fast disappearing. Rather than seeing this as beneficial to minorities in the long run, Thernstrom maintains that this will only lead to further racial polarization. She advocates political integration and biracial coalitions instead of solid minority voting blocks. The author chides civil rights organizations for not seeming to believe that a race-neutral "community of citizens" is possible.

Black and Latino activists and scholars alike would find much to criticize in this book. The facts cannot be quarreled with, but the author's interpretations are open to question. The book's tone is one that suggests that there is no longer a need for the close monitoring of the enforcement of the Civil Rights Act. However, evidence to the contrary can be found in the very pages of this book! Yet the book has

value in that it contributes to the affirmative action debate.

EDMOND J. KELLER

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Race and Ethnicity in Chicago Politics: A Re-examination of Pluralist Theory. By Dianne M. Pinderhughes. Urbana: University of Illinois Press, 1987. 318p. \$27.95.

Chicago once again provides the municipal setting for an examination in this book of the politics of race, ethnicity, and the political integration of previously unrepresented groups. The study incorporates the rich body of prior scholarship on Chicago politics and contributes important original research to that scholarly tradition. Pinderhughes' central argument is straight forward: race and ethnicity are not of equal status in U.S. city politics and the differences that define race from ethnicity retard the ease, speed, and degree of political inclusion of racial groups as compared to ethnic groups. Barriers to racial inclusion are identified throughout to account for the subordinate status of Chicago's blacks. The case of black exclusion is then developed critically to assess arguments about the penetrability and openness of the political system as advanced in pluralist theory.

Between the introductory and concluding chapters, the core of the book focuses on the time period 1910-40, comparing the black racial experience to the Italian and Polish ethnic experiences. Pinderhughes selected the two immigrant ethnic groups for study because their arrival in Chicago closely corresponded to black migration to Chicago. In chapters 2 and 3 the author shows how business and labor markets facilitated ethnic but not racial advancement, how housing markets concentrated blacks residentially more than the ethnic groups, and how the evolving Democratic political machine differentially impinged on the three communities. A model of political participation is introduced in chapter 4 to predict group success in winning citywide political leadership positions. When the model is applied, blacks are found politically dominant in areas where they were concentrated, but—unlike the ethnic immigrant groups—their repre-

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sentation stopped there. Chapter 5 develops an argument for a distinctive black political ideology that derived from the experience of racial discrimination and produced identification among blacks with collective racial status issues. Chapters 6 and 7 conclude the core of the book by examining the criminal justice and educational policy areas to assess the varying policy responsiveness of the political system to the three communities. Pinderhughes shows black underrepresentation and argues that municipal policies were unresponsive to blacks while higher inclusion rates were found for Italians and Poles, who also received more favorable policy treatment.

Throughout the central section the author provides several important insights about ethnic and racial community politics. The discussion of community characteristics distinctively influencing neighborhood organizations, leadership structures, and community institutions is richly textured. The evolution from competitive party structures in the 1920s, where blacks had political voice, to a monopoly structure in the form of the Democratic machine, where black influence flagged, is amply documented and effectively tied into differential policy treatment blacks received. The model of political participation persuasively suggests the constraints imposed on internal community organizations by external environments. A powerful argument about the conditions associated with high black electoral participation, historically and contemporarily, is presented when Pinderhughes suggests that since race is the single most important factor affecting blacks, in those elections where race is an issue or black candidates are on the ballot black turnout is unusually high, often exceeding that of whites.

Events taking place before 1910 and after 1940 are treated less thoroughly. A brief introductory chapter discusses the demise of the Irish-dominated machine in the 1970s and the subsequent black succession to power in Harold Washington's successful 1983 mayoral bid. The final chapter offers an explanation for the black electoral triumph of the 1980s, which is less convincing, perhaps due to how close the author was to the events themselves. Thus incompetent leadership is a recurring explanation for the demise of the machine, and the analysis of Mayor Washington's organizational base is less well developed than the earlier,

historical, organizational analysis. Perhaps more important, black succession to power has important implications for the continuing debate over pluralist theory that could be more fully developed. These reservations invite continuing scholarly research on processes of racial exclusion and inclusion, to which Pinderhughes has made an important contribution.

There is much in this book to recommend it to a variety of scholarly audiences. Students of urban studies will find in it a useful complement to the already vast literature on Chicago politics. Those interested in race and ethnicity will find the comparisons and contrasts between European immigrant groups and blacks of interest. The book is sure to occupy an important place in contemporary black studies. The discussion of criminal justice and education policy responsiveness will interest students of public policy. Pluralist theorists will find in Pinderhughes' book yet another critique about how pluralism fails to account for the case of racial minorities.

DAVID J. OLSON

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A Republic, If You Can Keep It: The Foundation of the American Presidency, 1700-1800. By Michael P. Riccards. New York: Greenwood, 1987. 227p. \$35.00.

First Use of Nuclear Weapons: Under the Constitution, Who Decides? Edited by Peter Raven-Hansen. New York: Greenwood, 1987. 249p. \$37.95.

The Secret Constitution and the Need for Constitutional Change. By Arthur S. Miller. New York: Greenwood, 1987. 179p. \$29.95.

As we head into the third century of constitutional government, these three books help light the way. One looks back to the creation of the presidency; another examines the most portentous problem of our own time, the control of nuclear weapons; and the third assesses the "survivability" of the constitutional system in the face of future challenges.

Michael Riccards takes the title of his book from Benjamin Franklin's remark as he departed from the convention of 1787. Riccards' book is laid out in two parts. The first

50 pages long, presents the roots of the U.S. tradition about executive power: the ideas of Locke and Blackstone, the experience with provincial governors and with U.S. leadership during the Revolutionary War, and the framing and ratification struggles. Part 2—about 150 pages long—surveys the history of George Washington's two terms as president.

Those who turn to this book for clarification about what Franklin and his colleagues may have meant by republicanism and whether we have "kept" it will be disappointed. Part 1 is superficial and textbookish and seems oblivious of the best recent scholarship on the colonial and early national periods.

Part 2 is old-fashioned narrative history. It is full of good yarns. But it must be read warily, for it is also full of errors. For example, there is a reference to "differential politics" (for *deferential*, p. 62), and Washington's unsuccessful Supreme Court nominee is cited as "John Randolph of Virginia" instead of John Rutledge of South Carolina (p. 113).

On the other hand, the book has some real strengths. I found the chapter on Washington's policy toward the Indians fascinating and full of relevance to modern concerns. Right from the start, it seems, the executive branch fought with Congress over the defense budget and the right to expose ill-fated policies to public scrutiny. One gains a deeper respect for the first president as, with the surprisingly able assistance of his secretary of war, Henry Knox, he groped to frame a policy amidst uncertain constitutional provisions and a maelstrom of racist passions.

The book contains at least one noteworthy interpretive idea: the suggestion, offered in the introduction (p. xiii), that Washington created a "model of the presidency" that fell into disrepute during the nineteenth century and has been "ironically rediscovered in the latter part of this century." Its main features, writes Riccards, are "mass appeal," strong executive authority, "general disregard of political parties," attention to pomp and protocol as means to buttress the incumbent, conservatism on economic and social matters, and "aloofness toward legislative controversies." Riccards contrasts this model with Jefferson's "party government [and] cabinet leadership" and with Jackson's populism, which "delighted in confrontations with Congress and with various elites." With the recent breakdown of the party

system and the longing for presidential stewardship based on a sense of personal communication with the people, Washington's "model," suggests Riccards, is again ascendant. Sadly, he never mentions this idea again after the introduction, though it seems to me worth pursuing.

The second volume edited by Peter Raven-Hansen makes a solid contribution to the debate about the viability of constitutional guarantees under modern conditions. It presents papers given in November 1985 at a symposium sponsored by the Federation of American Scientists and the Lawyers Alliance for Nuclear Arms Control addressing a proposal that Congress enact a ban on the first use of nuclear weapons by the United States, except in a declared war, to be waived only with the assent of a congressional leadership group.

The volume contains 11 papers: four that discuss the allocation of war powers under the Constitution, three on the form that congressional participation might constitutionally take, and four on problems of implementation. (Can Congress be trusted with sensitive national security data? Could the courts force presidents to obey this law?) Most of the authors are professors of law. A few have served on congressional staffs.

The problem confronted in this volume is a terrible one. The Constitution draws a reasonably clear line between the legislative and executive responsibilities in war making. Congress raises troops and determines the need to go to war; the commander-in-chief conducts operations.

Modernity has changed that. With standing armies and a worldwide network of entangling alliances, presidents have the means and authority to wage war on their own. And with launching mechanisms—our own and our adversaries'—on perpetual alert, our national security demands that the president—who alone can act instantaneously—have the authority, when necessary, to do so.

This book addresses two dimensions of the modern dilemma. The first is the nuclear threshold. Ought presidents to have the authority to initiate the use of nuclear weapons? Is that a command decision, once Congress has provided them and we are lawfully involved in hostilities? Or is it a policy question of sufficient gravity that Congress may and should by law forbid presidents to use

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these weapons without its explicit consent?

Assuming that the case can be made that congressional participation ought to be required, the second part of the dilemma has to do with the proper mode. It is impractical for 535 men and women, in two chambers, to take counsel with the president in the heat of hostilities about whether a particular emergency justifies the use of our nuclear arsenal. After the *Chadha* decision, can Congress lawfully empower a committee to act on its behalf? Will presidents accede to this arrangement? If not, can anyone force them to do so?

The essays in this book are carefully argued and thoroughly documented, and they represent a wide range of scholarly opinion. Particularly valuable were the judicious and skeptical analysis of the Federation of American Scientists proposals by John Norton Moore; Michael Glennon's review of the contention that treaties like NATO commit U.S. presidents to defend Copenhagen as they are committed to defend Staten Island; and William G. Miller's well-informed defense of Congress against the charge that it is not to be trusted on matters of national security. The editing by Raven-Hansen is also commendable, as are his succinct introduction and conclusion.

The third book, by Arthur S. Miller, presents a vigorous call for constitutional change. As Miller himself notes several times, it is meant to be suggestive rather than exhaustive.

Miller contends that there are in reality two constitutions in the United States: the formal, written one, and a "secret," unwritten one, an operational code of informal understandings and conventions. The former he disparages. It is essentially a myth, whose principal function is to mask what happens in accordance with the latter. The unwritten constitution legitimates groups, virtually ignores individual persons, and grants special privileges to super-corporations.

This system of governance has worked for two centuries owing primarily to the continent's abundance. But time is running out. The tide is rising, and "the natives are restless." We are on the brink of a "societal nervous breakdown." We must move soon toward a "sustainable society," one that grants all people what they need and deserve, in accordance with the limits on population and resources that nature prescribes.

To do this, we must change the Constitu-

tion—not so much its structural features as its definition of governmental powers. We must make it clear that Congress has power to make and enforce measures to achieve an "optimum population," to "defuse the threat of nuclear war," and to control environmental degradation. In addition, Congress must have power to "check the excessive use of presidential and bureaucratic power," and the Supreme Court must have original jurisdiction to determine the validity of allegations that Congress has failed to perform any of these new duties. For this purpose, "any citizen eligible to vote . . . shall have a right to bring an action in the Supreme Court, the writ of which, if granted, shall run against Congress generally and the members specifically."

Structural changes are given short shrift, though not ignored completely. Miller suggests that Congress be reduced to a single chamber with one hundred members, that the presidency be "bifurcated," that political parties be given control over members of Congress, that the 50 states be "telescoped" into regional governments, and that the "judicial role . . . be expanded."

Miller's ideas are obviously "utopian," as he freely admits. The question is, What value do they have? His diagnosis of the challenges we face is very shrill. Drawing heavily on the work of Lester Brown, Paul Ehrlich, and other like-minded folk, he paints an apocalyptic picture of the human future. The trouble is that his remedies seem almost bizarre, and he disdains to explain or defend them.

We should resist the easy impulse to dismiss proposals for constitutional change. The bicentennial provided very little opportunity for a broad public discussion of the fitness of our constitutional system for the challenges of its third century. However, the Iran-Contra hearings, the battle over the nomination of Robert Bork, and the collapse of the stock market vividly revealed strains in our system and raised the level of public discourse on these vital matters. It is a discussion that I hope political scientists will want to join.

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The Development of American Federalism. By William H. Riker. Boston: Kluwer Academic, 1987. 233p. \$38.95.

William H. Riker has reissued here a body of work on federalism written over a 30-year period. Eight of the 11 essays in the book were published before 1970. One essay is a previously unpublished 1985 paper on the invention of centralized federalism by the framers of the U.S. Constitution. Two essays represent previously unpublished work by two of Riker's former graduate students. Hence, most of the essays are familiar to veteran students of federalism.

The book, therefore, seems aimed more at young scholars, those trained mainly in the tradition of federalism as intergovernmental relations. In this respect, the book is a valuable reiteration of important issues in federal theory. Riker, a pioneer in the study of federalism, is hardly concerned with intergovernmental relations. Instead, he approaches federalism as a mode of political organization lying, in his view, between alliance and empire. Riker therefore addresses the structural politics of federalism: how federal systems come into being, how they operate as a complex of central and peripheral political institutions, and how they are maintained, especially with respect to centralization and decentralization.

Intellectually, though, Riker is largely preoccupied with political issues of U.S. federalism that dominated the liberal reform agenda of his generation. Methodologically, he is preoccupied with the tools of behavioral inquiry that were being developed during the 1950s and 1960s. These two preoccupations are joined in the book's central concern: the maintenance of a steady-state process of centralized federalism.

In a personal introduction to the book, however, Riker reverses his 1964 view that U.S. federalism is little more than a device for perpetuating racism. This reversal, he says, is associated with civil rights reforms and with his own ideological migration "from New Dealer in the fifties to liberal, anti-statist in the eighties." As a former New Dealer in the Reagan era, Riker now concludes that federalism is "a desirable, though still minor, restraint on the leviathan."

Nevertheless, an overriding concern for Riker is the legitimation of centralization. He

has no wish to "strengthen the central government so much that it can cannibalize the constituents," but maintenance of a strong, centralized federal system is essential. Riker seeks to legitimize centralization by arguing that U.S. federalism was centralized from the start. Compared to the Articles of Confederation, the U.S. Constitution established a highly centralized, though not unitary, polity. In certain respects, this point seems obvious. However, it becomes problematic when Riker tries to show a fundamental continuity of centralized federalism for nearly two hundred years. In short, the New Deal was not so new.

In the new essay published here, Riker examines key federalism debates in the Constitutional Convention of 1787. Although nationalists, such as James Madison, got less than they initially wanted (e.g., no congressional veto of state legislation), they did, in Riker's view, push federalism largely into the margins of the Constitution. One of the federal devices that made it to the center of the Constitution — state legislative selection of senators — failed to fulfill its purpose, argues Riker, and was excised in 1913.

This is a curious essay, however. It was originally presented at Indiana University for Vincent Ostrom's inauguration as Bentley professor of political science. Riker does not mention Ostrom's arguments set forth in *The Political Theory of a Compound Republic*, which was revised and reissued in 1987. In addition, no attention is given to new scholarship on the founding period. Nor does Riker attend to the ratification debates and, thereby, to what the people may have understood themselves to be ratifying in the state conventions. No attention is given to the antifederalist insistence on a bill of rights to further protect both persons and the states against tyranny and centralization.

Riker understands federalism to be a constitutional bargain, but he looks at the nationalist view of the bargain and at only one piece of the bargain, namely, the first, unamended offer put on the table. With respect to the first offer, moreover, he looks mainly at its structural and procedural provisions. Scant attention is given to the substantive provisions of the Constitution that created a "general government" (Madison's term) possessing limited, delegated powers, not a "central government" (Riker's term) possessing virtually plenary powers.

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By finding centralization in the birth of U.S. federalism in 1787, Riker can conclude that "a more or less steady state has continued since then"—although there has been "intense centralization of effort and expenditures" as well as "disharmony." Two essays, for example, make use of a crude historical index of political disharmony in the federal system. Amazingly, the index shows decreases in disharmony during the Civil War era (1861–73), the early New Deal years (1933–37), and the era of massive southern resistance to the national assault on radical segregation (1959–65).

Primarily, according to Riker, what prevents the federal system from becoming completely centralized and thereby unitary is the inability of national officials to control the nation's partisan political process. In particular, both the separation of powers within the national government and the decentralized structure of the political parties act as breaks on centralization. This does not mean that states can control national decisions, but it does mean "that the nation cannot control state decisions. The result is a standoff, which is . . . intended in the federal bargain." Riker seems to anticipate the views expressed by the majority of the United States Supreme Court in *Garcia v. San Antonio Metropolitan Transit Authority* (1985) and *South Carolina v. Baker* (1988); that is, state prerogatives are protected well enough by the political process, and there is no need to activate the Tenth Amendment as a break on centralization.

There is much that is rich and rewarding in this book, and some of the theses for which Riker is well known, such as the military and expansionist origins of federalism, deserve careful scrutiny. Yet the issue that dominates the book and the methods used to substantiate the steady-state thesis give this book the character of a historical footnote, albeit an important footnote that helps to show how we got where we are today in both federalism and political science. In light of the nationalizing and internationalizing forces at work in U.S. society, however, plus abuses of power by the no-longer-so-liberal national government, one wonders how much solace an antistatist can find in a federalism whose constitutional status has been reduced to that of a contest of wills.

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Hispanics in American Politics: The Search for Political Power. Maurilio E. Vigil. New York: University Press of America, 1987. 147p. \$23.50 cloth, \$11.75 paper.

Maurilio Vigil's latest book is one of a handful of scholarly works on Latino politics in the United States and one of still fewer that focuses on Latinos in the electoral arena. He calls his "a first work on Hispanics in American politics," and to the extent to which he characterizes individual Latino electoral success stories and calls for Hispanic political homogeneity, it probably is. However, this is not the first work on Hispanic-Latino politics per se. For instance, there is Garcia's *La Causa Politica* [1974], Garcia and Garza's *Chicano Political Experience* [1976], Rodolfo Acuna's *Occupied America* [1981], and Joan Moore and Harry Pachon's *Hispanics in the United States* [1985].

The stated premise of the work is that "Hispanic groups in this country have reached the point where they must accept being categorized as a single ethnic group" (p. 1). One could quibble with some of the terms Vigil uses. He effectively makes a case for the need for different Latino groups (Mexican Americans, Puerto Rican Americans, Cuban Americans, etc.), to "homogenize" under a common name in order to engender a political "oneness" such as has been prevalent among blacks but has not taken root among Latinos. However, the term of choice for Vigil—"Hispanic"—is one that inspires criticism and division in Latino communities, particularly among community activists who prefer the self-imposed term *Latino*. Many activists, primarily on the political Left, argue that *Hispanic* is akin to *Negro*, in being a term largely applied from the dominant culture and not one of the group's own choosing. In the same vein, Vigil frequently uses the terminology of the dominant culture—*illegal aliens*—when referring to immigrant Latino workers living in the United States without legal documentation. Latino activists, especially those working with such populations, prefer the less offensive term, *undocumented worker*. Finally, an avoidance of class and ideological themes is replete throughout this work. Dr. Vigil's focus on the necessity of creating broader in-group patterns among Latinos is well taken. However, more attention should be devoted to discussing class and ideological differences and cleavages that work against unity.

Vigil's first chapter begins effectively by recognizing erroneous implications of the dominant pluralist explanations of "democratic homogeneity." The dominant (pluralist) view is that we have a class-blind and a color-blind electorate in the United States. He effectively argues that on the contrary, U.S. history reflects racial-ethnic voting blocs, although there continues to be reluctance to recognize this since "it runs counter to certain propositions about the homogeneity of American culture and life." He then proceeds to inform us that "the first task that faces the social scientist who seeks to study one particular ethnic group . . . in American politics is to place that group in the context of traditional and modern conceptions of democratic political theory and the dynamics of American culture and intergroup relations" (p. 12).

However, the author falls into the self-imposed trap of theoretical myopia inasmuch as he strictly limits these conceptions to the two dominant theories of pluralism and elite theory before briefly considering alternative theories (internal colonialism, assimilation, melting pot, and cultural pluralism). His major omission here is that of Marxist class theory. He does not offer a critique from a Left political perspective, nor does he offer a discussion of class perspectives. However, the merit is that he explores the contradictions within pluralist theoretical homogeneity, a theme more extensively explored by Bayes in 1982.

Vigil's second chapter seeks to place the political juncture of today's Latinos in a mileau of historical understanding. As such it traces immigration patterns, levels of educational and occupational attainments, and the levels of socioeconomic accommodation of each. The third chapter, on the Hispanic vote, concentrates on the Latino vote in presidential elections, especially those in 1980 and 1984. It is valuable in that few scholars have attempted to assess the impact of Hispanic presidential votes and voting patterns, especially when compared with blacks. As informed as the chapter is, much of it seems veiled in excessive optimism—from his belief in the plausibility of biracial coalitions to his undue concentration on Hispanic electoral victories and minimizing of defeats. Nevertheless, there are high points like his discussions of political socialization variables and factors contributing to Hispanic political participation.

The real meat of the book, Chapters 4 and 5 (covering almost half the book), deals with Hispanic "success" stories in selected state and local elections in selected states. Herein there is both a positive and a negative. The positive is that it is extremely helpful to have listings and historical sketches of Hispanics who have achieved elective and appointive public office. The downside is that the focus is too narrow, too individualized, and too descriptive. There is little-to-no theorizing, modeling, or connection to voting rights legislation attempted. There is little to connect these individualized success stories with previous chapters. Vigil's sixth chapter is a useful if incomplete one on Hispanic organizations. His classification of organizational types makes little room for community-based and Left political-advocacy organizations.

Vigil sees a "new Hispanic politics" manifested by a "new style Hispanic politician," a new interest in registering Hispanics, and new Hispanic organizations that "have emerged to advance the interests of Hispanics through traditional lobbying strategies" (p. 134). While this is accurate, further discussion and elaboration among political scientists is needed.

JAMES A. REGALADO

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The Truly Disadvantaged. The Inner City, the Underclass, and Public Policy. By William Julius Wilson. Chicago: University of Chicago Press, 1987. 254p. \$19.95.

Wilson argues that the problems of the worst-off inner-city blacks result from complex demographic and economic changes, not contemporary racial discrimination or the erosion of the work ethic. Together, the social transformation of the city and the reorganization of the economy have created this "underclass." The continuous flow of black migrants to urban centers has produced a large pool of relatively young, poor, and uneducated minority labor. At the same time deindustrialization, the growing proportion of knowledge-based jobs, and the movement of industry from urban to suburban locations have sharply reduced the number of entry-level, blue-collar positions that would allow these people to build and sustain intact families and work

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their way out of poverty. The exodus of the black working and middle classes from ghetto neighborhoods compounds the problem by undermining the social, religious, and neighborhood institutions that might integrate the most disadvantaged into stable, community networks. The results, Wilson suggests, are unprecedented "social pathologies" in the center cities, including skyrocketing rates of joblessness, teenage pregnancies, out-of-wedlock births, single-mother-headed families, welfare dependency, and street crime (p. 6).

A social democratic critic of the U.S. welfare state, Wilson rejects both liberal and conservative analyses of the Great Society and standard explanations of the problem of poverty in the United States. Building on arguments he advanced in *The Declining Significance of Race*, the author criticizes liberals for unduly emphasizing the role of racial discrimination in explaining black poverty. Current racism does not play a central part in the creation and reproduction of the underclass, he maintains. On the contrary, since the 1960s federal programs have eliminated many of the most egregious barriers to formal racial equality. Historical discrimination has helped, of course, to create a racial division of labor that now plays a powerful role in reproducing black inner-city poverty. But, Wilson insists, "broader issues of economic organization" that "have little or nothing to do with race" more powerfully explain the fate of the underclass (p. 12). Yet liberal reformers have failed to confront the class issue. Instead of committing the federal government to job creation, they recommend policies that target social benefits on a narrowly defined racial constituency.

Neoconservatives are dismissed altogether. Charles Murray and Lawrence Mead, the author suggests, continue to blame the victim by carelessly misapplying Oscar Lewis's culture-of-poverty argument without having fully understood it or checked the facts. More fundamentally, they appear oblivious to the analytic pitfalls inherent in using explanations of social behavior that give causal primacy to individual values over the situations and social structures in which people find themselves. This cultural view is taken seriously not

because it makes sense but because liberals have failed to offer a coherent alternative.

Wilson's prescriptions flow directly from his diagnosis. Referencing Western European social democratic welfare states, he argues for an interventionist economic program that would keep labor markets tight and United States industry competitive by promoting economic growth and industrial restructuring. Means-tested and race-specific policies should be deemphasized in favor of universal entitlement programs that would simultaneously aid the most disadvantaged inner-city residents while benefiting the white majority. Only these can win the broad political support that is necessary if inner-city poverty is to be addressed successfully.

On balance, this is an important and impressive work. It correctly recognizes that race and class have combined in complex ways to create the underclass, and it makes a significant contribution to unraveling how. The comparative perspective, though not fully developed, is also welcome, as is the effort to situate the growing body of statistical work on urban poverty in an historical and sociopolitical context. There are problems, to be sure. The definition of the underclass—"the most disadvantaged segments of the black urban community" (p. 8), including the long-term unemployed, the unemployable, the poor, the welfare-dependent, and street criminals—is analytically diffuse, combining economic categories with forms of deviant behavior. The normative subtext is troubling: Wilson seems too willing to accept conventional notions of family structure and social pathology. It is not at all clear, for example, what black, single mothers in the ghetto have done wrong other than be poor. Finally, while the effort to focus attention on class is welcome, the claim that racial discrimination is no longer significant is probably an overstatement. Nonetheless, Wilson's mastery of the literature, careful scholarship, and imaginative proposals make this a compelling account of the crisis of the inner city and a powerful example of what deft hands can do with policy analysis.

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COMPARATIVE
POLITICS

Outside Moscow: Power, Politics, and Budgetary Policy in the Soviet Republics. By Donna Bahry. New York: Columbia University Press, 1987. 236p. \$30.00.

Ever since Merle Fainsod's *Smolensk under Soviet Rule* (1958) provided an early glimpse of life away from the center, there has been an increasing fascination with local (republic-level or below) politics or policy-making in the Soviet Union. The death of Stalin and demise of the totalitarian model other than as an "ideal type" have opened the door to numerous studies of the local scene, including Philip D. Stewart's *Political Power in the Soviet Union* (1968), Jerry Hough's *Soviet Prefects* (1969), William Taubman's *Governing Soviet Cities* (1973) and Ronald Hill's, *Soviet Political Elites* (1977).

However, less attention has been focused on the relationships between the center and the periphery. Donna Bahry attempts to fill the gap through an analysis of "politics and budgetary policies at two levels of government—union republic and local" (p. 11). Her focus is on both qualitative material (speeches of republic and local officials at CPSU party congresses and Supreme Soviet budget sessions) and quantitative data (budgetary and investment figures for Soviet republics, especially since the 1950s).

Bahry's initial chapter compares center-periphery relations in the USSR with those of several Western nations. While this seems like a fruitful topic, she returns to it only briefly at the end of the book. However, she has opened the door for what could be a promising area of comparative analysis. Succeeding chapters discuss the general development of Soviet "fiscal-federalism," consider the republics' role in the budgetary and planning process and the nature of the agenda put forth by republic officials, analyze the impact of several variables on policy outcomes, and examine problems of local leaders—those below the republic level. The concluding chapter assesses the implications of her findings "for the study of policy-

making and of political centralization in the USSR" (p. 12).

Bahry notes the difficulties associated with gathering and interpreting Soviet data and her appendixes provide useful commentary on these matters. Nevertheless, her analysis of these data is persuasive and she presents a thoughtful, if somewhat surprising, picture of trends in Soviet budgetary policy-making and relations between the center and those outside Moscow.

Since Stalin's death local leaders have gained in political stature (as measured by representation in higher bodies) and now have more programs and funds to administer. However, this decentralization requires greater coordination of efforts and a larger voice in decision making, factors that the center has been unable or unwilling to provide. As a result, republic officials need to lobby for greater resources and autonomy and find themselves as "players in an elaborate bargaining process between center and periphery" (p. 63). Their major efforts involve trying to influence planning decisions through various channels of access: Gosplan, individual ministries, the Council of Ministers, the Central Committee, Supreme Soviet sessions, and meetings of the Party Congress.

Bahry's analysis of republic leaders' speeches provides a picture of their lobbying agenda. The primary concerns raised by these officials relate to agriculture and related areas and to the development of energy, metals, and other natural resources. Problems relating to economic and social infrastructure and construction equipment and materials also appear frequently (p. 81). This is to be expected, since these areas provide republics with their major sources of revenue or are among those for which they share administrative responsibility with central ministries—without the power to control events. The nature of these concerns has remained steady over time and does not seem to be affected by political variables. Bahry controls for such factors as representation in the Politburo, whether a republic leader

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is native or from another republic, and whether the leader has been newly appointed and thus is someone who might feel free to be more assertive. None of these variables affects the nature of demands raised by republic officials.

The success of these appeals is examined through an analysis of Soviet appropriations in the areas of capital investment, enlightenment, and health care. Two patterns emerge. First, investment allocations are generally stable, showing only incremental changes over time. Second, the same political variables noted above have little if any impact on the nature of allocations. This pattern raises serious questions about the importance of political connections in the budgetary process. Republics with direct access to Moscow (e.g., with Politburo representation) do not fare better than others. Instead, Bahry suggests that Soviet national leaders may be pursuing a policy that emphasizes equality in republic allocations in an effort to increase their overall level of support throughout the country. The result may be a series of compromises that aim to keep everyone happy but may also reflect the fact that "there is simply no single grand strategy for regional growth" (p. 121).

Yet surely membership in the Politburo or access to others in Moscow does matter. Decisions regarding foreign policy, domestic priorities, current economic or political reforms, cadres' selection, and so on are still made at the center. But the scope of Bahry's analysis is limited primarily to budget-related data and does not address these issues at any length. After moving through her careful analysis, one longs for a livelier narrative. Some readers may yearn for a discussion of party infighting—Gorbachev and Ligachev arguing over the disposition of Yeltsin or the future of Shcherbitskil.

But that is precisely the point. Much of Soviet politics and economic decision making lacks the high drama of Kremlin intrigue. It is rather a more mundane process of trying to allocate a limited amount of funds (confounded by a significant lack of data and complex problems of coordination) to what must seem like an infinitude of reasonable demands made on the center by those on the outside.

Bahry's worthwhile effort raises serious doubts about whether such a planning process can ever work in other than an incremental

and uncoordinated fashion. Even *perestroika* may not be able to bring it into line.

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Ethnic Conflict: International Perspectives.

Edited by Jerry Boucher, Dan Landis, and Karen Arnold Clark. Beverly Hills: Sage, 1987. 332p. \$29.95 cloth, \$5.50 paper.

A variety of areas in political science could benefit from a serious examination of the impact of ethnic identification on patterns of conflict and cooperation. A volume subtitled *International Perspectives* with a table of contents offering case studies ranging from Sinhala-Tamil conflict in Sri Lanka to mechanisms for managing and diffusing antagonism among the variety of groups in Hawaii would seem to offer much. The scholar with an interest in comparative politics might hope to find an approach to ethnic conflict that identifies features common to several geographic and cultural regions. The student of international politics might approach the book looking for clues about the conditions under which internal ethnic problems lead to external intervention.

Those expectations are likely to be disappointed. The papers were originally delivered at a conference sponsored by the University of Hawaii's East-West Center in March 1985. The editors report that the conference participants sought to derive a list of themes that might generalize across all the conflicts discussed and might be used, at least descriptively, as organizing principles for the revisions leading to the present volume. Those themes, enumerated in an opening chapter by Dan Landis and Jerry Boucher, include perceived differences between groups, stereotypes, and ethnic identity; land tenure and homeland issues and immigrant-versus-native status; involvement of outsiders; disparate allocation of power and resources; effects of language and language policy; process of conflict resolution; and religion. For the most part the papers do not refer to the common themes, let alone use or share concepts or theoretical approaches. The final chapter, "Some Thoughts on Ethnic Conflict" by Winthrop D. Jordan, was intended as a summary. One can only sympathize with his

proviso that the papers do not lend themselves to a coherent overview.

The central concept, *ethnicity*, is nowhere rigorously defined or clearly contrasted with other forms of politically relevant identity. For example, only a very diffuse and fuzzy definition of *ethnicity* can justify inclusion of the Malay Peninsula case (Ronald Provencher's "Interethnic Conflict in the Malay Peninsula," focusing on three groups—Chinese, Malay, and Thai—who spring from different ancestry, have distinctive cultural and linguistic patterns, and different historical relationships) and Rolf Kuschel's study of vengeance and feuding among the approximately five hundred inhabitants of one of the Solomon islands, whose patrilineal clans share descent from ancestors who arrived together as migrants, feature extensive intermarriage, and have a common language and culture.

Some of the cases discussed in the papers ("Sinhala-Tamil Relations in Modern Sri Lanka" by Sinnapah Arasaratnam, "The Basque Conflict" by J. Martin Ramirez and Bobbie Sullivan, Ronald Bailey's "Black-White Relations in Mississippi," Joseph E. Trimble's "American Indians and Interethnic Conflict," or Eric S. Casino's study of the Moros in "Interethnic Conflict in the Philippine Archipelago") have been marked by varying degrees of violence; others have not. But neither the chapters themselves, the editor's introduction, nor a foreword by Ross Stagner offer any systematic guidance in identifying the variables that might explain the presence of violence or its intensity.

Provencher's study of the Malay Peninsula, "Ethnic Conflict Management in Yunnan, China" by Zhang Shifu and David Y. H. Wu, M. Jocelyn Armstrong's "Interethnic Conflict in New Zealand," and John Kirkpatrick's analysis of Hawaii focus on situations that might well be regarded as examples of successful and basically cooperative relationships. Armstrong is quick to point out that the official myth of "one people" including the original Maori inhabitants and the Pakeha (white) immigrants has never been completely accurate and describes the rise of a Maori consciousness that has led to some low-key demonstrations and official attempts to institute reforms to reinforce biculturalism. She leaves the probability of success an open question, as does Kirkpatrick in summarizing the

ability of Hawaiians to maintain consensual controls on potential stresses. But since the four studies are descriptive and rely heavily on idiosyncratic features of the local scene to account for outcomes, the relevance of the cases to other multiethnic settings is not clear.

It is difficult to see many ways in which a political scientist might put this work to use. A reader with little or no prior knowledge may, as I did, find several of the pieces interesting introductions to situations that do not receive a great deal of publicity but clearly matter to those involved. Area specialists are unlikely to find anything particularly new or innovative in what are, of necessity, quick overviews. Although some of the conflicts have had an important international dimension there is little analysis of the dynamics or impact of escalation or the role of external actors. This book is also unlikely to serve as either a text or set of readings for a course dealing with ethnicity and politics, given its lack of coherence and framework and scattershot geographic coverage.

SETH THOMPSON

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**Sex Differences in Political Participation:
Processes of Change in Fourteen Nations.**

By Carol A. Christy. New York: Praeger, 1987. 195p. \$37.95.

In 1978 Sidney Verba and his colleagues reported a persistent pattern of sex differences in political activity in the seven nations included in their survey of political participation (Verba, Nie, and Kim, *Participation and Political Equality*, [1978]). In this volume, Carol Christy proposes to examine, first, if these differences have diminished over time, and second, whether the generation, diffusion, or development models adequately explain those instances where reduction has occurred.

Her first problem is assembling reliable data. As she notes, trend data are needed for testing the three models of change. However, trend data were available only for the United States and West Germany. For the most part, therefore, her analysis is based on cross-national comparisons. She examines data from 27 surveys conducted between 1958 and 1980 in 14 countries. Christy recognizes the shortcomings

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of this procedure, noting that "while nontrend data permit the examination of more diverse types of nations and political participation, they also require making certain nonverifiable assumptions about the processes of change over time" (p. 14). She infers changes over time from data that measure differences across space. On the basis of her analysis, Christy concludes that while all three models are useful for explaining variations in sex differences, short-term forces operating outside these models also play an important role: "No single factor explains trends in sex differences in political participation" (p. 102). She notes, for example, the influence of feminist activity in West Germany and the impact of Reagan's stand on women's rights in the United States on women's political participation.

The question of sex differences in participation, or what in some contexts might be called the gender gap, is one that has become more important in the political behavior literature during the past 15 years. Much of the growing body of literature in this field stresses the importance of traditional socialization patterns that have channeled men into public or political activities and women into private or household activities. Increasingly, researchers have come to recognize as well that structural or situational obstacles such as party gatekeepers or the demands of a double day help to sustain sex differences in participation. Unfortunately, this study does not permit examination of the impact of these factors, since it only measures political participation as a function of socioeconomic indicators. Like many comparative analyses of political behavior, this investigation of sex differences suffers from both too much and too little data. The task of sorting through 27 surveys spanning over 20 years is a difficult one, even when a limited number of independent variables is used. The result, in this case, is a complex analysis that yields relatively little useful information. In the end Christy resorts to "human choice and action" over "abstract, mechanical forces" in her search for explanatory variables (p. 115). Her quick trip through 14 countries yields little new or substantive information about either women's or men's political participation in these countries.

Chapter 4 stands out as an exception. In this chapter on differences over time, Christy is forced to restrict her analysis to the two coun-

tries for which she has trend data, West Germany and the United States. Here we get a good test of both the development and the generational models. The author concludes that both models are only weakly supported. But the generational model has been applied successfully in studies of women's political participation in France and in Canada. If this study is to improve our understanding of cross-national differences, the apparent conflict between Christy's findings and those of other researchers needs to be addressed.

This failure to situate the study in the context of our understanding of women's political participation reflects a major weakness of the book. The comparative literature in this area has grown dramatically since the early 1970s, when the first attempts to include women in analyses of political behavior were made. One of the most positive contributions made by feminist scholars in political science has been the emphasis on qualitative research as a necessary complement to quantitative research, and on the sometimes different meanings assigned to politics and to political activity by women and men. In this study there is an absence of qualitative data. In addition, there has been little attempt to assess the impact of political culture, changes in the legal status of women, or the ideology of the party in power on women's ability and willingness to take part in the political process. Nor has the author demonstrated her sensitivity to the different forms of participation favored by women (in particular, group activity and personalized contacting).

As a result, Christy is able to demonstrate conclusively that a gender gap in some forms of political participation and related attitudes has narrowed over time but still exists in a wide range of societies. She is not able to explain why this sex difference persists. In the end this book is more valuable as a critique of existing data and data collection techniques than as a comprehensive analysis of sex differences in participation.

SANDRA BURT

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Public Interest Law. Edited by Jeremy Cooper and Rajeev Dhavan. Oxford: Basil Blackwell, 1986. 482p. \$75.00.

This volume is particularly welcome because it provides a comprehensive and challenging view of the hitherto neglected activities of British public interest lawyers – thus setting the scene for comparative research on problems that have for too long been dominated by the U.S. perspectives. While there are some books and articles on various aspects of British public interest law, there is nothing else that approaches the scope and coherence of this collection. The eclectic group of (mostly British) academics and activists has managed to provide both a lively debate and an effective primer for newcomers to the topic. The general quality is high and the essays hang together surprisingly well.

The scope of the enterprise is clearly conveyed by Cooper and Dhavan's definitional point of departure: "Public interest law consists in the use of litigation and public advocacy (i.e., lobbying by representation or publication) to advance the cause of minority or disadvantaged groups, and individuals, or the public interest" (p. 5). The central question posed by the editors is what role public interest law and lawyers can play in alleviating "the deepening concern about increasing social and economic differentials between various classes, economic recession and political tension" (p. 3). The contributors are in sharp and stimulating disagreement about how this question is to be answered.

But before getting into that debate, consider the basics. There are, in essence, three components to British public interest law: private practitioners funded primarily by legal aid; salaried lawyers working in community-based law centers; and social action groups organized to protect women, consumers, the disabled, immigrants, the mentally ill, civil liberties, and so on. While each of these components is given some attention, the major focus is on the social action groups, which, as Dhavan sees it, have been the most enterprising and savvy: "Social action groups in England have demonstrated, in abundant measure, their capacity to be involved in sophisticated legal campaigns in a multiplicity of legal, social and political arenas" (p. 37). This inventiveness seems particularly important in the United Kingdom

where, as Carol Harlow points out, the courts, "though increasingly politicized . . . have not emerged as protectors of the poor. Nor have they generally provided a platform for the expression of minority views" (p. 104). Moreover, because there is no written constitution or bill of rights, occasional judicial victories can frequently be neutralized by the government's parliamentary majority. In sum, as a primer this volume offers both an introductory overview of the many facets of British public interest law and an analysis of the constraints imposed on public interest lawyering by the restrictive orthodoxy of the British legal culture.

But the broader message has to do with the relevance of public interest law to transformative and reformatory politics in contemporary Britain. On this matter, the authors are sharply divided, with essays by Dhavan and Richard Abel framing the debate. Dhavan's two essays (one written with Martin Partington) challenge public interest lawyers to adopt and pursue a broad agenda of welfare, justice, and civil and political rights aimed at empowering the disadvantaged and at reversing the top-down flow of welfare state politics. Abel forcefully rejects this strategy: "We must not assume that because law is an essential shield against the abuses of state power it can be wielded as a sword to alter fundamental class relations" (p. 383). While Abel and Dhavan would agree that legal victories are hard to secure and of limited immediate benefit to disadvantaged interests, they differ on the long-term potential of a politics of rights.

To Abel, the politics of rights is an illusion – dependent on state-financed legal aid made available in a spirit of noblesse oblige: "Legal aid is an attempt by those who enjoy state power, ownership of capital, and patriarchal domination to convince themselves that those privileges are not abused through arbitrary action, exploitation, violence, or irresponsibility, and that any abuses are promptly redressed" (p. 389). Dhavan, in contrast, believes that public interest law has the potential for redefining the terms of political discourse and for contributing to political mobilization. For Dhavan, the problem is not the inherent limitations of a politics of rights but the failure of social action groups to seize the opportunities provided by public interest law.

With Abel and Dhavan framing the debate

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and the other essays providing an introduction to the mechanics of judicial and administrative activism as well as some case study "raw material," this volume enables readers to begin to draw their own, albeit preliminary, judgments about the political relevance of British public interest law. The book should, therefore, be useful to students of British and comparative politics as well as to specialists in public law.

STUART A. SCHEINGOLD

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Apartheid's Rebels: Inside South Africa's Hidden War. By Stephen M. Davis. New Haven: Yale University Press, 1987. 272p. \$27.50 cloth, \$8.95 paper.

The Politics of Economic Power in Southern Africa. By Ronald T. Libby. Princeton: Princeton University Press, 1987. 361p. \$40.00 cloth, \$12.50 paper.

These two books on South Africa present remarkable contrasts. Both tackle important subjects. Davis's *Apartheid Rebels* is a fluent presentation of the aims, organizations, and capabilities of the contemporary African National Congress (ANC). Libby's *Politics of Economic Power* examines the regional economy of southern Africa from the periphery as well as the center and shows how political considerations influence economic decision making. The first book is journalistic and only superficially analytical. The second embeds case studies in theory; proposes new contributions to the study of the state, of dependency, and of political economy; and takes itself very seriously. Yet the first is the more important of the two.

Davis cannot claim to have understood precisely how the ANC operates either militarily or politically from its bases in Angola or its political headquarters in Zambia. Nor can he do more than describe infiltration routes into South Africa and enumerate some of the tactical considerations of a "humanitarian" guerilla struggle prosecuted by vastly outnumbered and outgunned black cadres. There are others who have a fuller appreciation of the ANC's nuanced ideological stance and its overall strategic program. Nevertheless, Davis adds an immediacy and an informed observer's

gloss to existing appreciations of the ANC as an exiled entity capable of harassing Africa's most powerful state.

Libby accepts the realities of that power. South Africa dominates the southern African region (Libby carries the region north to Zaire and Tanzania but perversely excludes Angola). Nevertheless, he persists in casting the framework of his book within the shadow of South African hegemony while simultaneously insisting upon the analytical importance of what truly is a very simple and hardly arguable point: that the policies of the hegemon are also influenced by the economies of the separate states of the region. He posits interdependence in place of dependence, but no local observer could argue otherwise. It is the scale that is important and the ease with which South Africa uses its economic leverage (and overwhelming leverage) to destabilize even the least ravaged of the local economies.

Libby describes the interplay of economic activity throughout the region. Its rail routes, harbors, imports and exports, patterns of labor migration, and flows of deferred wages and remittances are set out well, though the data are no longer current. Indeed, Libby's approach depends as much as Davis's on being timely. Using dated, and thus suspect, material on trade, Libby contrives arguments already falsified by changing circumstances and altered policies. He also seems unaware of the more thorough and more supple discussions of many of the same problems by Gavin Maasdorp and Henry Bienen.

Libby's case studies are meant to exemplify his central theoretical reconsiderations. But they are dependent upon empirical research or at least empirical observations and betray confusion more than complete understandings rooted in nuanced knowledge. For example, the South African corporate community and multinationals with bases in the region would be startled to read that the ruling National party had *aligned* itself with them in order to cope with external and internal threats to the state. Libby tells us that the state sought to use transnational entities to divide Africans and to increase its leverage against hostile neighboring nations. He further asserts that the state's policy of constellationism had been intended to expand South Africa's market for manufactured goods and to mitigate the hostility of neighbors like Zimbabwe.

A few interviews (Davis's main form of research) with capitalists or officials might have modified Libby's assumptions about the alignment between the corporate world and the state in South Africa. Industry and foreign-owned multinationals have indeed been used for repressive and cooptive purposes by South Africa, but hardly is it or was it an alignment, and the manipulated employment of nonstate enterprise beyond South Africa's borders has been rare and hardly successful. Today, too, the rupture between corporate endeavor and the state is almost complete. Although enterprises do comply with officially imposed requirements and therefore assist state-sponsored repression and control, they also oppose the state, and the largest of them have systematically rejected anything that could be called an alignment.

This is but one disagreement. Libby makes his case most often with structural arguments rather than examples. He misconstrues the relations between industry and labor, neglects the historical dimension of regional manufacturing and the location of petroleum refining, takes housing issues out of context, and implies that the National party actually sought reform (linked with big business) before its well-intended policies foundered on the rock of Conservative party opposition.

The other case studies of the smaller economies of the region are presented with varying degrees of success. The description of Zimbabwe's situation is limited to a now-overtaken conflict between the political pulls of populism and the incentive-based technocratic imperative. Libby focuses on the manufacturing sector when the driving engine of Zimbabwe has been and will continue to be agricultural performance.

Among the best summaries of the interplay of politics and economy is the exposure (admittedly based on the research of others) of President H. Kamuzu Banda's authoritarian personal design in Malawi. His Press Holdings Ltd. dominates the country for personal as well as political gain. The discussion of Mozambique is also good, if only of historical interest now that the attacks of Renamo, a South African proxy force, drought, and mismanagement have forced the poor state to its knees. The main issue, unaddressed by Libby, is why South Africa refuses to take advantage of its new patron-client relationship with

Mozambique and persists in destabilization instead of development.

Libby misses the main economic question about Zambia—how to restore incentives to a very heavily urbanized polity, wholly dependent upon a single commodity, where leadership is ignorant. His chapter on Namibia likewise focuses on future land issues when the someday independent state's prominent feature will be its continued structural reliance on an overweening neighboring economy as the real value of its copper, uranium, and diamonds diminishes.

Davis's book can be faulted for its failure to probe skillfully below the surface of rhetoric or received information. Libby's overarching structure is flimsy and simple, and his case studies are either misleading or summary. Even so, his conclusion does embody one significant, if banal, policy truth: that the smaller economies of southern Africa are both dependent upon South Africa and at the same time capable of exerting a tug of their own on that powerful polity.

ROBERT I. ROTBERG

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Intellectuals in Liberal Democracies: Political Influence and Social Involvement. Edited by Alain G. Gagnon. New York: Praeger, 1987. 242p. \$39.95.

Intellectuals love to generalize. Yet very few generalizations about the current relationship between intellectuals and political systems command agreement. That may be because intellectuals also love to disagree and to coin ever-differing interpretations of their own role in politics. But controversy over changes in that role may also be strongly influenced by locale. It is possible, for example, that in some societies intellectuals may continue to be marginalized while in others they are increasingly admitted or recruited into the circles of power. I speak not only of Jewish intellectuals and Marxist intellectuals but also, with distinctive connotations, of English, French, and third-world intellectuals. It is a virtue of this book that its strongest chapters blend questions of perennial concern to the sociologists of intellectuals—whether intellectuals form a

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separate class, whether social marginality account for criticism or alienation, and so on—with a nearly equal concern with the impact of the differing national political contexts in which intellectuals are rooted.

In my view the principal contribution of the collection comes in seven case studies of the role of the intellectual in the "liberal democracies" announced in its subtitle: Canada, the United States, England, France, West Germany, Italy, and Israel. Supplementing them is a rather miscellaneous section devoted to "perspectives for comparative analysis," which contains familiar total systems comparisons by S. N. Eisenstadt. A provocative wrap-up chapter by Robert J. Brym takes a hard look at the main sociological interpretations of intellectuals in political life, dismissing social rootlessness, malintegration into society, and "new class" expertise as explanations for newer roles of intellectuals in favor of a Gramscian interpretation based on social origins and the structure of career opportunities.

The study of intellectuals is an area of inquiry especially lacking in agreed parameters or methodologies, and this collection, for all its numerous good parts, demonstrates rather than resolves that condition. The contributors seem to agree only that the status of intellectuals in liberal democracies is in flux and needs explaining. They differ in varying degree on the nature of such change and on general theory that might explain it. That does not mean that insightful middle-range propositions, derived from individual national experiences, are absent. For example, in a persuasive analysis of why Left intellectuals seem to have disappeared in France today, George Ross dwells on the effects of "the changing professional market" for Left ideas in a society increasingly dominated by mass rather than elite culture. Carl Boggs' examination of Italy concludes that while "free-floating" intellectuals may have disappeared, their "modernizing" cousins flourish inside the semicorporatized world of party bureaucracies, public corporations, and interest groups. This decline of the marginal (usually Left) intellectual is echoed in William Rubinstein's lively depiction of the transformed situation of Jewish intellectuals in a number of national settings. On the other hand, Paul Hollander's rather polemical essay on the United States finds the marginalized social critic alive, well, and destructive in the

United States, a position that can be read either as a confirmation of U.S. exceptionalism or simply as reflecting a blinkered perspective.

Fifteen years ago Charles Kadushin (quoted p. 208) wrote on the study of intellectuals that "theory building in this field has been marred by an abundance of opinion and moralization, a dearth of facts, and a plethora of parochial definitions." By itself the Gagnon volume does not overturn that judgment; but, especially in its presentation of comparative perspectives, it softens it a bit and thus is welcome.

JAMES B. CHRISTOPH

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The Military in African Politics. Edited by John W. Harbeson. New York: Praeger, 1987. 208p. \$35.95.

The task that Harbeson sets for himself and his colleagues is to challenge the dominant assumptions of much of the existing literature on the African military and to establish the foundations for a second generation of studies of military rule in Africa. Harbeson asserts that military intervention in civilian politics in Africa is not a "temporary aberration" in the political development of the continent but must be treated as "part and parcel" of that process (p. 2).

Rather than focusing upon how and why the military intervenes, two sets of questions form the central theme of this volume: (1) How do military officers behave in power? How do they mobilize resources in order to legitimize their assumption of power, and what policies do they pursue in order to address problems such as economic stagnation and ethnic strife, which led them to intervene in the first place? and (2) How do military rulers use their power to change the written and unwritten rules that regulate both public authority and political activity in Africa? What role do military rules play in the process of transferring political power from one leader to a successor?

While these questions are interesting, Harbeson relates them to the growing debate on the nature of the state in Africa. If the military is in fact equal rather than subordinate to civil authorities, then, Harbeson argues, theories of the state in Africa must transcend both Marxist and liberal democratic theories,

neither of which emphasizes this equality (p. 5). While this theoretical dimension is critical to the study of the African military, it is not fully developed in this collection.

Analyzing the role of the Sudanese military, Nelson Kasfir examines how former President Nimeiri's strategy of managing support for his regime by constantly courting new allies and turning his back on old ones, eventually cost him the support of his one essential backer, the armed forces. In contrast, Dov Ronen argues that the current military regime in Benin has been able to expand its support significantly. Ronen argues that much of this success can be attributed to the ability of the military to re-define salient social cleavages away from patterns of ethnic identity and—with the introduction of Marxist-Leninist rhetoric—towards ideological affiliation. Military management of ethnic cleavage also forms a central theme in Victor LeVine's essay on the Congo.

Two essays focus on attempts by military rulers to engage in political and economic transformation. Henry Bienen argues that West African "populist" military regimes in Liberia, Ghana, and Upper Volta have sought to reestablish ties between the governing elite and the broader population by creating new political institutions. While bypassing traditional structures and violating established rules of political behavior, Bienen concludes that attempts by the military to establish new popular power bases for their regimes have been much more successful than similar attempts at economic transformation. Such a conclusion stands in sharp contrast to that of John Harbeson, who argues that under the current military regime in Ethiopia, the political relationship between the centralized state and the peasantry bears a striking similarity to that which existed under the former imperial regime. The Ethiopian Derg has, however, been far more successful in engaging in economic transformation, particularly in the areas of literacy and land redistribution.

I. W. Zartman focuses his analysis of Algeria upon two contrasting patterns of military behavior. During periods of political stability, policy-making is characterized by patron-client relationships that enable various groups within the military and civilian sectors to form alliances. During periods of political succession, however, the military displays a much more cohesive pattern of behavior.

Similarly, in his study of Nigeria, Richard Joseph examines how the military has not only sought to restructure the polity but has also had a "profound impact on the rules that determine the legitimate exercise of public power" (p. 69).

The primary strength of this volume is the way in which it reconceptualizes the role of the military in Africa as being equal to that of the civilian authorities. With the exception of Algeria all of the countries examined in this volume have experienced a dramatic and overt assumption of political power by the military. An alternative road to parity might be much more incremental. In cases where the survival of the dominant social structures becomes increasingly dependent upon the use of force, the military might well see its role increase beyond parity, without the need to displace formal civilian political leaders. The obvious case in point is that of South Africa, where maintenance of the white regime has resulted in the civilian restructuring of formal political institutions in such a way that power has flowed rapidly into the hands of military leaders and their close associates.

The greatest weakness of this volume is that it excludes from its list of case studies southern Africa, the very region in which military-civilian collaboration has its deepest historical roots. The imposition and maintenance of settler colonial regimes throughout this region necessitated close cooperation between civil and military authorities throughout the entire colonial era. Similarly, one of the major political legacies of the struggle for independence throughout the region has been the need to unify the military and political structures of the various liberation movements. While the research task of this volume is solid, its findings could have been enriched significantly by examining civil-military relations both in settler state structures and African liberation movements, as well as in postindependence regimes.

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Thatcherism and British Politics: The End of Consensus? By Dennis Kavanagh. New York: Oxford University Press, 1987. 334p. \$44.00 cloth, \$11.95 paper.

When Prime Minister Margaret Thatcher led her Conservative party to its third straight general election victory in June 1987, she paved the way to surpass all other twentieth-century British prime ministers in length of continuous service in that office. On 3 January 1988 she achieved that record. Under her leadership many changes in British politics, economy, and society in general have resulted from the impact of what has come to be labeled "Thatcherism."

The title of Dennis Kavanagh's new book, *Thatcherism and British Politics*, points up something new and important as a force in British political life. No previous prime minister's name has been turned into an *ism*. Kavanagh undertakes a thoughtful and thorough analysis of what this development means to contemporary Britain by examining the "consensus politics" he sees preceding it, the forces that destroyed that consensus, and the prospects for the future.

In postwar Britain a political consensus developed, based on broad agreement between the major political parties' front benches on a mixed economy and welfare state. The author traces the challenges that undermined consensus and led to its breakdown in the 1970s, assessing the role of the major actors and groups involved. He then evaluates the development of a British "New Right" agenda in the 1980s. The New Right policies provide the doctrinal substance that, combined with the prime minister's unique personal style, constitutes Thatcherism.

Numerous books and articles on British politics and Thatcher leadership are currently appearing. Margaret Thatcher has become something of a "hot topic" among political analysts as she moves into seniority both domestically and on the international scene. The British political system is also more in vogue as a focus of study than it has been recently because of the changes in what many considered its static and relatively unproductive consensus politics. Most current studies have been either leader-personality-oriented or analyses of the policy-institution-process interplay. A particular merit of Kavanagh's book is

its more ambitious attempt to combine analyses of events, policies, processes, and personalities. In this he is largely successful, particularly with his treatment of Thatcher's style of leadership.

One of the most valuable chapters in this book is "Margaret Thatcher: A Mobilizing Prime Minister." Here the author applies the distinction between mobilizing and reconciling leadership styles to Thatcher, concluding that she is a "mobilizer in a rather immobilist society" (p. 278). As such, he finds her to be an unusual Conservative: activist and confrontational, forceful, radical, and practical in approach. In a system in which the prime minister is supposedly constrained by forces that hinder a U.S. president less or not at all, Thatcher captured her own party by surprise and has been able to exert a great measure of personal influence. The author evaluates the prime minister as party leader, cabinet manager, parliamentary force, and public figure. In contrast to other Conservative leaders, who traditionally work for compromise and accommodate to major interests (a style especially suited to consensus politics), he finds Thatcher's assertiveness and her determination to head a "conviction government" (p. 253) have resulted in both strong leadership and polarization. There is little middle ground. She is loved or hated.

The most unfortunate feature of this otherwise valuable study is the timing of its publication. Appearing in May 1987, it just missed the 11 June general election. This does not reduce its worth as an analysis of new directions in British politics through 1985-86, but it does leave the reader with some frustration since the campaign and election intervene between the writing and reading of the book. It leads to such then-reasonable observations as "By 1986 the tide of opinion seemed to have turned against the government. There seems little doubt, however, that she has left her mark on Britain in the 1980s and shaped the politics of the post-Thatcher era" (p. 317). It is certainly true, but in the wake of the 1987 general election outcome, this post-Thatcher era is some time distant. In light of Kavanagh's previous British election studies, the reader will regret the absence of coverage of this major election from his book, especially some further evaluation of the possible "new consensus."

Kavanagh provides a useful, informative,

and readable—if already somewhat dated—book that forecasts a new consensus for Britain, a possible middle ground between the old Labor positions and those of the New Right. This is likely to include acceptance of much of the welfare policies from the past and new Thatcher economics, Kavanagh speculates. Whether or not this combination can actually produce consensus remains to be seen, but it is made more problematic by the North-South polarization of Great Britain as revealed in the 1987 election outcome.

BARBARA B. KNIGHT

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The French Socialists in Power, 1981–1986.

Edited by Patrick McCarthy. Westport, CT: Greenwood, 1987. 212p. \$39.95.

The Socialist government that ruled France between 1981 and 1986 was unprecedented: it marked the first time in the Fifth Republic that the Left was elected to government between opposing political coalitions and the first time in the twentieth century that the French Left remained in office for much over a year. Although the Socialist party (PS) lost the 1986 parliamentary elections, it remains France's largest political party and its future could not be more rosy. A key development since 1982 is that the PS flourished while thoroughly renouncing the leftist orientation that it espoused throughout the previous decade. Moreover, it is ironic that during the PS's tenure there was a marked decline in the degree of class confrontation and statism, which have traditionally characterized French political culture.

The French Socialists in Power, 1981–1986 is a fine description of the fascinating period by leading U.S. and British scholars. It compares favorably with other recent treatments, including volumes edited by John Ambler, Philip Cerny and Martin Schain and George Ross, Stanley Hoffmann, and Sylvia Malzacher. Although much will be familiar to specialists, several essays make original contributions. Michael M. Harrison illuminates the complexities of French foreign and security policy, focusing on the ambivalent relationship between Mitterrand's France and Reagan's United States. On the key issue of East-West relations, Mitterrand vociferously supported siting U.S.

missiles in West Europe—although of course not on French soil—but denounced Reagan's attempt to prevent European nations from cooperating with the Soviet Union in constructing the gas pipeline. Harrison praises the government for having "managed the difficult feat of reconciling a form of restrained Atlanticism with the Gaullist heritage of national independence and European aspiration" (p. 59).

Michalina Vaughan usefully clarifies the Socialist government's complex, bewildering, and controversial educational reform projects, one of which brought down the first prime minister. Vivien A. Schmidt rightly emphasizes the importance of the decentralization reform. However, she exaggerates the impact of the Socialist-sponsored restriction on multiple officeholding (*cumul des mandats*): few elected officials exceeded the liberal limits established by the reform.

George Ross provides a characteristically stimulating account of the troubled relationship between trade unions and the Socialist government. Given the weakness, fragmentation, and ideological divergences within organized labor, it was impossible for the government to satisfy unions. Moreover, he asserts, in an era of economic crisis and union decline the Socialist reform of industrial relations (the Aurox laws)—which (in his view) sought to strengthen unions—actually benefited business. However, did the government in fact accord such high priority to strengthening unions? The Aurox laws had a multiplicity of goals, and certain provisions short-circuited unions.

Martin A. Schain contributes a superb analysis of the National Front's rise to respectability. He cogently suggests that the conversion of extensive antiimmigrant racist sentiment to support for the National Front was facilitated by the fact that leaders of every major party (including the Communist and Socialist parties) adopted antiimmigrant positions.

There are also useful contributions by Volkmar Lauber on the Socialists' Achilles heel, economic policy, Patrick McCarthy on the Socialist party's evolution in office; and D. S. Bell and Byron Criddle on the decline of the French Communist party (the PCF), which they rightly term "the most important development in French politics in the 1980s" (p. 156).

Several divergences occur within the volume—not a shortcoming, although the con-

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tending positions might have been developed more fully. A notable instance is differing assessments of the relationship between the Communist-leaning trade union (the CGT) and the government. Bell and Criddle assert that from the outset, the PCF used the CGT to express its distaste for the Socialist government. Ross sees the CGT (more accurately in my view) as "perhaps the *major* interest group' backer of the left between 1981 and 1984 (when the Communists left the government)" (p. 112). More significant than the CGT's intermittent criticism of government policies is that the union generally discouraged workers from mobilizing. (Indeed, the CGT's stance proved costly in its competition with other unions.)

A small disagreement and a large disappointment: Patrick McCarthy argues that the PCF "leadership offered Mitterrand its unconditional support" (p. 9) at the runoff ballot of the 1981 election. However, there is extensive evidence (some of which, admittedly, appeared after the book was published) that party leaders played a double game by publicly supporting Mitterrand yet privately seeking his defeat.

The volume illuminates important aspects of French policy and politics. Yet those seeking to understand the fundamental transformation in France's political, ideological, and economic landscape in the 1980s, as well as the Socialist government's responsibility for these developments, will be disappointed. In a nation long considered the epitome of class confrontation and statism, class identities and conflict have declined, new forms of cross-class compromise have developed, and support has increased for nonstatist methods of regulating conflicts. The Socialist government sponsored some of these changes, resisted others, and (by its failures) provoked others yet. *The French Socialists in Power* is a fine study of the trees. The forest, however, remains obscure.

MARK KESSELMAN

Columbia University

Politics and Sexual Equality: The Comparative Position of Women in Western Democracies. By Pippa Norris. Boulder: Rienner, 1987. 165p. \$25.00.

The Women's Movements of the United States and Western Europe: Consciousness, Political Opportunity, and Public Policy. Edited by Mary Fainsod Katzenstein and Carol McClarg Mueller. Philadelphia: Temple University Press, 1987. 321p. \$34.95.

Women's Rights in France. By Dorothy McBride Stetson. Westport, CT: Greenwood, 1987. \$32.95.

In the decade since the first broadsides against the neglect of women's politics were fired, much has been done to redress the balance. The late 1970s witnessed the first blossoming of gender studies of U.S. politics, and in the 1980s comparative work has flourished. The three volumes under review, along with the recently published second edition of Vicky Randall's *Women and Politics* (1987), are part of this ongoing wave of research.

The three books are very different in conception. The prolific Pippa Norris, senior lecturer in government at Newcastle Polytechnic in Britain, has written a quantitative, comparative, multiple indicator study of factors affecting women's social, economic, and political equality in 24 first-world countries. The 14 essays in Katzenstein and Mueller are empirical—although seldom quantitative—studies of women's movements in seven Western countries. Coverage is uneven. The United States is the sole empirical referent of 4 chapters and is treated comparatively in 4 more, and Italy is the subject of 3. Stetson's book is a heavily documented examination of how, over the last 20 years, France has moved from being one of the Western European states least favorable to women's equality to a position of leadership in women's legal rights.

What unites these diverse approaches is a common emphasis on feminist movements as stimulating collective consciousness, institutional alliances, and public policy changes over the last two decades. All three volumes acknowledge that various strands of feminism, labeled radical (separatist), socialist, and liberal (reformist) exist; but the authors nevertheless see these groups as sufficiently united in the short term to have an impact. Stetson argues that feminist advocacy groups and their

capacity to penetrate the state via ministries for women in the Giscard and Mitterrand presidencies have been major agents of change in France. The Katzenstein and Mueller contributors are more concerned with how feminist groups have raised the general level of consciousness of women's political problems and how such groups have formed alliances, however temporary, with political parties. Public policy shifts attributable to these movements and alliances are not systematically analyzed across the seven countries. Norris, on the other hand, is the least concerned with the dynamics of group formation and consciousness raising but concentrates on the substantial public policy variations across Western democracies.

All three books explicate the problematic relationships of women's movements with leftist parties. As ideological advocates of greater social, economic, and political equality, such parties are often expected to be the natural allies of women's movements. But several analyses show that despite some notable successes, the relationships have been stormy. The key problem is that leftist parties continue to focus primarily upon the politics of class, which most feminists consider of secondary importance at best. Little serious attention is paid to how rightist groups and parties resist or accommodate feminist demands, a topic brilliantly analyzed by Jane Mansbridge in *Why We Lost the ERA* (1986).

This raises the issue of what theoretical contributions these volumes make. Norris casts her work in the familiar theoretical mold of the argument about whether public policy variations are due to socioeconomic forces only or are also influenced by political factors. She argues for the latter, claiming that leftist party control of government makes a difference for women's equality. This argument, however, does not hold for all areas of policy and is not buttressed by a detailed examination of leftist influence in any specific countries. Several of the Katzenstein and Mueller contributors utilize social movement theory, largely in an ad hoc fashion. Stetson concentrates on how feminists in France have been able to change the definitions of public policy issues affecting women over the years. She calls this approach studying the logic of policy problems. Aside from Norris, these perspectives rarely go beyond lower-middle-range theory.

What alternatives are there? An unresolved problem in these analyses is how to account for political change in the form of the rise and public policy impact of women's movements. Ronald Inglehart has argued that postindustrial society generates different value cleavages than industrial society. Despite much discussion of the feminist problem of allying with class-oriented parties, none of the authors in these books takes up the task of evaluating the utility of Inglehart's argument. Other possibly fruitful theoretical perspectives to use are those of political agenda, policy sector, policy culture, and corporatism. Some of these perspectives are given a passing nod, but nowhere in these books is there an extended application of any of them.

In short, what is needed now is a systematic, theoretically oriented, comparative attempt at explaining the growth and impact of women's movements across developed polities. Norris points in the right direction, but there should be more work sensitive to variations within countries as well as across them. Then one might be able to explain the Swedish paradox, in which governmental policies favorable to women's equality (many would argue *the most favorable* in the Western world) exist without a strong feminist movement to lobby for them. Then the tension and the variable outcomes between women's protective policies and individual advancement policies, recognized by Stetson and most popularly addressed by Sylvia Ann Hewlett in *A Lesser Life* (1986), might be explainable in the context of the political cultures and policy processes of different Western states. As the books under review show, the study of women in politics has come a long way. But there is still a long way to go.

DONLEY T. STUDLAR

Centre College

Italy at the Polls, 1983: A Study of the National Elections. Edited by Howard R. Penniman. Durham, NC: Duke University Press, 1987. 216p. \$39.95 cloth, \$13.95 paper.

The 1983 Italian parliamentary elections resulted in a postwar electoral nadir for the Christian Democratic Party (DC) (32.9%, down from 38.3% in the previous elections).

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The Italian Communist party (PCI) maintained, in contrast, a relatively stable 29.9% (30.4% in 1979). Two minor parties fared well: the Republicans (PRI, 5.1% from a previous 3.0%) and the Socialists (PSI, 11.4% from 9.8%). *Italy at the Polls, 1983* analyzes these results.

The extent of the DC's 1983 losses and the reasons for them are examined in Douglas Wertman's chapter on the Christian Democrats as "big losers." Wertman presents extensive discussion of the DC internal reforms under Ciriaco de Mita and examines where the DC lost votes and to whom. Wertman does not overdraw the long-term consequences of the 1983 elections: the DC losses were "A continuation of its crisis of the mid-1970s . . . a result of the combination of the long-standing and profound disillusionment with the DC's rule in Italy and the diminished usefulness of the anti-Communist appeal in 1983" (p. 50). Wertman's claims that extreme changes in the Italian party system are unlikely and that Mita's strategy of renovating the DC did not have enough time to succeed before the 1983 elections have been borne out by the 1987 results.

According to Marcello Fedele the 1983 elections marked crucial but less obvious changes for the Communists. The PCI was able to maintain its 1979 electoral showing in the context of a "new political cycle" (p. 60). In the 1983 campaign the PCI attempted to express a new Communist identity, choosing to (re)present itself as a governing alternative, having rejected the "historic compromise" strategy in 1980. At the same time, democratic centralism as an internal organizing principle became a serious issue. Fedele provides a complicated discussion of internal party struggle over organizational issues and concludes that "the Communists [emerged] from the 1983 elections with no more votes and with the same problems that they have had since they launched the policy of the alternative" (p. 74).

The experience of the PSI in the 1983 elections illustrates how the minor parties in Italy can translate very modest electoral results into significant cabinet positions, as well as the extent to which the DC—as a dominant, but not majority, party—is dependent on them to retain its position in government. Despite Bettino Craxi's 1983 appointment as prime minister, it is still unclear whether the PSI will have any greater role in governing Italy than that

suggested by Robert Nilsson in his chapter on the Socialists: "the PSI . . . must be either a subordinate or a destabilizing element in any center-left coalition it joins. . . . The PSI can veto any unwelcome DC initiatives without being able to impose its own will on its partners" (p. 79). Nilsson provides a useful discussion of PSI campaign proposals, and recognizes that despite its modest success in 1983 and ascension to the prime ministership, the PSI has not been able to attract large numbers of new voters, particularly among "the emerging classes" (p. 91). While the PSI increased its share of the vote in the 1987 elections, it is not yet clear from whom those votes were received.

Giacomo Sani's excellent essay on the electorate's "ambiguous verdict" examines the changes in voter choice in 1983 and the reasons for the DC's unusually poor showing. Examining electoral returns on a province-by-province basis, Sani concludes that "changes in party strength in 1983 have not significantly altered the preexisting pattern of vote distribution over the national territory" (p. 26). The record defections among former DC voters did not automatically benefit the PSI. Italian voters still employ Left-Right distinctions when evaluating political parties and are not inclined to move far from their party of previous choice. However, the minor parties are becoming a viable alternative among a particular segment of voters under certain restricted conditions in the context of continued strong core support for the DC and the PCI.

Robert Leonardi, in his controversial chapter on the small parties, claims at the outset that "the 1983 parliamentary election signaled the opening of a new era for the minor parties, an era characterized by a fundamental restructuring of the party system" (p. 100), although not by a major transformation of the electorate. Since the minor parties that gained votes in 1983—the PSI, the PRI, and the Social Democrats (PSDI)—are those that have been essential for the maintenance of a PCI-excluding center-Left coalition, even modest increases in their share of the vote could have an impressive impact on the balance of party power. (Leonardi claims these parties were particularly successful in converting their votes into parliamentary seats.) The implications of the minor parties' 1983 successes are a major issue of disagreement among contributors to

this volume. While Leonardi's fine chapter is bound to provoke debate about interpretation and prediction, it will serve as an essential resource for those who wish to know more about the small parties in the Italian system.

Italy at the Polls, 1983 also includes Carol Mereshon's solid essay on the role of the Italian labor unions, William Porter's chapter on the mass media, and Joseph LaPalombara's reflective concluding chapter. There are several extremely useful appendixes, including Senate and Chamber district electoral results and Douglas Wertman's concise explanation of "government formation in Italy."

Italy at the Polls, 1983 is somewhat disappointing. Its major failing is that it is not a coherent work. First, it lacks an introductory chapter that would provide a context for understanding the 1983 elections. Had there been a substantive introductory chapter (for example, see Sidney Tarrow's superb introduction to *Italy at the Polls, 1979*), this volume would have been an outstanding work on Italian politics, rather than an uneven collection on the penultimate Italian elections. Second, the volume's intended audience is unclear. For example, Fedele's chapter on the PCI will be incomprehensible to all but those who are fully cognizant of contemporary Italian politics, yet Norman Kogan's background chapter is clearly intended for the uninitiated. However, scholars will still be well served by reading this volume. Like its predecessors, *Italy at the Polls, 1983* is an excellent resource volume, and several of the contributions are essential reading.

KAREN BECKWITH

College of Wooster

Ireland at the Polls: 1981, 1982, and 1987: A Study of Four General Elections. Edited by Howard R. Penniman and Brian Farrell. Durham, NC: Duke University Press, 1987. 275p. \$52.50 cloth, \$17.50 paper.

This book is part of a national election study series under the general editorship of Howard Penniman, published jointly by the American Enterprise Institute and the Duke University Press. An earlier volume dealt with the 1977 parliamentary elections in Ireland.

The Irish elections are interesting from several perspectives. First, Ireland is the only major country to employ the single transferable vote (STV) system of proportional representation for choosing members of both houses of the national legislature. Second, Ireland has had comparatively stable electoral and political systems, with the two largest parties displaying little ideological difference. Third, the Republic of Ireland's political system has sustained considerable stress in recent years due to serious economic and budgetary difficulties.

Several distinguished analysts of Irish politics contributed to the volume. The book is comprised of chapters by John Bowman, Basil Chubb, John Coakley, Brian and David Farrell, Peter Mair, Joseph O'Malley, and Richard Sinnott, as well as two additional chapters by Brian Farrell. Basil Chubb wrote an appendix on voting procedures, and Richard Scammon compiled an appendix providing electoral data. Howard Penniman's preface provides a useful brief discussion of the Irish STV system in comparative perspective.

The essay by Basil Chubb and the concluding essay by Brian Farrell and David Farrell assume a somewhat gloomy hue in their treatment of the ability of the Irish parliamentary system to cope effectively with economic crises confronting the nation. In Chubb's words, "Although Ireland is still a parliamentary democracy, there are no grounds for complacency about its prospects for remaining so. The democratic mainspring, its representative institutions, does not function well" (p. 207).

The excesses of democracy, the ineffectiveness of corporatist politics, and weakness of the parliament are themes that make these two essays and the volume as a whole of particular interest to those interested in democratic political development. The concluding essay suggests the need for a new constitution, noting the necessity for reform at the administrative, parliamentary, and executive levels. Given the pressures of crisis management, Brian and David Farrell see little chance that such structural reform will actually take place.

The chapter by Richard Sinnott provides an interesting analysis of patterns of electoral support, noting—among other salient points—the absence of a significant class basis to Irish politics for the elections studied. Peter Mair provides a useful discussion of efforts to centralize party control in a political system noted

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for the importance of localism in electoral politics. Maurice Manning's chapter on women in politics will be of interest not only to students of Irish and electoral politics but also to students of women and politics. The tension between powerful conservative and male-dominated institutions on the one hand and a developing women's movement on the other, suggest that Ireland may be an interesting case study for this issue, particularly in light of such phenomena as constitutional prohibitions against divorce and abortion. John Coakley's contribution on elections to the weak Irish Senate is of interest because Ireland has the only national parliament with a house chosen along occupational and functional lines. All chapters of the volume are competently done.

Unfortunately, most of the book is dated, since only the concluding essay deals explicitly with the 1987 election and its ramifications for the party and political systems. It is a pity that the other chapters could not have been updated, if only to include brief concluding sections on the impact and implications of the 1987 election.

This book, like the series of which it is a part, is a useful addition to the political science literature dealing with electoral and party politics. It also constitutes a significant addition to available literature on the Republic of Ireland.

DAVID E. SCHMITT

Northeastern University

Women, State, and Revolution: Essays on Power and Gender in Europe since 1789.
Edited by Sian Reynolds. Amherst, MA: University of Massachusetts Press, 1987.
\$25.00 cloth, \$9.95 paper.

This anthology of readings deals broadly with issues relating to women and power in Germany, France, Britain, and Russia since 1789. The articles are drawn primarily from a set of papers presented at the University of Sussex in 1985. While the editor suggests that "what we did was to take a central notion and to contribute from our different fields a variety of essays—case studies—that would not have been possible say, ten or even five years ago" (p. xi), the "central notion" that animates the collection is never made explicit. Hence, the

book contains a series of discrete articles, of a somewhat uneven quality, focusing on disparate eras and aspects of women's lives in European nations. While revolutionary activity is not examined in all the articles, most seem to fall loosely into two categories: articles examining the symbol of woman as a revolutionary figure and articles assessing women as political activists.

The best of the essays included in the volume seek to confront the issue of whether women's presence in political and revolutionary efforts is significant in promoting change or progress. Beginning with an essay on women's politics and sexuality in Delacroix, several of the articles examine the abiding power of woman as a symbol in revolutionary politics. Marcia Poynton explores the symbol of Delacroix's *Liberty on the Barricades* as occupying an uneasy and hitherto unexplored terrain between freedom and license. In a related essay, Sian Reynolds argues that the connection modern French feminism has made with the establishment of the French Democratic Parliamentary Republic in 1789 is based on flawed analysis. Reynolds juxtaposes the symbol of Marianne, a representation of the French Republic, with the actual, unemancipated role of women in the Republic. Reynolds argues that far from liberating women, the French Republic was an agent of their exclusion from the public sphere, following Rousseau who viewed women as a destructive political force. She demonstrates that politicians, historians, and feminists alike have accepted the view, bolstered by anticlericalism, that women were unworthy participants in the Republican polity. Perhaps the most unusual variant on the subject of women and revolution included in this collection is presented in Ulrike Hanna Meinhof's "Revoluting Women" which attempts to link media representation of terrorists in West Germany with the press treatment of Greenham Common women in Britain. I am prepared to accept the author's argument that the press may seize on women acting in nonconventional roles (the "second face" of Delacroix's *Liberty*) and pervert their role in political protest. But it is a far cry from the nonviolent antinuclear efforts of Greenham Common women to the Baader-Meinhof terrorists in West Germany. Beyond making the point that the sensationalistic press distorts gender-related factors by comparing these two

political activities—one pacifistic, one terrorist—the article virtually equates the two movements and legitimizes the latter. Is this what the author really intended?

Other articles tackle somewhat less controversial topics. Beryl Williams portrays the Russian revolutionary Alexandra Kollantai as a fighter for women's liberation within a socialist, collectivist society. Her vision of a "new society" apparently has much in common with aspects of contemporary reality in modern-day Scandinavian politics. Kollantai emphasized the importance of women's entering the labor force in order not to have to rely on bourgeois marriage and being free from housework and child care through the provision of alternative collectivized services. Kollantai, however, saw motherhood as a duty, lacking a vision of reproductive freedom as part of her concept of a new life for women. In a very different kind of analysis, Sybil Oldfield argues for a reinterpretation of women's role in the anti-Nazi resistance, stressing the significance of those who overcame their subordinate role in order to seek to save Hitler's victims.

The essay most concerned with the role of women as citizens in the existing state is Anne Stevens, who documents the absence of women from positions of public institutional power in Germany, France, and Britain. While Stevens wonders whether the inclusion of women in greater numbers would bring about meaningful change, she does not draw sufficiently on U.S. data (from the Center for the American Women and Politics at Rutgers) or data drawn from Europe (e.g., Sweden) suggesting that female officeholders do bring a more progressive and ultimately supportive view of women's issues to bear on their public roles and responsibilities. Nonetheless, Stevens argues in an unacknowledged Millsian vein that women would both contribute to state affairs and develop their own talents to a greater degree if their participation increased commensurate with their numbers.

The articles presented in *Women, State, and Revolution* offer the reader important new information and provocative insights about, although they provide no definitive answers regarding, the significance of women activists in modern Europe.

JOYCE GELB

City College of New York

Foreign Aid Reconsidered. By Roger C. Riddell.
Baltimore: Johns Hopkins University Press,
1987. 309p. \$35.00.

With the problem of massive deficits confronting many developed states, the justification for continued foreign aid to developing countries was bound to become the subject of careful analysis. Riddell deals with this question by reviewing empirical evidence and the moral arguments for and against aid. This book is not only a thorough analysis of the aid issue but it is also a microcosm of much of the debate on the international political economy of distribution.

Riddell notes that the moral case for aid has either been assumed or tautologically defended. In his discussion of the moral argument (some of the reasons include human solidarity, alleviation of misery, and equality) the author includes much of the normative thought on distributional justice. Thus Rawls, utilitarianism, and Christian thought provide a moral defense. Critics of the moral case include Nozick, Hayek, and Bauer. Key to this minimal state view is that governments should not provide aid but that individuals may.

Other critics of the moral case for aid focus on the issue of national interest and claim that aid should be given only when it is in the national interest. Riddell finds these "rightist" views theoretically flawed.

The book also focuses on the economic theory behind foreign aid. The mainstream view is that aid provides additional resources, which allows for accelerated development. Riddell reviews many cross-national quantitative studies for a consensus on the merits of the mainstream perspective. Unfortunately, the conclusions of these studies are mixed. Riddell attributes the lack of empirical consensus to inaccurate data and faulty methodology.

Riddell reviews the debate around the consequences of aid. Critics on the Left (Lappe, Hayter, and dependency theory) claim that aid only protects the status quo and capitalist economies that hurt the poor. The Right (Friedman and Krauss) argues that aid distorts market forces. Obviously aid cannot both help and hurt the market. The Left and the Right do agree that the state is strengthened.

Riddell analyzes several micro and macro aid projects. The conclusion here is that while there is much evidence to indicate the limits of

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aid, Riddell is not willing to call for an end to all aid. The reasons for the failure of aid are complex: political structures, economic policies, resource use, policy coordination, and bottlenecks. "Part of aid's failure lies in the very nature of underdevelopment" (p. 184).

In the concluding chapter, while Riddell notes that "there is nothing automatic about the link between aid and development" (p. 267), he gives qualified support for continued aid. The justification is largely a moral one combined with the fact that sometimes aid works. Riddell also is critical about the amount of aid usually given: "A trickle of water into a dry hot riverbed will sink without trace" (p. 270).

A deficiency of the book is that there is little discussion of aid from the perspective of the recipient state. It would be in order to analyze how state sovereignty obstructs donor states' ability to create the conditions in which aid would work.

Riddell's analysis is thorough and is likely to be viewed as significant work. Academics should be pleased that this practitioner (Riddell is a research officer at the Overseas Development Institute in London) has a solid grasp of the academic literature, including the normative views on distribution. Riddell's conclusion, however, given the extensive antiaid evidence he cites, is something of a leap of faith.

GEORGE J. CVEJANOVICH

Barry University

Multidimensional Terrorism. Edited by Martin Slann and Bernard Schechterman. Boulder: Lynne Rienner, 1987. 138p. \$23.50.

Few issues in recent times have generated as much interest as terrorism. Its popularity as a subject of scholarly inquiry is evidenced by the number of journals and conferences specifically devoted to it and the variety of books and articles published concerning this phenomenon. Given this interest, it is surprising that so few authoritative studies have emerged. Indeed, the best treatments remain the seminal works published over a decade ago by Walter Laqueur and Paul Wilkinson.

The aim of this slim volume, in the words of one of the editors, is to provide "explanations

of aspects of terrorism by highlighting and examining several basic theoretical approaches to the subject and offering complementary case studies" (p. ix) and thus presumably to fill a gap in the literature. To this end, six essays addressing the theoretical dimensions of terrorism with five accompanying case studies are offered. A useful appendix detailing terrorist campaigns waged by both governments and nonstate actors since 1945 is also included.

The book is divided into three parts. In the first part "Theory Building and Terrorism," Norman W. Provizer covers familiar ground in tackling the problem of defining terrorism. Robert Maranto examines the rationality of terrorism by employing the economic theory of collective (or "public") goods to describe the goals and motivations of terrorist organizations. Bernard Schechterman's provocative discussion of what he terms "irrational terrorism" is by far the most interesting essay in this part. Schechterman persuasively argues that in places long afflicted by violence, such as Northern Ireland, South Africa, Lebanon, and the Israeli-occupied West Bank and Gaza Strip, a new generation of terrorists has emerged among children who regard violence as a normal if not "automatic" means of societal interaction and communication (p. 24). This development, he concludes, has created a situation where indiscriminate violence, "once politically or religiously motivated, increasingly reflects unthinking behavior or outright hatred unconnected to rational goals" (p. 25).

The second part of the book focuses on more specific theoretical issues, such as counterterrorist strategy, terrorism perpetrated by the state, and the rhetorical significance of the terrorist act. Martin Slann trenchantly delineates the "characteristics, graduations, and variants" of state terrorism directed against its own citizens (pp. 39-42) and the "primitivism and zealotry" of state-sponsored *international* terrorism committed by such countries as Libya, Syria, and Iran (pp. 42-43).

The third part contains four case studies and a concluding essay on the difficulties faced by democratic societies in combating terrorism. It is in this part that the book's main weakness becomes apparent. Despite the editors' ambitious effort to link these case studies to the preceding theoretical discussions, they fall short of that goal. Leonard Weinberg and William Lee Eubank, however, provide a fascinating

examination of the contrasting demographic characteristics of Italian terrorists before and after 1977, which illustrates the manifold differences in the socioeconomic composition of successive terrorist generations. "Over time," they conclude, "the terrorists tended to become younger, less politically experienced and more marginally committed to the groups to which they were affiliated" (p. 93). Edward P. Moxon-Browne's excellent analysis of the social and political alienation that fuels terrorism among the Catholic minority in Northern Ireland is of a similarly high caliber. Of special interest is his cogent discussion of the policy choices available to the British government in view of this situation.

Considerably less impressive are Michael M. Gunter's examination of Turkey's response to Armenian terrorism and Gregory F. Rose's discussion of the mobilization of the masses during Iran's revolution in 1978-79. In fact, the inclusion of Rose's essay in this volume is questionable, given that it has little or nothing to do with terrorism. Finally, William Lasser uses three historical examples (Lincoln's attempts to deal with the secessionist threat of 1861, the internment of Japanese Americans during World War II, and Britain's experience in Northern Ireland) to provide a serviceable overview of the problems faced by democratic societies when confronted by terrorism.

With the exception of a few of the essays (Schechterman's, Slann's, Weinberg and Eubank's, and Moxon-Browne's), the material presented in this volume is disappointingly jejune. It is a book that will appeal neither to the specialist, who will find little that is new or especially interesting, nor to the general reader, who will find the theoretical aspects of the book unnecessarily dense. Both types of reader will probably prefer to turn to the standard, if older, works on this subject or perhaps to the similar but far superior collection of theoretical analyses and case studies edited by Peter Merkl, *Political Violence and Terror* (1986).

BRUCE HOFFMAN

RAND Corporation

Koreans in the Soviet Union. Edited by Daesook Suh. Honolulu: University of Hawaii, 1987. 138p. \$17.00 cloth, \$13.00 paper.

This volume pioneers the largely overlooked study of Soviet Koreans within the context of a Korean independence movement. Five essays provide a useful introduction to the study of issues and related areas beyond the scope of the book. Throughout the collection the term *Soviet Koreans* is used to identify Koreans who, for one reason or another, left their homeland and settled in territories that became part of the Soviet Union.

"The Korean Movement in the Russian Maritime Province 1905-1922" by Teruyuki Hara is a cogent review of the Korean anti-Japanese movement in the Russian Maritime Province. In his description of the Korean independence movement during this formative period in Soviet history, Hara assumes that Japanese elitist fear of anti-Japanese activities by Soviet Koreans was a significant factor in shaping Russo-Japanese relations. He establishes imperial Japan's perception of a threat and describes the apparent willingness of Tsarist and Soviet governments to suppress loyal Soviet Koreans for the sake of amicable Russo-Japanese relations, thereby portraying Soviet Koreans as victims of bilateral diplomacy.

Haruki Wada's "Koreans in the Soviet Far East 1917-1937" expands the theme of oppressed Koreans, emphasizing fluctuating social status before and after the October Revolution. His well-researched survey includes a significant event ignored by Soviet historians—the forced relocation of Koreans from the Soviet Far East to Central Asia. The description of vacillating Soviet policies and their socioeconomic impact on Koreans, while important, is incomplete. Further research should include analyzing policy reversals in terms of Russian domestic politics. In addition, the Russian exploitation of Koreans as cheap labor and Soviet security concerns about ethnic minorities residing on the USSR's periphery merit more study, especially in light of continued Soviet efforts to develop Siberia and Gorbachev's Vladivostok pronouncement that the Soviet Union is and will remain an Asian power. Excellent companion reading for these concerns is Rodger Swearingen's recent volume, *Siberia and the Soviet Far East* (1987).

The unique history of Soviet Koreans is framed in terms of USSR nationality problems

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by Youn-cha Shin Chey in "Soviet Koreans and Their Culture in the Soviet Union." From this cultural perspective the author provides an insight into the competition between Soviet assimilation processes and the struggles of minorities to preserve elements of their culture while still achieving social mobility. More importantly, Chey also provides a historical background from which we can develop significant questions. For example, his description of a dilemma encountered by Tsarist administrators—exploiting cheap Korean labor while avoiding Korean economic success that would threaten the social order—raises important questions today as Soviet economic reforms are contemplated: Are current Soviet institutions capable or willing to deal with successes of adaptive, achievement-oriented cultures? Will the slow pace of "Russification" be deemed a necessary impediment to new methods of socialism? To these questions and many others Chey provides an inquiry.

Although the introductory remarks are slightly misleading (the author implies that he will compare Koreans in the Soviet Union with Koreans in Japan), Hidesuke Kimura's "Korean Minorities in Soviet Central Asia and Kazakhstan" compares Koreans with other Soviet nationality groups to yield sound conclusions. Using population data and properly qualified arguments, he criticizes the absence in Soviet literature of the mass deportation of Koreans to Kazakhstan and asserts this action to be the precedent for later deportations of other nationalities, which were also prompted by internal security fears. Kimura categorizes Koreans in the USSR as generally urban dwellers or wealthy farmers who have diligently achieved success in the Soviet system without eschewing Korean traditions. His study should stimulate not only ethnographic analyses that compare degree of assimilation and "success" but also examinations of the implications associated with a shrinking Russian population attempting to control a territory of increasing minorities. A related work that places Soviet policies into a perspective of broader regional goals is Donald Zagoria's *Soviet Policy in East Asia* (1982).

Dae-sook Suh concludes the volume with "Soviet Koreans and North Korea," where he outlines characteristics and activities of Soviet Koreans in North Korea and offers explanations for their lack of influence. The fluctuat-

ing fortunes of Soviet Koreans are examined as consequences of Soviet-Korea and Sino-North Korea relations as well as internal factors, such as lack of Soviet Korean organization, ideological differences regarding party constituency, and Kim Il-sung's obsession with *juche* style independence.

Koreans in the Soviet Union is a short compilation that focuses attention on an important but neglected topic while encouraging broader research. Clearly, it does not profess to be all-inclusive. For example, reports of North Koreans' working in Soviet forced labor camps or Soviet Koreans' role in Siberian development are not mentioned (see Allen Whiting's *Siberian Development and East Asia: Threat or Promise?* [1981]). With this understanding of its scope, readers will appreciate this capable introductory inquiry because of its application to a wide range of scholarly interests.

THOMAS A. DROHAN

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The Rise and Fall of Italian Terrorism. By Leonard Weinberg and William Lee Eubank. Boulder: Westview, 1987. 155p. \$25.00.

Domestic terrorism in Italy during the "leaden years" of the 1970s was unprecedented among democratic polities in the postwar era. Although it is true that terrorism was widespread in many countries in these years, the scale of Italian terrorism was more like that of regimes in the process of democratic "breakdown." Instead, Italian democracy has emerged with a renewed vitality in terms of economic health and the efficacy of state institutions. The authors of this important study examine the role and effect of terrorism in industrialized societies and provide a balanced and intelligent account of the Italian case.

The problem in explaining the causes and decline of Italian terrorism has been complicated by a plethora of self-serving analyses, particularly among Italian scholars. Weinberg and Eubank have instead relied on their own analysis of carefully gathered data. Most studies of Italian terrorism have been either highly speculative or apologetic of the motives of the terrorists. Joseph LaPalombara's recent *Democracy Italian Style* (1987) goes so far as

to lay a large portion of the responsibility for the growth of domestic terrorism in Italy at the door of an indulgent press and intellectual community. Weinberg and Eubank break new ground by focusing their study on survey research and empirical analyses of many factors—both within the Italian political system and among the terrorists themselves.

The authors shed light on three related and significant areas of comparative study: a definition of domestic terrorism, a study of the democratic polity in transition and under stress, and an objective portrait of Italian politics.

The authors combine cultural and historical analysis with the use of multiple regressions and biographical data on the terrorists (2,512 individuals). Terrorism perpetrated by Italians against their own state can be distinguished by two periods: the early period 1970–76 and the late period 1977–84.

The early period was dominated by neo-Fascist violence that was most often aimed at provocation, typically indiscriminate as to its victims, most deadly in terms of the volume of deaths, and generally secretive and privatized. The late period was dominated by leftist violence by a great number of organizations, most notably the *Brigate Rosse*, or Red Brigades. This revolutionary terrorism was more public in claiming responsibility, usually chose its victims carefully in relation to their “political crimes,” and exceeded the neo-Fascists in the volume of activities and membership. Both types were widespread but exhibited concentrations within certain regions of Italy and significant characteristics relating to the socioeconomic background and upbringing of the perpetrators.

These arguments and ultimately the authors’ conclusions, are based on layers of empirical analysis. The research is persuasive, but greater sensitivity to the *spettacolo*, or “drama,” character of Italian politics would have been helpful.

Why did this happen in the 1970s, who were the terrorists, and how did the Italian state survive? While recognizing numerous influences, Weinberg and Eubank decline to be bound by explanations seeming to say that (1) Italian society and culture is violent and polarized and (2) the state is too inefficient and disorganized by multiparty conflict, corruption, and patronage to administer to society. Such explana-

tions would be simplistic and inaccurate.

Moreover, analysis of the type of violence showed that a significant part of the second (1977–84) wave was a kind of warfare between the terrorists and the state. A large proportion of the attacks were aimed at the legal establishment or security agencies or were acts of revenge carried over from the first wave.

The authors further discovered that members of Italian terrorist organizations could not be easily categorized. Italian terrorism was widespread and did not correspond so much to socioeconomic status and upbringing as to political ideas and to a lack of consensus endemic to the Italian polity. The authors found a strong correlation between ideas and the commitment of the terrorists. Terrorist organizations were very much a reflection of the splintering of the major political parties, so that “one could do worse than to consider their groups as miniature versions of right- and left-wing European political parties” (p. 89).

Implicit in this study but partially overlooked is the linkage to the historical lack of consensus within Italian politics and the effect of regionalism on the national identity. The strength of terrorism also reflects the disagreement among the Vatican, the liberal center, and the Socialist tradition in Italy dating to the *Risorgimento*. Finally, the authors seemed to underestimate the possibility that the Italian Communist party’s gradual move to the center may have encouraged terrorism through frightening the far Right and frustrating the far Left simultaneously.

As the authors analyzed the response of the state they concluded that certain techniques, particularly the state’s use of “repentant” terrorists and liberalized arrest procedures, were efficient in putting down terrorism. In fact, the state response reflected the wishes of a people tired of the violence and polarization. With the kidnapping of Aldo Moro the terrorists crossed the Rubicon and, as the authors imply, may have thereby induced greater democratic consensus. The irony is that Italian terrorism that sought to destroy the liberal democratic state may have abetted the growth of a strengthened Italian center.

Weinberg and Eubank regrettably avoid going into greater detail on the emergent stability of democracy, if not of individual governments, in Italy. Significantly, they discovered some useful lessons Italy learned in the process

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of controlling terrorism in an environment that could have easily called for the extreme contraction of civil liberties. They conclude that terrorism did not come close to defeating Italian democracy. It was ultimately public support of the state that strengthened its ability to decapitate and defuse terrorist organizations.

This small book is an innovative and pro-

vocative contribution to the field and will be useful for the classroom and specialists alike — and all interested in the evolution of democracy in industrialized societies.

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INTERNATIONAL RELATIONS

The Strategic Defense Initiative: Shield or Snare? Edited by Harold Brown. Boulder: Westview Press for Johns Hopkins Foreign Policy Institute, 1987. 297p. \$24.95 paper.

SDI: Has America Told Her Story to the World? Report of the IFPA Panel on Public Diplomacy. By Dean Godson. Washington: Pergamon-Brassey's for the Institute for Foreign Policy Analysis, 1987. 73p. \$9.95 paper.

In his March 1983 "Star Wars" speech, President Reagan reopened the doctrinal "closet" into which the issue of strategic defenses had been left since the signing of the 1972 anti-ballistic missile (ABM) treaty. This renewed doctrinal debate on the utility and feasibility of strategic defenses has had to attempt to overcome a number of new hurdles, such as explaining what the Reagan administration's strategic defense initiative (SDI) program actually is; whether its goal is a nationwide "impenetrable shield" or only point defense of intercontinental ballistic missile command and launch facilities; and what its implications are for U.S. domestic politics, NATO alliance relations, and superpower relations.

The two books under review join a publishing growth industry of books and articles on SDI, though former defense secretary Harold Brown asserts at the beginning of his collection that "the issues and arguments have not changed much since the spring of 1983" (p. 1). He contends that there is "the need for a substantial U.S. research and development program on SDI," with the only real question being "the appropriate level and nature of an

R[esearch] and D[evelopment] program" (pp. 4-5).

The 10 tightly written papers in his collection are part of the results of a Foreign Policy Institute study group on the issue of military uses of outer space, some of which have already been published elsewhere. Though sensibly divided into three functional parts based on "thinking" about SDI, its feasibility, and its desirability, the contributions in each part tend to be uneven in their approach and range of assessment.

Alternatively, four of the papers can be read together as a well-outlined overview of the nature and implications of SDI. Stephen J. Hadley provides a brief but concise introduction to the nature of SDI, while Congressman Dave McCurdy argues that Congress will continue to fund SDI as a high-priority project, though not above 1987 levels (p. 190). With regard to the Soviet Union, Bruce Parrott suggests that it would be prudent to pursue ballistic missile defense research while scaling down SDI if the latter's main value is "as a hedge against unpredictable Soviet behavior and as a bargaining chip in superpower arms negotiations" (p. 225). And lastly, the late Robert E. Osgood sets out the implications of SDI for U.S.-European relations as seen from this side of the Atlantic.

For those concerned with SDI's feasibility, Brown's own paper argues that defensive technologies for area defense appear cost-effective in the near term (10-15 years), though defense of populations through the year 2010 and beyond looks poor (p. 138). As to the fiscal and economic implications of SDI, Barry M.

Blechman and Victor A. Utgoff suggest that the "opportunity cost" of deploying a comprehensive strategic defense system is possible if the additional revenue could be raised and alternative potential benefits are foregone (pp. 178-79). And with regard to SDI's impact on U.S. foreign policy, Robert W. Tucker argues that a transition to strategic defenses would "in all likelihood leave U.S. foreign policy substantially where it would be without SDI," with the greatest potential effect being "to reassert the primacy of the United States and the Soviet Union in the international system" (p. 96).

The remaining papers consider SDI in terms of the national security traditions in the U.S. ethos (Michael Vlahos), the technological challenge to superpower geopolitical parity (George Liska), and an assessment of the "rationalistic view of the tradeoffs" (Richard K. Betts).

Dean Godson's slim volume, too, supports a continuation of the SDI program, though it neither explicitly states reasons for such support nor suggests "precisely what the objectives of SDI should be" (p. 50). Based upon the findings of the Institute for Foreign Policy Analysis Panel on Public Diplomacy, the apparent purpose of this special report is to review the Reagan administration's "selling" of the SDI program to Western Europe, with a view to offering policy choices for countering adverse Soviet propaganda while encouraging increased allied support. These include such questionable proposals as the necessity "to create an N[ational] S[ecurity] C[ouncil]-based SDI High Command" (p. 51) to develop a coherent SDI public diplomacy strategy toward Western Europe and other key countries elsewhere in the world. Such a "high command," with its chairman purportedly to hold cabinet rank, would "launch and monitor all Public Diplomacy programs related to SDI," including the efforts of the United States Information Agency-United States Information Service missions overseas (p. 51).

By contrast to his reference to "continued single-minded Soviet opposition to the SDI" (p. 66), Godson's volume is a call for single-minded support of SDI. Through more effective use of public diplomacy Godson hopes to achieve "any substantial changes in European popular or elite perceptions" toward SDI that might be possible (p. 67). He suggests targeting

European national "issue-oriented" concerns to gain such support and goes so far as to name groups and even specific individuals in France, Britain, and West Germany from whom support should be sought (pp. 59, 63, 66). Such a proposal suggests that the United States' European allies would support SDI if only they recognized their own best interests.

As a proposed game plan Godson suggests that the United States must (1) accurately define the concept-product (SDI), (2) ensure that the campaign has the full support of the key members of the administration, and (3) promote SDI overseas with broadly based support at home (p. 50). With regard to the first two objectives, he is basically correct as to the administration's confusion in defining SDI and its goals. But in the case of the third one, Godson appears to have missed a key point, namely that the administration's pursuit of overseas support can and has been seen as primarily intended to influence opinion within the United States, specifically the congressional legislators, based upon broad alliance participation.

In comparison, the collection edited by Brown would encourage a continuation of SDI research and development within the "narrow" terms of the ABM treaty, while leaving considerations of production and deployment of any strategic defenses for the future. Godson's report, on the other hand, calls for enhanced and possibly intrusive administration efforts to promote SDI in Western Europe, based upon the creation of a centralized command of cabinet-level policymakers, though with little apparent concern for its substance or goals.

Evidently written with U.S. audiences in mind, these books offer foreign observers, first, a sense of the intensity that the SDI issue has attained among at least two Washington-based think tanks and second, a sense of the nationalistic "window" through which the United States' allies are seen. Both singularly fail to come to grips with allied concerns about SDI and its widely perceived destabilizing impact on both intraalliance and superpower relations, even during its current research and development stage, despite repeated references to European anxieties and fears over SDI.

But are the Europeans (as well as other U.S. allies) unreasonably fearful of SDI's destabilizing nuclear deterrence, decoupling U.S. and European security interests, limiting NATO nuclear and conventional defense options, and

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so on? The lack of a European perspective or perspectives only conjures up "wimpy" images of allied misgivings over SDI. But such suggestive images cannot cover over the Reagan administration's own confusion as to SDI goals, Weinberger's NATO "invitation" to participate in the SDI program officially (with its "acknowledgement" deadline), or administration "sales" tours to Europe with the inducement of potentially lucrative SDI research contracts upon signing the official participation memorandum.

ROBERT D. HENDERSON

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The Federal Republic of Germany and the European Community. By Simon Bulmer and W. E. Paterson. Winchester, MA: Allen & Unwin, 1987. 276p. \$39.95 cloth, \$17.95 paper.

This book explains the role of West Germany in the European Community. The authors argue that European policy is conditioned by the same factors as shape domestic policy. As a result they present a detailed and comprehensive volume offering analyses of Federal German policy-making processes, interest groups, parties, public opinion, the legislature, the *Länder*, and West Berlin, in relation to the European Community. The book also provides case studies of West Germany policy on the Regional Fund and the European Dimension of its relations with France and East Germany. The authors claim that one cannot make sense of West Germany's European Community relations with either a power politics or a neofunctionalist approach. Accordingly, their study is premised on an interdependence perspective in which governments remain central but are viewed as enmeshed in a complex network of relations with transnational, state, and nonstate actors.

Critical to their analysis is the concept of "policy style." By this they mean the character of the government's approach to problem solving and its relationship with socioeconomic forces in West Germany. The most important source of this style is the constitutional arrangements of the Basic Law. According to the

authors, West Germany is a "vague" actor in the European Community because of the institutional pluralism that has been built into its formal political structures. They refer, for example, to the federal political system, the independence enjoyed by the federal bank and the tradition of ministerial autonomy that exists in West Germany. Such features, the authors argue, combine with a permissive consensus towards the European Community in society at large to produce a distinctive form of policy making. The policy style of West Germany is decentralized and routinized. Its consensual character permits most European Community issues to be treated as technical, rather than political, problems. Further, this process gives rise to very little active, coordinated policy except in response to crises.

As a consequence of this policy style, the authors argue, West Germany is poorly equipped to assume a political *Vorreiterrolle* (leadership role) in the European Community compatible with its current economic status. However, its elites also are not particularly interested in assuming such a role. The reasons for this are quite clear. First, trade in general is of critical importance to the country's economy. The German economic recovery of the 1950s was premised on free access to Western European markets and its health is dependent on continued access. Thus as it presently exists and insofar as it embodies elements of the "social market economy" ideology said to account for West Germany's economic success, the European Community constitutes an attractive arena of economic competition. Secondly, the European Community also provides an attractive environment for political cooperation in that it allows West German government to participate in European diplomacy in a manner that is neither offensive nor threatening to non-Germans or Germans who are mindful of the catastrophe inflicted upon Europe by the "national" imperatives of Nazi foreign policy. The existence of the West German government in its present form, therefore, is a product of, and dependent on, the same political, ideological and economic developments that gave rise to the European Community. Rather than seeking a leadership role that might place this environment at risk, West Germany is content to play the *Musterknabe* (model boy) of the Community, seeking only a single market within the European Community

and freer trade between it and the rest of the world economy.

Of course, the authors note that below the level of grand principle, West Germany is not always a model of *communautaire* behavior. For example, the fragmented policy style of the government has permitted the agriculture Ministry to defend certain price levels for cereal production even though this slows the pace of Community reforms and is at odds with the social market philosophy espoused by the Finance and Economics ministries. Further, West Germany's opposition to Community policies that have an interventionist character has been more sustained when, as in the case of steel, they are seen as undermining a competitive edge enjoyed by the relevant sector of its own economy. Finally, not all moves towards closer European integration are welcomed by all elements within the West German government. The environmental proposals of the recent Single European Act, for example, not only bother the Economics Ministry and its industrial clients, but also pose problems for the jurisdiction of the *Länder* on such issues.

This is a very good book in the sense that it provides a great deal of information about West Germany and the European Community. Its scope is broad, ranging from the early days of the Coal and Steel Community up to the European Community reforms of 1986 and the new issues of high technology development and service industries. There are prices to pay for this detail, however. At times the authors present very dense narratives to set contexts for rather small points, for example, in their discussion of the European Research Cooperation Agency research project and West German policy. Finally, the book should offer a more extensive defense of its theoretical perspective. The authors' research may have led them to adopt the interdependence perspective in preference to realist, neorealist, Marxian, or functionalist approaches. Their study of the forces that shape domestic policy in West Germany reflects this choice but rarely makes the case for it.

PAUL SHARP

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Big Brother: The Soviet Union and Soviet Europe. By Hélène Carrère d'Encausse. Translated by George Holoch. New York: Holmes & Meier, 1987. 332p. \$39.50 cloth, \$24.50 paper.

Soviet influence in Eastern Europe, acknowledged as a principal factor in generating the cold war, continues to be routinely—almost ritualistically—denounced in the West. Although the denunciations may in most instances be richly deserved, they are a poor substitute for efforts to analyze and understand the Soviet Union's relationship with this complex and unlucky region. It is a daunting challenge for an individual author to attempt an explanation of Soviet policy in Eastern Europe from the Second World War to the present day. Such an unwieldy subject is more often confronted in multiauthored and edited works. Hélène Carrère d'Encausse has, however, in *Big Brother: The Soviet Union in Soviet Europe*, accepted the challenge of single authorship with results that on balance constitute a very impressive achievement.

Originally published in French in 1983, the book is now available in an English translation that contains an updated afterword by the author. This addition is particularly important, given the fact that the original version's publication predates the selection of Mikhail Gorbachev as general secretary of the Soviet Communist party.

Divided into three sections, *Big Brother* seeks, in part 1, to describe the ways and means by which Stalin's Soviet Union sought and achieved control over Eastern Europe both during and immediately following the Second World War. Carrère d'Encausse then turns in Parts 2 and 3, to the subject of the repeated attempts by the peoples of Eastern Europe to resist Soviet hegemony and the nonetheless largely successful efforts of the Soviets to overcome resistance and devise new ways to build a "socialist community."

Carrère d'Encausse debunks the "Yalta myth" that at the 1945 conference the West handed Eastern Europe to the Soviet Union. Rather, Yalta "merely confirmed previous decisions and a situation that favored Soviet demands" (p. 31). This is not to say that Stalin had a detailed plan for the subjugation of Eastern Europe. He was not empowered with the capacity to see into the future. Stalin *was*,

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however, pragmatic and adaptable and prepared to take political advantage of the Soviet military presence in Eastern Europe. And Stalin feared the West. Carrère d'Encausse suggests that U.S. atomic capability conveyed an unmistakable signal to the Soviet Union that the balance of power had shifted against it, and that the United States sought strategic dominance. Stalin, therefore, no longer felt bound by his wartime commitments, and he greatly intensified efforts to solidify, and indeed institutionalize, the Soviet hold over Eastern Europe.

Stalin's methods of ensuring political uniformity were often harshest when directed at loyal communists who were nonetheless suspected of national deviationist tendencies. In dealing with this issue, *Big Brother* suffers from an organizational defect in that Carrère d'Encausse chooses to discuss the purges of the late forties and early fifties *before* she describes the Soviet-Yugoslav confrontation, an event that was itself largely responsible for provoking the bloodlettings in Czechoslovakia, Hungary, Bulgaria, and elsewhere. Despite the somewhat confusing chronology, however, Carrère d'Encausse provides us with a depressingly thorough description of the lengths to which Stalin was prepared to go to ensure political homogeneity in Eastern Europe.

In Part 2, entitled "The Fratricidal Empire," Carrère d'Encausse takes up the numerous challenges to Soviet authority that range over the period from the late forties to the eighties and that at one time or another have involved every country in Eastern Europe save Bulgaria. Here she recounts not only the succession of sometimes dramatic crises that have plagued the Soviet Union but also the continuous effort on the part of the Soviets to pursue a policy of "normalization" combining violence and limited concessions.

The attention paid to Eastern Europe in the West, and perhaps especially in the United States, tends to be sporadic and self-serving. It becomes riveted on the region only in times of upheaval as politicians and others seek to exploit discontent in order to embarrass the Soviet Union. What is generally overlooked and what becomes the focal point for Carrère d'Encausse in Part 3 of *Big Brother* is the unspectacular but essentially relentless process engaged in by the Soviet Union to construct an

integrated and permanent socialist system in Europe, similar in key respects to the Ottoman Empire of the Janissaries. Utilizing principally the instrument of the Warsaw Pact as an integrating tool, the Soviet Union's long-range objective has been to create the "habit of living together" (p. 294). Despite repeated setbacks, the Soviets can claim a large measure of success in reaching their goal.

Unfortunately, Carrère d'Encausse's generally scrupulous analysis is marred by her conclusion, in which she attempts to place the Soviet role in Eastern Europe in the context of global politics. This proves to be altogether too ungainly a subject for the few pages devoted to it, with the result that the reader is treated to little more than ominous speculation about the fates of Yugoslavia and Turkey, punctuated by unexamined quotes from Jean-Francois Revel and Sir Halford Mackinder.

The afterword, written especially for the English language edition, bears marks of having been rather hastily put together (e.g., Brezhnev died in 1982, not 1983). But Carrère d'Encausse raises some very pertinent questions about the relationship between *perestroika* in the Soviet Union and the imperative of preserving the socialist system in Eastern Europe. Developments as recent, for example, as those in Bulgaria in 1988 suggest that Gorbachev's Soviet Union, despite the necessity of efforts at internal reform, will continue to keep a very watchful eye on the pace of change in Eastern Europe.

SCOTT MCELWAIN

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Trading Technology: Europe and Japan in the Middle East. By Thomas L. Ilgen and T. J. Pempel. New York: Praeger, 1986. 203p. \$37.50.

Trading Technology is an examination of the trade in high technology between four industrialized countries—Britain, France, West Germany, and Japan—and the Arab states of the Middle East. The book is an outgrowth of a project organized by the Office of Technology Assessment (OTA) designed to examine the policies of major suppliers of technology to the Middle East. The book expands on the OTA report "Technology Transfer to the Middle

East" (1984) because the authors concluded "that our research on the policies of technology suppliers offered broader analytical insights into the dynamics called the generic process of technology transfer. It also permitted us to link scholarly understanding about the unique national features of foreign economic policies to the particular characteristics of technology trade" (p. ix-x).

The book defines technology very loosely as any scientific knowledge that can be put to a socially useful purpose. They examine technology trade in the Middle East within four stages of transfer: technological planning and project design, infrastructure projects and transfer, large-scale industrial imports, and small-scale transfers and specialized technologies.

The authors present a brief overview of trade between the Middle East and the major industrial countries and then concentrate on six different product areas as a representation of technology transferred: nuclear plants, machinery, telecommunications, medical equipment, aircraft and air transport equipment, and arms. As one might expect, market share for products in these categories is specialized and varies over time. Germany, for example, is the leading supplier of medical equipment to the region, France and Britain are the leading suppliers of telecommunications equipment but are now being overtaken by Japan.

The book then devotes four chapters to how the four countries have approached technology trade with the Middle Eastern nations. Each chapter explores the historical context of relations between the industrialized nation and the Middle East, explores trade and investment initiatives by government and the private sector in that country, discusses the activities that support trade and investment policy as it applies to technology transfer, and assesses the national strategy of relations toward the region and accounts for distinctive national features. The chapter on Japan, for example, notes Japan's dependence on oil from the area and the recent origin of its ties to the Arab nations. It briefly discusses the role of government in Japanese industrial policy including the Ministry of International Trade and Industry, the Japan External Trade Organization, and the Ministry of Finance. Japan's success in developing technological sophistication is also attributed to its conglomerates (*keiretsu*); its

banking structure, particularly the Bank of Japan; and the major trading companies (*sogo shosha*).

Then follows a discussion of the Euro-Arab dialog and its complexities and difficulties. The book concludes with a discussion of the need for planning among technology importers, specialization among suppliers, the role of government in trade policy in supplier nations, and potential changes in trade relationships between developed nations and the Middle East.

The book is almost an abbreviated cross between Peter J. Katzenstein's *Small States in World Markets* (1985) on the one hand and Chalmers Johnson's *MITI and the Japanese Miracle* (1982) and M. Y. Yoshino and Thomas B. Lifson's *The Invisible Link* (1986) on the other, but with a Middle Eastern flair.

For those interested in this niche in the trade literature, the book provides a quick overview. It also exemplifies the potential strengths and weaknesses of various types of industrial policies as they apply to trade promotion. But as with so many things in life, strengths are also weaknesses. While brevity makes for nice overviews it does not allow the authors the opportunity to tell us much that we have not been told before.

RICHARD D. BINGHAM

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The Limited Partnership: Europe, the United States, and the Burdens of Alliance. By Josef Joffe. Cambridge, MA: Ballinger, 1987. 225p. \$29.95.

In this analytically rich and eloquently argued book, Josef Joffe who has published widely in the area of transatlantic affairs and who is currently foreign editor of the prestigious Munich daily, the *Süddeutsche Zeitung*, deals systematically with the major past and present problems of the alliance, especially the area of security policy.

In the introduction and chapter 1 Joffe provides a succinct and convincing argument of why and how the alliance suffers from a transatlantic rift. Also addressed are the consequence of a changing European and global order the United States finds it difficult to adjust to, the differing hopes and aspirations the alliance partners attached to the détente proc-

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esses of the 1970s, differing assessments of the nature of the Soviet threat and how the alliance should respond to it, and deeply rooted and divergent historical experiences and cultural proclivities. Joffe's analysis is particularly cogent when he insistently points to the singularity of the Federal Republic's historical, institutional and political position in the alliance—the result also of Germany's peculiar geostrategic and historical middle position in Europe—which explains in large measure why the problems that have plagued NATO were always felt more acutely in the Federal Republic than elsewhere in Western Europe. In stressing the special problems, anxieties, aspirations, and resentments that accompany West Germany's membership in the alliance, Joffe also provides a persuasive set of reasons why there is no "Western Europe" as such and why a full understanding of what ails NATO must recognize that West European states follow divergent foreign policy interests. This places an intra-West European rift on top of the transatlantic rift and compounds the problems the alliance faces on the eve of its fortieth anniversary.

A large but sometimes hesitantly argued theme that runs through the entire book—especially chapters 2–4 is that NATO must reconnect politics and strategy and develop a political-strategic posture that is sensitive to the changing political circumstances of Europe and adequate to the task of meeting the challenges of the post-Intermediate Nuclear Forces era, especially in the area of East-West arms control where Soviet diplomatic dexterity is pronounced. Hopefully, this reconnection could help also in restoring broad domestic support for the nuclear policies of the alliance, support that has become increasingly corroded (especially in the Federal Republic) as a consequence of ill-considered U.S. arms control policies and strident cold war rhetoric.

What makes Joffe's book an especially welcome addition to the spate of books on the transatlantic alliance is its sustained emphasis on the political rather than the purely military-strategic sources and consequences of the tensions within NATO. This is done deftly, interweaving political and strategic considerations in a way that demonstrates—more through implication than exposition—the damaging consequences of U.S. nuclear and arms control policies in Europe during the Reagan years.

There is no need to lay every malaise of the alliance at the doorstep of Washington administrations. (The Europeans contribute their fair share as well.) The alliance has survived so many crises that one feels grateful that Joffe sounds less alarmist than many others who view the present state and future prospects of the alliance (especially its German-U.S. backbone) with growing concern. Even so, not everyone would share Joffe's rather sanguine conclusions—for example, that NATO has done its job well and that with respect to nuclear versus conventional deterrence, "four decades of ultrastability is an impressive argument for the status quo" (p. 166). It is here perhaps that Joffe lets lapse his otherwise persistent stress on the primacy of politics over strategy. For NATO was never intended to serve narrowly defined Western security interests—if that had been its primary purpose, the success of the alliance might indeed be evaluated in equally narrow terms of how it sustained deterrence—but rather was intended to stabilize the postwar European political order by containing both the Soviet Union and the Federal Republic. The real question (and Joffe provides ample evidence for its cogency) is not so much in what ways the alliance can continue to uphold deterrence but whether it can transform its traditional role of double containment and adjust it to the more complex circumstances and challenges of the 1990s and beyond.

WOLFRAM F. HANRIEDER

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Moral Paradoxes of Nuclear Deterrence. By Gregory S. Kavka. New York: Cambridge University Press, 1987. 243p. \$34.50 cloth, \$12.95 paper.

This study tries to answer the question, "Which possible nuclear deterrence policies are morally superior to which other policies, and for what reasons?" (p. 1). Disclaiming any comprehensive purpose, Gregory Kavka does not treat the use of nuclear weapons. Kavka believes that achieving nuclear disarmament is a moral imperative. Reaching the goal, in his view, will not be easy and may not even be

likely. This is the case not only because of the entrenchment of nuclear weapons but also because of the existence of nuclear deterrence paradoxes.

Relying on Thomas Schelling for the identification of several paradoxes, Kavka is chiefly interested in moral paradoxes overlooked by strategic analysis, for example, paradoxes arising out of the moral status of the defender's intention to retaliate. Such "evil" intentions may actually prevent greater wrongs, notably nuclear first strike or nuclear blackmail. The book is replete with carefully delineated moral ambiguities that nuclear abolitionists have failed to address or have dismissed too readily. The study is addressed much more to nuclear pacifists, especially unilateralists (who may be rare), than to ideological and security partisans of nuclear deterrence. One misses references on the prudential side to the Harvard Nuclear Study Group, Ernest Lefever, and Wolfhart Pannenberg and on the apocalyptic side to the American Methodist bishops, who have gone beyond the U.S. Catholic bishops to deny any legitimacy to nuclear deterrence.

The book is composed largely of articles appearing previously in philosophy journals. Well-integrated, the volume addresses, inter alia, deterrence and utilitarian moral theory, the dilemmas of nuclear protest, and nuclear coercion. After concluding that unilateral nuclear disarmament has risks that disqualify it from being a lesser evil as compared to minimum nuclear deterrence, Kavka examines world government as a possibly lesser evil than a minimum deterrence regime. A Hobbesian scholar, Kavka reviews Hobbes' argument against anarchy and his views on international relations as a run-up to analyzing different variants of the "Nuclear world government" argument. To Kavka, the "proliferated world" version of the argument is more persuasive than the present world condition in which only a few states have nuclear weapons. The point is recalled that most members of the nuclear-armed crowd would be highly unlikely to have second-strike capability, thereby undermining the supposed effective equality stemming from possession of nuclear arms.

Kavka might have noticed that both the nuclear mob spectre invoked by Albert Wohlstetter over a decade ago, which is still exploited by antiproliferation sensationalists, and the seldom-replicated proliferation-

favoring advice once offered by Kenneth Waltz in the name of stability have not been realized. The antiproliferation regime has been more effective than even many of its partisans have been willing to admit. The book and cited sources do not mention the leading proliferation challenge, the potential spread of covert bomb states in emulation of Israel.

Identifying well-known problems of gaining surrender of national authority to a world regime, the chapter on world government concludes that its achieving is not necessary to securing nuclear disarmament. World government may not be possible without nuclear disarmament. There are lessons in these findings for the world order school and other anti-statists.

No comfort is offered to the Strategic Defense Initiative. Using game matrices to analyze strategic defense, a chapter concludes that even a "pure defense of the innocent" could be morally flawed, because "such an act could protect the guilty, deflect harm onto other innocents, make aggression by the defended party safer and more likely, or be ineffective as a means of defense" (p. 159).

After disposing to his satisfaction of a "dangers of transition" argument, Kavka places his final trust in "bilateral mutual nuclear disarmament to the lowest level providing a secure second-strike capacity to each side (i.e., minimum deterrence)" (p. 206). For the "very short run" he urges the nuclear giants to adopt a nuclear freeze (undefined), a comprehensive test ban, a ban on space weapons, a continued bar on strategic defense, "large" reductions of nuclear stockpiles, elimination of first-strike strategic weapons, and a no-first-use pledge (p. 207). This highly ambitious agenda may unsettle arms controllers and gradualist disarmers waiting for Strategic Arms Reduction Talks progress, while pleasing abolitionists, whose haste has been questioned in the book. The study has a built-in paradox. Nonetheless, the volume is a distinctive contribution to nuclear discourse.

PAUL F. POWER

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Making and Marketing Arms: The French Experience and Its Implications for the International System. By Edward A. Kolodziej. Princeton: Princeton University Press, 1987. 518p. \$55.00.

France, Kolodziej tells us, is the third largest arms exporter in the world, after the Soviet Union and the United States. Arms represent perhaps only 4%–5% of its overall exports, a ratio comparable to that of the United States and one-third that of the Soviet Union. But on a per capita basis France now sells three times as much as either of these. Why? What impels it? What does the French sales effort mean in terms of the burgeoning militarization of the third world and the prospects for control of the arms trade?

The importance of Kolodziej's book lies in providing answers to these questions in the broad context of historical dynamics and international relations theory, as well as in its impressive marshaling and presentation of data and its detailed analysis of the French military-industrial complex. The anarchic Hobbesian self-help system impels states that claim sovereignty and independence to arm themselves against potential enemies. Yet the revolutionary and expensive technology of our times make it less and less possible for states other than the superpowers to do this on their own, to truly remain sovereign independent entities in today's world. France, however, has seized upon arms sales as a means to maintain an arms industry capable of satisfying what successive French governments have seen as necessary for real independence. Virtually all parties accepted the Gaullist rationale that France's own deterrent, its *force de frappe*, was necessary in avoiding an unreliable and inadmissible U.S. hegemony. Building it, however, put a budget squeeze on conventional forces. Withdrawal from empire provided some relief and allowed France to mend its third-world relations. French policy of sales to third-world countries then helped finance continued conventional weapons development and production. Moreover, it has been rationalized across the years as serving a dual purpose: (1) by giving third-world countries an alternative to purchases from the superpowers it allows them, too, to avoid falling under superpower domination; (2) during a period when superpower conflict is muted by the nuclear standoff, it

helps decentralize power in the world, creating regional, competing centers. If NATO standardization and coproduction has remained something of a chimera, this, too, is because of the imperative of independence.

Moreover, the French Fifth Republic appears as a result to have successfully avoided—even reversed—the choice between guns and butter. Arms production and sales became a way of stimulating a thoroughgoing modernization of the French economy, helping to make it competitive in a world of rapidly changing technology, to make it meet what Jean-Jacques Servan-Schreiber called—25 years ago—the “American Challenge.” The military-industrial complex and its exports thus became essential to the French government's stimulation of employment and welfare. The result of all this is that only extraordinary circumstances will bring a diminution in French resolve to continue arms sales. More third-world states desirous of attaining some measure of independence and modernization may well, Kolodziej argues, emulate France. The imperatives of the anarchic, self-help state system drive states to amass weapons. But to this Kolodziej adds the imperative of modernization: the fact that France's use of weapons production and sales has accompanied the extraordinary transformation of French society in the last quarter of a century has not escaped the observation of decision makers in major new states, and once one state enters into the process, through purchase, coproduction, or licensing, its neighbors follow suit.

Kolodziej argues persuasively that the progressively concentrated arms industry has been crucial to French modernization. But in the course of the process France has created a set of interlocking institutions that are geared to arms sales, divorced from a largely acquiescent public opinion and a generally uninformed legislature, and supported by both the Right and the Left. The Socialists talked about “moralizing” arms sales before coming to power in 1981 but made little change in policy.

Twenty years ago George Thayer's *War Business* (1969), sounded a warning, chronicling the change in the arms trade from the period after World War II, when surplus and private sales dominated the field, to the spread of government sales, based mostly on economic criteria. A decade later, updating Thayer, Andrew Pierre's *Global Politics of Arms Sales*

(1982) stressed the political element in the accelerated growth of arms sales in the 1970s, arguing that they might or might not contribute to local balances or to local conflicts but that they were largely a function of the end of the bipolar postwar world and of the diffusion of power: new states wanted armies and weapons. Arms sales, moreover, represented the continued competition between East and West for influence in the non-Western world. The economic benefits to sellers were less than generally believed, however, and since four sellers dominated the market, a cooperative international endeavor in which recipients worked with sellers to limit sales might have been possible. Kolodziej's new work, in contrast, demonstrates the enormous internal barriers to working out such cooperation and underlines the fact that the complex interdependence of our current world must still be mediated through the institution of the sovereign state. French policy will never run smoothly: the export market shrank in 1987, modernization of the *force de frappe* and construction of the nuclear carrier *Charles de Gaulle* and a potential second one have put a new squeeze on conventional forces. The costs of new equipment continue to rise. The Intermediate Nuclear Forces treaty is taken by some French as the sign of further U.S. unreliability. Franco-German cooperation, while largely symbolic so far, may increase, though differing geostrategic interests constantly interfere. But Kolodziej's first-rate and important book, marred only by some repetitiousness, provides the deepest insight so far into what lies behind French policy and gives us a model to analyze the arms policies of other countries. The work is essential to anyone who wants to go beyond simple condemnation of what has in the last quarter century become an important part of international relations.

CHARLES L. ROBERTSON

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Exploring the Stability of Deterrence. Edited by Jacek Kugler and Frank C. Zagare. Boulder: Lynne Reinner, 1987. 168p. \$18.00.

However much they may differ on the wisdom of particular policies, both "peace studies"

and "security studies" analysts are widely agreed on the importance of the subject of this edited book: how to maintain and increase strategic stability and decrease the likelihood of a nuclear war. In *Exploring the Stability of Deterrence* editors Jacek Kugler and Frank C. Zagare contribute to the extensive literature that uses formal models and game theory to explore the implications of alternative assumptions and force structures for strategic stability. As such, the book reflects both the benefits and the limitations of that approach.

This book is not for those who are allergic to highly formalized arguments or to game theory. But for those who find this kind of analysis useful or are skeptical but willing to invest the effort in understanding, this slim volume will be a worthwhile addition to their libraries. The models are explained clearly, and the assumptions and conclusions are reviewed in the text.

After a useful introductory overview of strategic stability issues and of the book by Kugler and Zagare, Michael D. Intriligator and Dagobert L. Brito explore the stability of mutual deterrence. In the book's most interesting chapter, they argue that mutual deterrence is now extremely stable. This is due chiefly to the large number of nuclear weapons possessed by the superpowers, which renders the power that strikes first vulnerable to a devastating "second strike" by the other. The nuclear arsenals are so large that mutual assured destruction (MAD) is secure for the foreseeable future even in the face of technological change. "Arms control" that seeks only to reduce the stockpiles of nuclear weapons or, worse, to eliminate them is destabilizing and highly dangerous.

A better focus of concern, they argue, is the danger of accidental launch, particularly if either superpower has a "launch-on-warning" posture that could begin a nuclear exchange based on false information. Other legitimate arms control concerns include preserving the system of mutual deterrence (with limits both on antisubmarine warfare and on a totally effective missile defense and with the construction of more systems that could reliably survive a first strike) and preventing nuclear weapons from falling into the hands of unstable leaders. (An additional concern would seem to be the coming to power of unstable or unwise leaders in a country already

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possessing nuclear weapons). They conclude that other arms control mistakes include nuclear freezes, "no-first-use" agreements, and the agitation of public hysteria about these issues.

In "Assessing Stable Deterrence," Jacek Kugler takes a more pessimistic view of strategic stability, arguing that wars are frequently initiated even when antagonists are of equal power and a leader could expect to pay great costs for doing so. This suggests the importance of exploring the risk-taking propensity of leaders as a way of predicting behavior. This is vitally important, but I have reservations about how it is built into the model. In fact, a very low probability of destruction may be sufficient to deter nonlunatics, in view of the catastrophe that the loss of a single city would be. The problem is in determining what level of expected destruction would be so unacceptable as to deter a particular leader (or set of leaders) from initiating a nuclear exchange or, more broadly, from taking any actions that could lead to one. It is most difficult to do this in the abstract, as game theorists know best.

The editors argue in "Risk, Deterrence, and War" that MAD is not a stable basis for deterrence. Even in the face of expected catastrophic losses, risk-acceptant decision makers can destabilize a situation by provoking a nuclear confrontation. Ironically, although risk-acceptant leaders are the most dangerous, risk-averse leaders may also be a threat if they tempt an opponent who knows of this propensity to provoke a nuclear challenge. Risk-neutral leaders provide the most stability.

In "A Darwinian View of Deterrence," Christopher H. Achen argues that certain non-rational factors favored by evolution, such as aggression and willingness to retaliate, powerfully affect deterrence. A "Darwinian" perspective can help to explain outcomes that would not be predicted from rational choice models alone.

Steven J. Brams and D. Marc Kilgour analyze a deterrence game based on chicken in "The Path to Stable Deterrence." The key difference is the addition of a credible threat of retaliation in response to preemption. Although retaliation may not be "rational" in the short run, the expectation of some probability of its occurrence may move the equilibrium of the game from preemption to deterrence. In such a situation, the expectation of a

"nonrational" reaction would actually increase strategic stability.

In "A Stability Analysis of the US-USSR Strategic Relationship," Frank Zagare attempts to account for the lack of superpower war since 1945. Most interestingly, "the capability requirements for successful deterrence are much lower than are generally supposed" (p. 147), at least so long as the credibility of retaliation is high. Stability is not assured either by an excess of missiles or by leaders so obviously concerned about the costs of nuclear war they thereby reduce the credibility of their nation's deterrence.

Formal modeling is absolutely dependent on the analyst's assumptions, in this case about the most important factors for strategic stability: decision makers' preferences and their probable actions in different situations. Unfortunately, it is precisely in this area that the least is known, and different sets of assumptions produce dramatically different conclusions with diametrically opposed policy implications. That some of the conclusions of the analyses are counterintuitive or contradictory should not be surprising. Real-world outcomes are also affected by factors outside the model, such as uncertainty, misinformation, distractions, mind-sets, and chance—and may even be determined by them. To their credit, the editors are aware of this and do not exaggerate the direct applicability of their results. Although final answers are elusive, those who are seeking ideas about how to increase strategic stability will find many suggestions here.

JOHN ALLEN WILLIAMS

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Irrationality in International Confrontation:

By Robert Mandel. Contributions in Political Science no. 185. New York: Greenwood, 1987. 147p. \$37.95.

Irrationality in leadership and decision making is clearly an important topic for study. Nevertheless, it has been a neglected one. In the selected bibliography that Mandel includes in his book there are references to psychological, decision-making, and related literatures, but there is little available directly on the phenomenon of irrationality. This book seeks to fill the gap.

The book includes only one chapter on the

nature of irrationality. In this the author reveals a typology. Then, having explained his methodology, Mandel moves into a series of case studies that seek to demonstrate the different forms of rationality contained in his typology.

The typology is a simple one. There are three types of foreign policy decision making: low fluctuation, which is sluggish and stagnant; medium fluctuation, which is coherent and calculated; and high fluctuation, which is rash and reckless. The first and last of these are termed irrational. The second, medium fluctuation, is rational, being coherent and calculated.

In explanation of his conception, Mandel argues that irrationality can enter into every stage of decision making: identifying the problem, gathering information, arriving at a decision, implementing the action, and the feedback stage.

The sources of such irrationality vary. The author stresses human frailty, constraints on human reason, the bureaucratic environment, the roles that are being enacted within government structures, and the social and cultural contexts. There are also levels of information and time pressures to be taken into account.

It will be seen at once that Mandel is not dealing with madness. Indeed, he makes it clear at the outset that irrational behavior may be conducted by rational persons. His concept would have been still clearer if he had adopted the David Singer notion of *nonrational* rather than *irrational*.

This probably would have led him to an aspect he seems not to have taken into account—the way decisions are made to satisfy an immediate political gain, even though it is anticipated that there will be a longer-term negative outcome. This could possibly be the most frequent source of “irrational” behavior in political life.

The use of *nonrational* would also have helped in dealing with “perceived versus genuine irrationality” (p. 13). His interpretation of case studies gives the impression that in his view there is a large amount of “genuine irrationality” as defined by him, whereas a deeper analysis of the phenomenon could have led him to the view that the cases he was studying were cases of perceived irrationality, perceived madness, for instance, the perception of Iran by the Reagan administration.

Five chapters of this book are devoted to case studies and their evaluation. There is one final chapter on policy implications and prescriptions. There are those who attach importance to case studies as a methodology, but for me case studies are not a prime source of insights, merely examples to put some flesh on ideas derived from the theory within which the cases are interpreted. I would have liked to have seen more attention given to developing the theoretical aspects of irrationality. For example, almost no attention was given to irrationality as a perceived phenomenon, that is, as a means of explaining behaviors that the observer cannot understand, whether through lack of knowledge of, or lack of sympathy with, the reasons that give rise to the observed behavior. Our foreign enemies are mostly “irrational.” This labeling makes unnecessary any further analysis.

Irrationality as a subject is part of decision-making theory, which in turn is part of system science and philosophy. Rational behavior implies commencing with a hypothesis, examining it in the light of all information available in any discipline, deducing an explanation or theory, and deducing from that an approach to the problem—which in turn suggests the application, from which there is feedback into the original hypothesis. Starting with a notion of rational behavior would have led to observations of how traditional political decision making focuses only on approach—usually ideological—and on action. If there is failure, it does not call for reexamination of any basic hypothesis of theory but is attributed to lack of sufficient action, and the remedy is more of the same. This truly traditional notion of what is “rational” would have demonstrated how nonrational most decision making is.

The “abductive” thinking of C. S. Peirce could be a basis (see D. H. Mellor’s *Science, Belief, and Behavior* [1980]). Some such theoretical framework from which to deduce “irrational” behavior, making use of cases as examples only, would have led to a richer analysis and explanation of the phenomenon being exposed.

The book presents a challenge. Here is a topic that deserves more study.

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Arms Control and Nuclear Weapons: U.S. Policies and the National Interest. Edited by W. Gary Nichols and Milton Boykin. New York: Greenwood, 1987. 136p. \$29.95.

This provocative slender volume offers readers a lively debate directed toward creating an informed public discussion that will center attention on what is clearly the critical issue of our times: how to survive the nuclear age.

In his introductory chapter, General George Seignious sets the pace by making it clear just what the stakes are. He cites a speech given by General Omar Bradley more than 30 years ago in which Bradley said, "If we are going to save ourselves from the instruments of our own intellect, we had soon better get ourselves under control and begin to make the world safe for living." Arms control is serious business. Among the lives we save will be our own.

Seignious stresses the fact that SALT I and SALT II have to be understood in their own terms, namely that the offensive systems of the United States and the Soviet Union were asymmetrical in makeup and in strength. The Anti-Ballistic Missile Treaty forced both nations into an acceptance of vulnerability and thus put into place the concept of mutually assured destruction of cities and populations, and that is still the case.

Kenneth Adelman, former director of the U.S. Arms Control and Disarmament Agency, discusses the Strategic Defense Initiative (SDI). He argues that SDI offers new opportunities to get out from under the balance of terror. He sees it as an option that just might be better than holding populations hostage to nuclear death while piling on new weapons. He makes the point that SDI is focused on a nonnuclear defense against *nuclear weapons* and argues that it would save the world from a "fail-safe" scenario under which a president might have to ride out the destruction of a major city while waiting to see if it were the result of a deliberate strike or rather a dreadful mistake or miscalculation. To the charge that SDI would be an immoral program, he comments that reducing the risk of war can hardly be against morality.

This point of view is directly challenged by Molly Ravenel, a former member of the National Governing Board of Common Cause.

She believes that common sense should take us in another direction and that the SDI program would create uncertainty on the part of the Russians and tempt them into a preemptive strike. What President Reagan calls Mutually Secured Security is possibly just the reverse. Citizens should not be cowed into silence because they are not scientists. The concept of SDI is not incomprehensible, nor are its risks. The public should be alert to the loopholes in the language of strategy.

Boykin in his critique of U.S.-Soviet arms control negotiations argues that there are no sensible military uses for nuclear weapons, that they are political weapons and that Clausewitz' concept that war is but an extension of politics by other means has to be taken with some caution. War might be fatal to all. He cited the argument of Carl Sagan that war could be suicide in terms of the nuclear winter theory. Boykin makes the case for nuclear morality based upon the simple principle that life is good and should be preserved for all.

Ambassador Gartoff examines in his essay the nuclear weapons policy of the Soviets, going back to their attempts in 1940 to make an atomic bomb. He argues that contrary to the thinking of many U.S. citizens, the Russians were never very far behind and that that remains true today. Soviet policy has evolved, and arms control is not a thing in itself but rather an instrument of policy. He feels that SDI is destructive to this approach, since it forces the Soviets to emphasize offensive actions.

Jack Perry tries to get the reader to think beyond the simple East-West dialog. The superpowers are not the only powers. There are the U.S. allies, the Soviet allies, and the rest of the world. Their interests affect the nuclear challenge, and they raise the issue of just whose security is at risk.

Although the book was published in 1987, these essays were written in 1985—two years before Gorbachev came to Washington and the framing of the Intermediate Nuclear Forces agreement. Yet they all retain their relevancy. No longer can scientists be separated from citizens, nor can the concerns of the military ignore the moral messages of religious leaders. If there is a central message to this book, it is that we are all in this together. In the words of the late President Lyndon Johnson, "We must

reason together to make things come out all right."

MILTON COLVIN

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Peace in Parts: Integration and Conflict in Regional Organization. By Joseph S. Nye, Jr. Lanham, MD: University Press of America, 1971. 1987 Reprint. 210p. \$22.50 cloth, \$9.75 paper.

During the 1960s and early 1970s, the study of regional integration occupied a prominent place in international relations theory. The European Economic Community had clearly transformed economic and political relations in the traditional heartland of world politics; and other regional communities were being established in both developed and developing areas for a wide variety of purposes. Beyond its unquestioned empirical importance, furthermore, regional integration was generally considered "good" from a normative perspective. Thus, regional communities are assumed to make an important contribution to the economic growth and well-being of their members and to the control and eradication of political and military conflicts within the region.

Joseph Nye's *Peace in Parts* (1971) represented a major scholarly contribution to the intellectual debates about the nature and consequences of regional integration on several fronts. It presented perhaps the most incisive yet comprehensive model of how the integrative processes proceed and expand from narrower economic to broader political issues and institutions and also discussed the development of indexes of the key concepts in this theory with some imagination. Nye also made the felicitous distinction between "economic microregions" and "political macroregions" and showed with a good mix (or "integration") of theory and case study that they had somewhat different effects upon political and economic outcomes. Thematically, the book was organized around five major hypotheses about how regional integration could promote peace in the contemporary world. Nye concluded that while integration had little impact upon how people related to their state, several regional communities had proved significant for the

conduct of interstate relations, thus making a modest contribution to the creation of "islands of peace" in a conflictual world. This analysis incidentally reflects another striking characteristic of Nye's work. Unlike the quite optimistic conclusions that dominated the study of integration, Nye's models indicated the "limits of integration" with what turned out to be admirable foresight.

Peace in Parts, therefore, is clearly an important and valuable work of scholarship. It reflected and synthesized a decade of theoretical and empirical work in what was then a central concern of international relations theorists, but it also looked ahead to the general stagnation and failure of regional communities. This decline of regionalism was multi-causal. State sovereignty and nationalism proved much more intractable at both the elite and mass levels than scholars anticipated. The transportation and communications revolutions rapidly created a global economic interdependence that made most regional cooperative schemes obsolete. The world economic volatility of the 1970s and 1980s greatly increased competition for sharing the burdens of a contracting "pie." Regional political conflicts became increasingly entangled in superpower rivalry as both the United States and USSR tried to play "global policemen." As a result, regional communities, especially in the third world, lost most of their ability to promote economic growth and reduce political conflict, the Central American Common Market being perhaps the most miserable and spectacular failure, thereby sending international relations scholars scurrying to find more profitable paths of inquiry.

Given this decline and near demise of integration theory, one may certainly question the reissuance of *Peace in Parts*. In a brief introduction, Nye linked regional integration theory to the development of the "liberal"—as opposed to the "realist"—perspective on international affairs. Thus *Peace in Parts*, as one of the best representatives of the analysis of regional integration, can be viewed as having played a significant role in the intellectual history of an important strand of international relations theory that went on to analyze global interdependence and international economic regimes and to ask what will happen to the global political economy "after hegemony."

In fact, the declining hegemony of the

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United States in the international economy and the seeming willingness of the two superpowers to curb their global competition suggest that regionalism may again become relevant but in a somewhat different way from that visualized by integration theorists of 15 years ago. In the economic sphere several regions face similar problems in the global economy (e.g., East Asia's need to combat rising Western protectionism, the debt crisis in Latin America, political reform and technological upgrading in the Soviet bloc, responding to the industrial competitiveness of newly industrialized countries in Western Europe and North America, and so forth). Thus, the coming decade may witness a trend toward regional negotiations over international economic regimes rather than the revitalization of common markets. Politically, growing similarity of economic interests may make regional members more sensitive to the costs of conflicts and to the fact that superpower interventions usually exacerbate rather than solve crises. Thus there is at least some chance that primarily regional negotiations, whether they involve formal bodies or not, may again work along the "modest" lines depicted by Nye's analysis as the international system increasingly moves into a phase of the "deconcentration" of political and economic power.

CAL CLARK

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The Foreign Relations of North Korea: New Perspectives. Edited by Jae-kyu Park, Byung-chul Koh, and Tae-hwan Kwak. Boulder, CO: Westview, 1987. 491p. \$39.50.

How is foreign policy made in North Korea? How have North Korea's foreign relations evolved in the past 40 years? What are military and economic capabilities and constraints in the conduct of foreign policy in North Korea? And how can we best characterize North Korea's contemporary diplomatic relations with other countries, including the U.S. and the Soviet Union? These are some of the questions dealt with in this collection of 17 articles by 16 authors. The articles vary in style, approach, and emphasis; but all the authors appear to have attempted to highlight their

research findings based on a variety of most up-to-date, and often hard-to-find, source materials. This volume offers an excellent insight into the study of foreign relations of North Korea, one of the most secretive countries in the world today.

The book, designed to provide a "theoretical analysis," is divided into five parts: an overview of North Korea's foreign policy making; "domestic sources" of foreign policy; and North Korea's relations with Communist countries, Western Europe and Japan, and South Korea and the rest of the world. In part 1, Jae-kyu Park outlines the "fundamental premise" and goals of North Korea's foreign policy. Unification is considered as the "ultimate goal." In Byung-joon Ahn's view, however, such consistent goals have accompanied changing policies and strategies in response to changing internal and external situations. Byung-chul Koh, in an attempt to explain the foreign policy-making process, devotes much of his paper to analyzing three factors he developed: operational environment, psychological environment, and the structure and routines of decision making.

Part 2 consists of articles on ideological, military, and economic capabilities and constraints relevant to North Korea's conduct of foreign policy. Han S. Park offers a view that Kim Il-sung's doctrine of *juche* (self-reliance) will become a source of constraints in North Korea's foreign policy implementation since it would not afford flexibility in adopting the rigidly controlled regime to changing situations. The article by Jong-chun Baek presents useful data on North Korea's military strengths, compared with South Korea. The three "basic" military capabilities North Korea has developed over the years are its capabilities as a revolutionary base, revolutionary capabilities in South Korea, and international revolutionary capabilities. And Joseph Chung deals with North Korea's record of economic development, focusing on the various economic plans and pointing out specific reasons for the relatively poor economic performance record in comparison with South Korea in recent years.

Part 3 is an analysis of North Korea's relations with the Soviet Union, People's Republic of China, and Eastern European countries. Joseph Ha and Linda Jensen contend that a unified Korea under a Communist regime,

even under Kim Il-sung, is not what Moscow really wants. Chin-wee Chung reviews North Korea's foreign policy in the context of the Sino-Soviet dispute and concludes that Pyung-yang's relations with Beijing—compared to relations with Moscow—are likely to remain friendly because of historical and cultural similarities between the two. North Korea's relations with Eastern European countries have been on good terms since 1948, according to In-young Chung, who also highlights policy implications of Kim Il-sung's recent visits to the Soviet satellites.

Parts 4 and 5 deal with North Korea's diplomatic relations with the United States (by Ralph N. Clough), Japan (Jung-hyun Shin), Western Europe (Gregory F. T. Winn), South Korea (Tae-hwan Kwak), Southeast Asia (Kook-chin Kim), Middle East (Chung-in Moon), Latin America (Man-woo Lee), and Africa (Jae-kyu Park). According to Clough, North Korea has failed to obtain what it wanted from the United States, due to its "harsh tactics" and underdeveloped diplomacy. Shin does not see any drastic change in North Korea's hostile relations with Japan in the foreseeable future although both countries might try to improve relations. In spite of recent commercial agreements, Winn concludes that prospects for North Korea's relations with Western Europe remain not very encouraging. Kwak, after identifying fundamental differences between North Korea and South Korea in their unification policies, offers a "cautiously optimistic" view on the future of the inter-Korean dialog. Southeast Asia has been a crucial diplomatic target for North Korea, according to Kim, but in recent years North Korea has lost its edge to South Korea, particularly among Alliance of Southeast Asian Nations. Moon's excellent study shows that North Korea's influence over some parts of the Middle East is also on the wane. However, as Lee and Park suggest, North Korea, compared with South Korea, has done a rather impressive job in establishing diplomatic relations in Latin America and Africa.

This volume is a collection of research conducted mostly, if not exclusively, from South Korea's vantage point, with ample room for further debate on the objectivity of the research findings and interpretations. While the organization of the topics is excellent, readers of the book will find several articles with vir-

tually the same or similar historical or chronological accounts of events. On balance, however, this book covers a wide range of topics relevant to North Korea's foreign relations, and the editors attempt to cover the Communist state's contacts with virtually all the regions. The editors are to be commended for their efforts. Readers of this volume will find the bibliography section particularly useful in the study of North Korea's foreign relations. The volume was not designed to be a textbook; it may be used as a reader in international relations or comparative government courses for both undergraduate and graduate students. This timely book is certainly a welcome addition to the much-needed literature on the communist regime on the Korean peninsula.

KEON-S. CHI

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Turkey: America's Forgotten Ally. By Dankwart A. Rustow. New York: Council on Foreign Relations, 1987. 155p. \$14.95.

Because of the rapid transformation Turkish society has undergone during recent years, books that would be current in more gradually changing contexts become outdated when the object of study is Turkey. If one were to read a book on Turkish politics written five years ago, for example, one would have little idea what the Turkish political party system looks like today. Similarly, in terms of foreign policy one would find it difficult to believe that a country described five years ago as underdeveloped, unstable, and authoritarian felt developed, stable, and democratic enough to apply for full membership to the European Community in 1987. Rustow's book is a timely publication, coming at a moment when there is a notable paucity of writing on contemporary Turkish society, politics, and economics.

Benefiting from his extensive familiarity with Turkey, Rustow has adopted a historical perspective. In this way, the description of today is rendered more comprehensible by relating it to past developments. The book may best be described as a brief study of socioeconomic and political change in Turkey. As a study of change, it may be of interest to students of change in other environments.

The book is brief, and it appears to have

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been written not for consumption in the academic community but for readers interested in Turkey for reasons (such as business), who would like to get a summary description of social, economic, and political life in Turkey. This approach has its advantages: the book is easy reading, and it draws a comprehensive but elegant picture of change in a country. It does bring with it, however, a number of problems. Because a theoretical concern is lacking, the relevance of the work to specialists who do their research in other parts of the world is severely curtailed. Similarly, because the analysis is brief, the reader may be left wanting to know more about a number of assertions or generalizations offered in the book. A case in point is the explanation that the tradition of secularism in Turkey and the differences of religious sect between Iran and Turkey make it most unlikely that an Iranian-type fundamentalism may carry the day in Turkey. This is a judgment that I personally share, but many readers might want to know in greater detail the nature of Turkish secularism and the particular version of Sunni Islam professed in Turkey and how it differs from Iranian Shiism in its ideology, practice, and organization, before they accept Rustow's analysis.

The book is organized around five major themes, which also constitute five of the six chapters in the book. The first chapter is an introduction. In the subsequent five chapters the westernization of Turkey, the change in Turkish economic policies, the nature of Turkish democracy and its evolution, Turkish-U.S. relations, and the major features of Turkish foreign policy are examined. There is, at the end of the book, a list of other sources the interested reader might consult on the topics taken up in each chapter. This list is short and not particularly comprehensive.

We have here an interesting book for the lay reader, and the student of socioeconomic and political change. The expert on Turkey may find the book too general, but those interested in the study of change may discover here a case they would like to learn more about.

ILTER TURAN

Istanbul University

The UN Commission on Human Rights. By Howard Tolley, Jr. Boulder: Westview, 1987. 300p. \$29.95.

Howard Tolley, Jr. of the University of Cincinnati has written an interesting and useful monograph describing the history of the United Nations Commission on Human Rights from its founding in 1946 until 1986. He adds an afterword on the commission's 1987 activities. He also asks two important questions. First, can a policy-making institution responsible for drafting international norms be successful at enforcing those norms? And second, can a policy-making institution composed of governments provide impartial and effective protection to citizens of those governments? The answer to both questions, of course, is *no*, and Tolley documents the answer in appropriate detail.

He begins by describing some of the forerunners of the commission, including such conceptual ancestors as the U.S. Bill of Rights, the nineteenth century Geneva conventions, and the Mandates Commission of the League of Nations. From these historical antecedents and the World War II holocaust-inspired vision that something had to be done to protect human rights came the institution of the UN Commission on Human Rights, founded in 1946 under the aegis of the United Nations Economic and Social Council.

The commission's first great success was the Universal Declaration of Human Rights, approved soon after its founding. This declaration set a standard and tone that the commission subsequently found difficult to live up to, as it was forbidden by its parent council to take any enforcement action against complaints of human rights violations. After all, the UN itself was forbidden by its charter to interfere in the domestic affairs of any of its members, and the prevailing view in the early days of the organization was that a government's treatment of its own citizens was essentially a domestic matter and not a threat to the peace. So the early years of the commission were spent principally in research, in the setting of standards of governmental conduct, and in promoting the idea of human rights.

After the commission moved into the 1960s, its membership eventually increased from 18 to its present 43, and control shifted from the hands of a Western-oriented majority to a

third-world majority with a much different agenda. Resolutions and activities of the commission were politicized by the East-West and the North-South conflicts, and the focus moved to anticolonialism and antiracism, largely in South Africa and Israel.

In 13 years of debate, Tolley writes, only three regimes—South Africa, Israel, and Chile—were actually singled out for special concern. Tolley describes well the double standard used by the commission in the 1960s and 1970s, as third-world countries refused to denounce the human rights violations of their own governments, and the so-called Western values of freedom of speech, press, religion, and assembly were given low priority.

But the author concludes on a more optimistic note, suggesting that a better balance now exists in the commission and that a "tenuous balance . . . has created a fragile vitality which survived the deadening rhetoric" (p. 219). Tolley adds, "Equally important, the Commission has cracked the citadel of national sovereignty first by drafting standards and then by eliciting government responses to complaints about their misconduct" (p. 220).

Clearly, the UN Commission on Human Rights serves in many ways as a microcosm of the United Nations itself. Like the latter, the commission was formed to correct the perceived causes of World War II, controlled in its early years by a pro-Western majority, dominated in more recent years by a third-world majority intent on a much-different agenda, politicized by conflicts that had little to do with the main purposes of the institution, and plagued by institutional and procedural problems which have tended to reduce its value to informed publics.

Tolley's book is a valuable addition to the literature on the United Nations and its subsidiary organs and stands alone as a current history of the Commission on Human Rights. If the book's style is not exciting, the story it tells is, and the conclusions are balanced and carefully drawn. The documentation, bibliography, and index are excellent. This monograph will be important reading in the next few years for scholars who are working in the field of international organization or human rights.

THOMAS A. SARGENT

Ball State University

From Brezhnev to Gorbachev: Domestic Affairs and Foreign Policy. Edited by Hans-Joachim Veen. New York: St. Martin's, 1987. 378p. \$45.00.

The volume under consideration is a revised edition of the proceedings of a joint German-U.S. conference on post-Brezhnev Soviet domestic politics and foreign policy held in 1984. It is broken into three sections, dealing with Soviet economy, culture and society, and the political arena. The purpose of the book is twofold: to enhance German-U.S. cooperation in the field and to deepen our understanding of the nature of, and prospects for, political change in the USSR.

It has several strengths. Most significant is the role it plays in bringing to U.S. attention the depth and breadth of West German scholarship on the Soviet Union. The offerings by Karl-Eugen Wadekin on agriculture, Jochen Bethkenhagen on energy, Klaus Schroder on debt, and Christian Schmidt-Hauer on leadership transition, among others, reflect a level of dispassionate and careful scholarship all too often absent from debate on the USSR in this country.

Although in this sense, the work makes an important contribution, it is flawed in a number of ways. First, there is no sustained effort to draw together the various strands of the analysis into a synthetic whole. A lot of ground is covered without ever really getting anywhere. By the end of the section on energy, for example, one has read that the "energy situation offers very favorable prospects for Soviet leaders" (p. 53) and that "energy will present much more in the way of problems, much more in the way of investment demands, much more in the way of technological requirements that will burden the economy than it will present in terms of opportunities for economic growth and expansion of trade" (p. 57). One expects differences of interpretation in a volume of this type, but such fundamental disagreement is disorienting to the non-specialist.

Moreover, there does not appear to have been any real attempt at quality control. Although some of the contributions are well-documented and amply developed, others are brief, undocumented, and superficial. Perhaps more seriously, in some cases constructive scholarship gives way to crass politically

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motivated distortion. Richard Staar's incomplete and inaccurate characterization of the activities of the Pugwash movement, for example, is insufficient to justify his dismissal of it as a "sounding board for anti-American and pro-Soviet propaganda" (p. 240). He leaves the impression that anyone believing that dialogue with the USSR may be useful is a dupe. His further remark, with regard to the U.S. campus protest movement of the 1960s, that "the meaning of words like peace or academic freedom and the 'people' in reality signified coercion and the will of a small minority that used any available means to introduce anarchy" suggests that he is more concerned to vent his spleen than to engage in serious discussion of Soviet politics (p. 250).

Finally, in many instances analysis stops in 1984, the date of the conference on which the volume is based. Given the character of the intervening three years, systematic efforts to update research results would have been prudent. It is odd in a work published in 1987 to run into an analysis of the process of economic reform that makes no mention whatever of the Gorbachev era. Although some of the more substantial contributions dealing with the economic structure and with the political sociology of the USSR (e.g., Meissner's contribution on social change and its impact on the social structure of the party) have lasting value, much of the work is dated and of little utility to the student of current politics in the USSR.

The work shows this weakness with a number of earlier collections, such as Bialer's *Domestic Context of Soviet Foreign Policy* (1981) or Byrnes' *After Brezhnev* (1983). All suffer by comparison to the most recent edition of Colton's *Dilemma of Reform in the Soviet Union* (1984), which is as close as one currently gets to a definitive treatment of the subject.

S. NEIL MACFARLANE

University of Virginia

Security in the Middle East: Regional Change and Great Power Strategies. Edited by Samuel F. Wells, Jr., and Mark A. Bruzonsky. Boulder: Westview, 1987. 366p. \$38.50 cloth, \$15.95 paper.

The Middle East in Global Strategy. Edited by Aurel Braun. Boulder: Westview, 1987. 274p. \$31.50.

The Persian Gulf and the West: The Dilemmas of Security. By Charles A. Kupchan. Boston: Allen & Unwin, 1987. 254p. \$39.95 cloth, \$14.95 paper.

It is clear that at a time when even such practitioners as former secretary of defense Caspar Weinberger (speech to the Comstock Club, Sacramento, 19 July 1985) reveal a considerable measure of sophistication in relating the efficacy of resort to military force to the prevailing political and social contexts, scholarly works on strategy may not remain parochial. In fact all three books under review are aware that strategy in the Middle East cannot be fruitfully analyzed in terms of its military dimension alone and consequently devote most of their contributions to a consideration of the relevant political and social contexts.

In view of the fairly large volume of scholarly output currently available on different aspects of the subject, the more recent publications should be subject to higher standards than in the past, when little was known. I have used three primary criteria to gauge the quality of each contribution. The first criterion consists of theoretical sophistication, that is, the degree to which the authors are aware of their own theoretical frameworks and operative assumptions and ultimately make a contribution to the refinement and further advancement of the available theory. The second consists of methodological rigor and objectivity. This is particularly significant in the case of contributions on the Middle East, which must inevitably deal with such conflicts as those between the United States and the Soviet Union, the Arabs and Israel, Iran and Iraq, and nationalism and religion, each of which has the potential to evoke a considerable amount of partisanship and advocacy even under the rubric of social science. The third consists of the level of contribution to policy-making and implementation, that is, the degree to which the authors identify security objectives, relate such objectives to other objectives that are

engaged and to the available means, generate well-conceived options in terms of cost-benefit assessment and trends in the situational contexts, and so on.

Security in The Middle East: Regional Change and Great Power Strategies consists of 16 papers, most of which were originally presented to a seminar held at the Woodrow Wilson Center for International Scholars from 1981 to 1983 and subsequently modified in light of fast-moving events in the Middle East, particularly developments in Lebanon. Despite variation in the quality of the chapters, the collection as a whole rates rather high in terms of objectivity and contribution to policy-making. It is weaker in the area of theory. In fact, theory is not a strong suit in any of the three books.

In the introduction, Samuel Wells, Jr. and Mark Bruzonsky provide a fairly comprehensive outline of the questions related to internal dynamics of the regional states and their relations with the West. While such issues as the role of religion in the area and internal political dynamics of the United States itself may have received more attention, the editors have made a deliberate, and to a great extent successful, effort to overcome parochialism and set a tone of objectivity that has paid dividends in the overall quality of the volume.

Part 1, "Stability and Change within the Region," is composed of seven quite competent but at times somewhat thin chapters on Syria by John F. Devlin, Lebanon by Augustus Richard Norton, Israel by Bernard Reich, Egypt by Ali Hillaal Dessouki, Saudi Arabia by Adeed Dawisha, Iraq by Christine Moss Helms, and Iran by Robert Litwak. Since the authors are as a rule political scientists, they are subject to higher theoretical standards than other contributors—historians, lawyers, journalists, and government officials. Consequently, one could have expected greater analysis, less description, and more concern with clarification of such concepts as stability, which has tended to be confused with the status quo. This tendency has in turn led to prescriptions for failure, because in the long run change cannot be stopped altogether, even by mightier forces than those at the disposal of the United States.

Part 2 deals with the Palestinian problem. It consists of three thoughtful essays that make serious contributions to problem solving and

collectively constitute the most distinguished part of this volume: Herbert Kelman's "Creating the Conditions for Israeli-Palestinian Negotiations," Simha Flapan's "The Long Search for Peace: A Progressive Israeli View," and Bruce Kuniholm's "The Palestinian Problem and U.S. Policy." Kelman brings in a psychological dimension that is badly needed but usually missing in treatments of U.S. relations with the Middle East. Unfortunately, there is no Palestinian or Arab scholar represented in this section. Given this fact and the prevailing biases of U.S. policies in the last four decades, which, intended or not, have substantially contributed to the plight of the Palestinians, one may conclude that there is nothing new here. But there is something new. Flapan, a former director of the Arab department of Mapam, bends over backward to present the other sides' arguments as well as his own before arriving at his conclusions. In fact, concern for objectivity and the nature of operating assumptions also characterize the essays by Kelman and Kuniholm. What is missing in this part is a separate chapter of equal objectivity on Soviet policies toward the Palestinians and the potential Soviet role in achieving peace.

Part 3 consists of five essays of uneven quality: "Middle East Oil and Industrial Democracies" by Robert Lieber, "Soviet Decisionmaking for the Middle East" by Dennis Ross, "Soviet Policy in the Middle East" by Shahram Chubin, "American Diplomacy and Arab-Israeli-Palestinian Peace since 1967" by Harold Saunders, and "A Political/Military Strategy for the Persian Gulf and Southwest Asia" by Kuniholm. Although each chapter has something useful to say, insufficient concern with theory and methodological rigor has taken its toll. Lieber, for example, takes issue with the West European view that settling the Palestinian conflict would improve the Western position, asserting that "such a peace could unleash even more divisive tendencies than currently exist. In this regard the observation of Fouad Ajami is worth quoting at length. He judges that a major Middle East peace initiative by the United States would greatly inflame the passions of the extremists" (p. 230). Yet the quote that follows does not sustain such an assertion (p. 231). Instead, it refers to other sources of "terror" as well. But surely this is not a major discovery, and referring to other sources of terror does not make the impasse on the Pales-

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tinian issue irrelevant or peace a greater source of extremism and passion, unless sustainable evidence and analysis to this effect are presented, which they are not. To cite another example, Chubin states, "How far Iran will be permitted to evolve in its own way and at its own pace will be an indicator of Soviet intentions." Why would this be an indicator of Soviet intentions but not of Soviet capabilities? As the literature of decision making clearly indicates, objectives can be meaningfully considered only in terms of the relevant available means and cost-benefit assessment. It would not have been helpful to the Afghan Mujahedin, for instance, to be persuaded by such arguments as those of Chubin because there is little doubt about the nature of the Soviet intentions in their case. But it would have been helpful if this essay had taken the evidence from Afghanistan—as well as that from Vietnam and Lebanon—to examine its operative assumptions. Furthermore, it should be noted that the Soviets are unable to achieve their objectives in Afghanistan despite the existence of a friendly and obliging government, something which does not exist in Iran. In any case, the Iranian leaders do not seem to be as concerned as Chubin about Soviet permission when it comes to their initiating and implementing of policies. On the whole, a little more social science and less journalism would have helped most of these substantive essays. Kuni-holm, clearly well-versed in policy analysis, verifies this proposition by producing the outstanding essay of this part. It demonstrates the ability of historians and area specialists to produce rigorous contributions that are of utility to both social scientists and decision makers.

The Middle East in Global Strategy begins with an introduction by the editor, Aurel Braun, which refers to the works of Clausewitz and André Beaufré but does not refer to more recent developments in strategic thinking and decision-making theory. Despite the stated fondness for Beaufré, the essays dealing with Israel, the Arabs, and the United States, particularly those in parts 1 and 2, do not as a rule provide an objective assessment of "political, economic, diplomatic, psychological, and military dimensions" (p. 6). They accord somewhat more readily to the statement that "strategy for Beaufré is an art rather than a science because it cannot have clearly determined rules" (p. 7).

The problem is not so much that these contributions to illuminating conditions of the Arab states and Iran and assessing the United States' national interest include Israelis—with teaching background in Haifa, Hebrew, Bar-Ilan and Ben-Gurion universities—and some committed pro-Israeli Canadian and U.S. scholars but no Arab or Iranian authors. Rather, the problem stems from the effect of shifting paradigms and standards, exacerbated by the absence of the countervailing influence of even such Israeli scholars as Flapan. Meir Zamir's "The Emergence of Syria?" for example, is quite correct in relating foreign and domestic policies and pointing out the problems posed for that country's external capabilities by its potential domestic instability. But the problems posed for the legitimacy, hence stability, of the pro-Western Arab regimes caused by the behavior of Israel and policies of the United States, undertaken in response to the pressures of domestic pro-Israeli groups, have received little attention. Yet it should be clear that if these regimes fall before the onslaught of nationalism, religion, or both, which have also been neglected or underemphasized despite their significance, the United States is likely to lose leverage in the Middle East; and when the United States loses, Israel probably does too.

In the same vein, Gerald Steinberg and Steven Spiegel's "Israel and the Security of the West" is a competent statement of Israel's advantages but not a serious discussion of its disadvantages for the Western countries. The most interesting case of shifting standards is represented in "Strategy and Politics: A U.S. Perception" by Howard Teicher, senior director for political-military affairs in the National Security Council. He argues that "Americans, as a moral people, want U.S. foreign policy to reflect the values that the nation espouses. But Americans, as a pragmatic people, also want an effective policy." Foreign policy is not made by the U.S. people, however, but by a few (mainly the president and his advisers and members of Congress, mostly the Senate) who make policies in the name of the people and not always according to the latter's values or benefit.

In reference to Reagan's initiative of 1 September 1982, Teicher maintains that the U.S. positions are (1) idealistic and moral, (2) pragmatic, and (3) realistic. Since these concepts

are not systematically related to each other, they provide a perfect pretext for justifying one's own positions and condemning the alternative positions by shifting from standard to standard. When some people point out the immorality or illegality of our position, we will accuse them of not being realistic; and when we are not realistic in the pursuit of agreed-upon criteria of the national interest, we argue that we are moral. At the same time, we can use the opposite attributions for Arab or Soviet policies with which we disagree. When they are realistic, we brand them as immoral; when they are moral, we brand them as unrealistic, and so on.

It may be helpful to refer to some actual policies in this connection. The United States is supposed to adhere to a policy of nuclear non proliferation. When assistance funds to a country such as Pakistan—which supports our policies toward Afghanistan—are considered, Congress indicates great concern with non-proliferation policy, threatening to cut, or actually cutting, funds because of Pakistani potential in this area. But it has shown no interest in ever discussing the issue of a full-fledged Israeli nuclear arsenal and has contributed to its construction by substantially adding to the already generous funds usually requested for Israel by the executive. Many of the estimated 850–900 million Muslims perceive this as yet another sign of U.S. hypocrisy, as well as of a plan to make 3.5 million Israeli Jews dominant not only over 120 million Arabs but also over the entire Muslim world. Furthermore, eventually the Arabs and Muslims are also likely to acquire nuclear weapons and missiles, and it is not clear that such development would benefit anyone, especially Israel. When the U.S. government deals with the Soviet Union, it emphasizes human rights; when it deals with the Palestinians in the occupied territories it emphasizes that Israel's security, even over the annexed areas, overrides the right of the dispossessed. Another case is Reagan's initial support for Israel's right to all of Jerusalem and disagreement with Carter's position that Israel's settlements in the West Bank were illegal, hence strengthening the hands of those in Israel who have been opposed to some of the Camp David provisions. I do not know whether these behavioral patterns by Congress and the president would be regarded as "moral," "realistic,"

or both by Teicher. But the possibility that they may be neither certainly deserves some consideration, particularly in light of what our policy of simultaneously guaranteeing Israel's military superiority over its Arab neighbors and insisting that the parties to the conflict must find a mutually acceptable solution has produced for the Palestinians.

The sections on external influences consists of three essays on superpowers and three on alliances. Sanford Lakoff's "Power and Limits: U.S. Strategic Doctrine in the Middle East," provides a useful historical summary and raises meaningful questions about the limits and potential utility of resort to force. S. N. MacFarlane's "The Middle East in Soviet Strategy" is a fairly sophisticated examination of the elements of Soviet foreign policy and of the potential Soviet role in the resolution of the Arab-Israeli conflict. Edwin Fedder's "NATO and the Middle East" is the most distinguished of the essays in this part because of its objectivity, its ability to overcome parochialism, and its exposition of the sources of divergence between European and indeed even of Arab states' policies and those of the U.S. The other essays in this part consist of "The Superpowers and the Middle East: The Maritime Dimension" by Joel Sokolsky, "Alliance Politics in the Middle East: A Security Dilemma Perspective" by Avner Yaniv, and "The Warsaw Pact and the Middle East" by Braun.

Part 4 deals with the gulf. Roger Savory's "Geopolitical Impact of the Islamic Revolution in Iran on the Gulf Region" is not restrained in its castigation of either the Islamic Republic or its major perceived cause for success, Carter's administration. The "vital interests" of the United States in the gulf region, "which are now geopolitical rather than economic," consist of four items of which the first is "support for Israel." As to the resolution of the problems that this may cause for another admitted interest, "the security of the 'friendly' Arab states in the area," he readily provides it by ignoring the problems. This is followed by Litwak's "The Soviet Union and the Gulf Area." Zalmay Khalilzad's "U.S. Strategic Concerns: Deterrence Dilemmas in the Gulf Region" is the last and probably the best essay in this collection. He is quite aware that "concepts and plausibilities" are no substitute for "empirical evidence" and displays an understanding of recent developments in strategic theory, particularly of the

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writings of Thomas Schelling, Robert Jervis, and Alexander George. It would have been of considerable help to the general quality of this book if these developments had been incorporated in the introduction and guided the relevant contributions. Khalilzad also presents a provocative application of "the balance of interest" to the gulf and distinguishes the northern tier from Saudi Arabia and smaller oil-producing states in terms of the strategic requirements for mutually assured destruction (MAD). I, however, find it more fruitful and in touch with the empirical world to rely primarily on the Afghans and Iranians, perhaps with assistance from the West, to frustrate any Soviet conventional thrust into their lands, rather than delving headlong into speculative consideration of the incredible world of MAD in the context of the gulf.

Being a single-author book, Kupchan's *The Persian Gulf and the West* is considerably more integrated and focused than the other two volumes. Its basic purpose is to use the Persian Gulf as one particular area of the periphery "as a means of delving into debate about the Western security" and developing policy prescriptions. The introduction stakes out three basic security dilemmas for the United States in the gulf arising from "strategy vs. capability, globalism vs. regionalism, and unilateralism vs. collectivism" (p. 4).

Chapters 2-5 are primarily historical: "The Setting, 1946-73," "The October War and Its Aftermath," "Iran, Afghanistan and the Evolution of the Carter Doctrine," and "Rapid Deployment Force: Planning, Strategy, Operational Requirements." They are generally quite competent and sufficiently analytical to make them interesting to political scientists. Chapter 6, "U.S. Power in the Gulf: Military Strategy and Regional Politics," breaks some new ground. It addresses the difficulty of reconciling the requirements of different U.S. objectives, points out the "gap between strategy and political feasibility" stemming from some "key political assumptions," and concludes that the "task of linking military strategy to political reality . . . was less affected by bureaucratic dynamics and electoral concerns than was the process of policy formulation itself" (pp. 157-8).

Chapters 7 and 8 deal with relations between the United States and NATO. Both are well informed and analytical. The political problems,

which constituted the main reason for hindering "the ability of the alliance to address security issues in the Middle East between 1950 and 1980," still remain important for the sustainable but somewhat overdrawn conclusion that "NATO as presently constituted is unprepared and unable to address adequately regional threats to Western security" (p. 208). An additional factor, which has not received adequate attention, is the seemingly long-term structural trends in the United States' comparative capability in the economic realm. If not halted, it will require difficult decisions in terms of priorities and allocation of resources. Whether such decisions can be made sooner—rather than later and at greater costs—is highly debatable, given the internal political system of the United States. But one thing is clear. Western Europe will have to assume a greater role—and, concomitantly, a greater voice—in determining and providing for its own security. It is in this light that chapter 9, "The Dilemmas of Persian Gulf Security," becomes particularly interesting and useful to both social scientists and policymakers.

Each of the three books reviewed makes some useful contribution to the subject of Western security. Future works may find it helpful to be more concerned with the adverse effects of reification of states and place greater emphasis on internal political dynamics not only of the regional states but also of the United States and Israel. Further, it may be worthwhile to place greater emphasis on long-term structural trends and their implications. This is particularly important for guarding against intellectual lag in coming to grips with the implications of change and against using frames of reference that are dysfunctional. Finally, analysts may find it rewarding to go out of their way to seek the views of those with whom they disagree, because, as Jervis noted long ago, "scholars and decision-makers are apt to err by being too wedded to the established view and too closed to new information, as opposed to being too willing to alter their theories."

G. HOSSEIN RAZI

University of Houston

The Manner of Giving: Strategic Aid and Japanese Foreign Policy. By Dennis T. Yasutomo. Lexington, MA: D. C. Heath, 1986. 147p. \$22.00.

Japan stands figuratively as a "nexus" in international society, the place where East-West and North-South relations come together. In the coming years—indeed, through the next century—Japan will bear an incalculably heavy responsibility encompassing not only politics and economics but also all aspects of human existence. Today, Japan is the number two nation in the amount of foreign aid it provides to third world nations, and foreign aid is one of her major foreign policies. This book makes a significant contribution to the literature about Japan's current international roles and foreign aid activities.

Foreign aid is the expression most frequently used to describe the flow of financial and technical resources from the developed world to the underdeveloped world. The understanding of foreign aid processes, problems, and policies is difficult even for the most sophisticated observer because there are so many facets to economic development, so many conflicting viewpoints. The whole foreign aid complex is intricately enmeshed in the fabric of national interests and foreign policies. The author untangles Japanese foreign aid policy, providing insights into its motivations and effects.

The author's central message is stated in the five sets of questions presented at the beginning of the book (p. 6). What role does domestic politics play in shaping strategic uses of aid? What is the exact relationship between economic assistance and defense policy? What effect will globalization have on Japan's traditionally regional aid focus (the association of southeast asian nations [ASEAN])? Would Japan have strategic aid policy without outside prodding? Exactly what role does strategic aid

play in Japanese policy and what impact is it likely to have in the future? By answering these questions, the author attempts to chart Japanese foreign policy through the 1980s and beyond.

The highlight of the book is chapter 5, entitled "The Limits of Strategic Aid: Globalism versus Regionalism." The chapter offers a unique analysis of eight cases—in Latin America, Africa, the Middle East, the ASEAN nations, China, Pakistan, and Indochina—to illustrate five interrelated objectives: economic well-being, national prestige, domestic support, peace diplomacy, and national security. The author concludes that "strategic aid has survived in a skeptical domestic environment because of conceptual ambiguity. Comprehensive national security, countries bordering areas of conflict, areas which are important to the maintenance of peace and stability of the world, interdependence, humanitarianism—these concepts are strategic aid's relatives, but none can claim parentage" (p. 114). Foreign aid is not entirely a one-way street since the recipient countries themselves contribute significantly in local manpower, materials, and resources on a cooperative basis. In varying degrees, one wonders how much they contribute ideas, raw materials, and manpower to Japan.

The book is well documented with official speeches, official data, and interview records with government officials and the Liberal Democratic Party leaders. It is descriptive as well as quantitative in its analysis. It contains a bibliography of selected books and articles for further references. The book should be highly recommended since it analyzes many valid and important questions about Japan's "manner of giving."

PETER CHENG

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Erratum

Hayward R. Alker, Jr. "The Dialectical Logic of Thucydides' Melian Dialogue" (September 1988, 805–20). On page 811, col. 1, line 40 should read

$$\sim D / (N_1 / \sim S_1 \ \& \ N_2 / \sim S_2) \& (N_1 / \sim S_1 \ \& \ N_2 / \sim S_2).$$

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